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WATER METERS.

COMPARATIVE TESTS OF ACCURACY, DELIVERY, ETC.—DISTINCTIVE FEATURES OF THE WORTHINGTON, KENNEDY, SIEMENS AND HESSE METERS.

By ROSS E. BROWNE, Mem. Tech. Soc.

From Transactions of the Technical Society of the Pacific Coast.

With the purpose of employing a meter for the measurement of water, in connection with some hydraulic experiments conducted at the University of California, the writer instituted a series of tests of a meter invented by Prof. F. G. Hesse. The accuracy, at various rates of delivery, was carefully determined. The results of these experiments, properly tabulated, furnish a correction for the indicated delivery. It is believed that, where the flow is uniform, the possible error in the corrected index reading may be safely placed at ½ per cent.

The meter is of the class known as *velocity* or *inferential* meters. and is so permanent in its sources of error, as to make it peculiarly adapted to the purpose named.

These experiments led to comparative tests of the Worthington meter. Reliable information was also sought concerning other meters in common use. It is thought that sufficient of interest was developed to warrant this paper, particularly as, in the course of this investigation, it was noted: that no great amount of accurate and systematic information upon this subject is to be found in the available files of our engineering and scientific journals; that the several reports consulted, of engineers and superintendents of water works, while well answering their immediate purposes, do not furnish facts in sufficient detail to guide the independent judgment of one not familiar with the construction of the meters in competition; that very few of the circulars of the American manufacturing companies supply such data as will answer for comprehensive comparison.*

It will be attempted to bring forward, as fairly as may be, the distinctive features of two of the principal forms of Piston meter—the Worthington and the Kennedy,—and three forms of velocity meter—the Siemens of English manufacture, the Siemens of German manufacture, and the Hesse meter already mentioned.

The Worthington is widely known in the United States. The Kennedy is one of the most perfect of the meters used in England. The two Siemens meters are probably the most extensively employed in Europe. The Hesse meter has been but recently perfected, and not yet introduced. If undue prominence has been given to the last named meter, the inter-

* The circular of the National Meter Co. of New York is exceptionally complete, though there is missing some of the data necessary to warrant the inclusion of their rotary Crown Meter in the list of meters discussed.

est of the writer in his special investigation must plead as excuse.

It is to be regretted that this list cannot be made more comprehensive by adding an example of the rotary piston form, such as the Crown. It will be understood that, in speaking of the piston meter, special reference is only intended to the Worthington and the Kennedy.

These meters will be considered in the main with reference to their adaptability to house use, or use in the sale by volume of water under pressure.

This being the purpose of a meter, it should fulfill the following conditions:

1st. It should register with a suitable degree of accuracy, the quantity of water delivered at every rate of flow, from that of the maximum capacity of the service pipe, to a rate so small as to discourage theft. The admissible error is variously placed at from 2 to 5 per cent.

2d. This degree of accuracy should be reasonably permanent, *i. e.*, the meter should not be subject to any change, seriously affecting its accuracy, by wear, by slight deposition of sediment, etc. Sudden opening and closing of the house faucets should not induce any considerable error of registry.

3d. The introduction of the meter should not materially affect the delivery of the service pipe; *i. e.*, should cause no serious loss of effective head or pressure.

4th. The price should be small and the necessary repairs inexpensive.

Notwithstanding the demand and the effort made by inventors to meet these desiderata, such marked success has not been attained as to make it a universal custom to sell water by the volume.

In the systems of city supply, there result decided advantages from the employment of the meter. Reckless waste is checked and the consumer is not charged for his neighbors' extravagance. A number of comprehensive articles have been published upon this subject, and a few points only will be reiterated.

It is claimed that nearly one-half of the water consumed in our cities is uselessly wasted. It is doubtful if this lavish consumption is on the whole a sanitary benefit, the waste being in large part the result either of leakage or of willful negligence, and not of a character to effect any proper flushing of closets and sewers. By placing meters near the point where the service pipes enter the premises, and thus making the consumers responsible for such negligence, and for defective plumbing, a wiser use of water is effected. In order to prevent the penurious consumer from pursuing an economy so stringent as to result in certain sanitary evils, it is recommended that a minimum quantity of say 10 or 20 gallons per capita per diem be established, and the consumer charged for this whether he use it or not. The water department of Providence, R. I., makes a minimum charge of $10 per year (equivalent to about 100 gallons per diem) for each meter service. Meters were provided, in Providence, for about one-half the total number of services, and a decided economy effected. The daily consumption at present is about 350 gallons per service, or 25 to 30 per capita—less than one-half the average in American cities.

When the water supply of a city, employing few meters, becomes inadequate to meet the demands of the consumers, two propositions may be entertained: the increasing of the capacity of its water works, and the introduction of meters. It is maintained that in most cases the latter proposition is by far the more economical. As the city grows, it will, from time to time, become necessary to increase the supply; but it is thought much cheaper to keep the meters in repair and maintain water works of double the capacity sufficient without meters.

It seems inevitable that the meter system should rapidly grow in favor with improvement of the present forms of meter. In a few cities their use is already extensive. In London* nearly 40 per cent. of the houses supplied by the various water companies are now provided with meters. In New York and Boston, meters have been introduced into from 5 to 10 per cent. of the services. In Providence, R. I., 50 per cent. In San Francisco 20 per cent. In Oakland 3 or 4 per cent.

The Comparison of Meters.—A just and comprehensive comparison of the merits of competing meters will frequently involve an extended investigation. If, for instance, the extent and effect of wear and rusting are difficult to estimate, prolonged trial may become necessary.

Of the more important considerations

* See *London Engineer* of Aug. 1, 1884. For detailed information concerning the U. S. and Canada, see circular of National Meter Co., N. Y.

in such a comparison, the following are enumerated in an order not pretending to indicate relative importance.

Delivery under various effective heads.

Greatest advisable rate of delivery.

Accuracy of registration at various rates of delivery.

Sensitiveness.

Necessity and difficulty of special adjustment.

Permanence of initial degree of accuracy and sensitiveness.

Liability to obstruction.

Compactness.

Price, both upon the basis of delivery and of greatest advisable effective head.

Expense of repairs, including the consideration of the life of the meter.

Head lost in the meter.—By head lost is understood the difference of the heads in the inlet and the outlet openings. If H is the actual head at the inlet and h that at the outlet, then the head lost is H-h. The rate of delivery will depend upon this difference of heads, and not upon the actual magnitude of H and h.

In each of the meters described, the law of loss of head is, within a practical limit, roughly the same—the resistance being mainly due to impact and fluid friction, and therefore approximately proportional to the square of the rate of delivery.

This is about the same as the law of loss in a pipe. Hence the loss of head in a meter is fittingly indicated by the length of pipe of given diameter which will cause the same loss. Thus 10 feet of ½-inch pipe, or 30 feet of ⅝-inch pipe, will occasion the same loss as the Worthington ⅝-inch or the Hesse ½-inch meter.

Effective head.—By effective head is understood the actual head necessary to force water through the meter at a given rate. This is equal then to the "head lost" plus the velocity head in the outlet pipe, and will, in the meters mentioned, be very little greater than the "head lost."

Delivery of the meter.—It is customary to designate the size of a meter by the diameter of service pipe for which the inlet and outlet openings are fitted. This classification furnishes no general measure of delivery. When the rate of delivery is approximately the same function of the head lost, in each, it is admissible to adopt a unit and classify the meters accordingly.

The loss in the delivery of a service pipe occasioned by the introduction of a Worthington ⅝″ or a Hesse ½″ meter, is easily calculated.

Suppose for example the ⅝-inch service pipe to have a length of 100 feet; also 5 elbows each equivalent, in loss of pressure occasioned, to 5 feet of pipe; also one service cock, together with minor obstructions equivalent to 25 feet of pipe. The equivalent length of pipe, of diameter, $d=\frac{5}{8}$ inch, is $l_1=150$ feet $=1800$ inches. After introduction of meter $l_2=(150+30)\times 12=2{,}160$ inches. Weisbach's formula gives

$$v=\sqrt{\frac{2g\,H}{1+\lambda\frac{l}{d}}}$$

wherein H will be the effective head in the main, v the velocity in the service pipe, and λ a coefficient $=.02$ for such velocities as are here involved. If v_1 represents the velocity before, and v_2 after the introduction of the meter

$$\frac{v^2}{v^1}=\sqrt{\frac{d+\lambda\,l_1}{d+\lambda\,l_2}}=\sqrt{\frac{\frac{5}{8}+.02\times 1{,}800}{\frac{5}{8}+.02\times 2{,}160}}=0.914$$

In other words, the meter would occasion, in the case given, a loss: in delivery of service pipe, of about 9%; in effective head back of the faucet, of about $1.00-(0.914)^2=16\%$; in the kinetic energy or capacity for work, of about $1.00\div(0.914)^3=24\%$.

Greatest Advisable Rate of Delivery.—When a meter is taxed beyond a certain point it will be seriously damaged. A practical limit in rate of delivery is therefore fixed upon, and this should govern the selection. In addition to this limit in rate of delivery, the corresponding effective head should be given. The allowable effective head in the oscillating piston meters is small, in the rotary piston meters considerably greater, in the velocity meters very high. Where a high head is at hand, and the capacity of the service pipe is great, an oscillating piston meter of large size should be used, whereas it is safe to introduce a comparatively small velocity meter.

Sensitiveness.—The rate of delivery necessary to cause motion of the dial hands, or the greatest quantity per minute which may pass without causing registration, will be taken as an inverse measure of the sensitiveness of the meter.

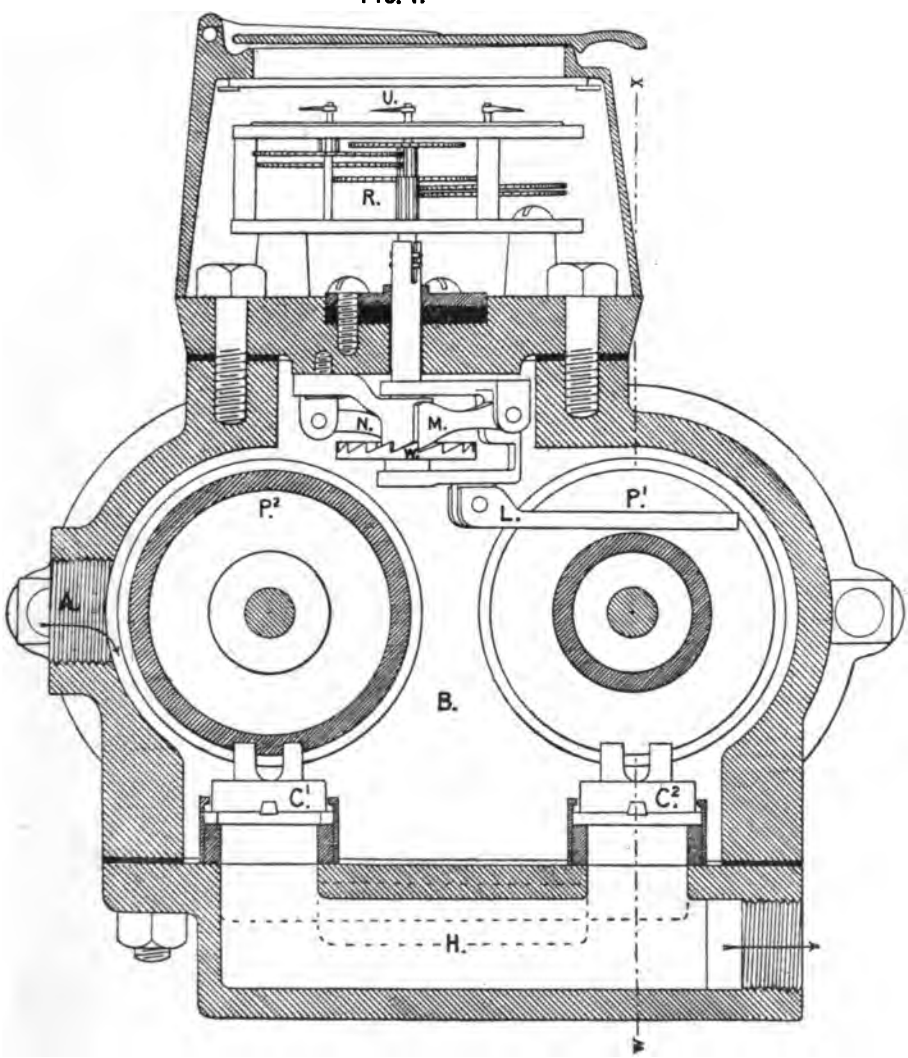
The head lost and accuracy of a meter, are conveniently illustrated by means of curves.

Curves of head lost (see diagram).—These were obtained by plotting on suitable scales, in a rectangular co-ordinate system, the rates of delivery as abscissæ, and the corresponding heads lost as ordinates.

Curves of Registry (see diagrams).—By recording the measurement of the actual quantity delivered, and the reading of the index, under various rates of flow, the data is obtained for plotting a curve which will illustrate the effect of the change in rate of delivery upon the accuracy of registration. The curves given were obtained by plotting the rates of delivery as abscissæ, and the corresponding per cent. registered of the actual quantity delivered as ordinates.

WORTHINGTON METER, ⅝ inch, Scale ¼.

FIG. I.



Upon the basis of the considerations enumerated, a comparison of the meters selected has been instituted. A statement of this comparison will be preceded

by a detailed description of each meter concerned.

Piston Meters.—In the piston meters mentioned, the oscillating pistons displace a quantity of water each stroke. The measure of the quantity delivered, is, in the Worthington the number of strokes, in the Kennedy the approximate distance traveled by the piston. The degree of accuracy for the various rates of

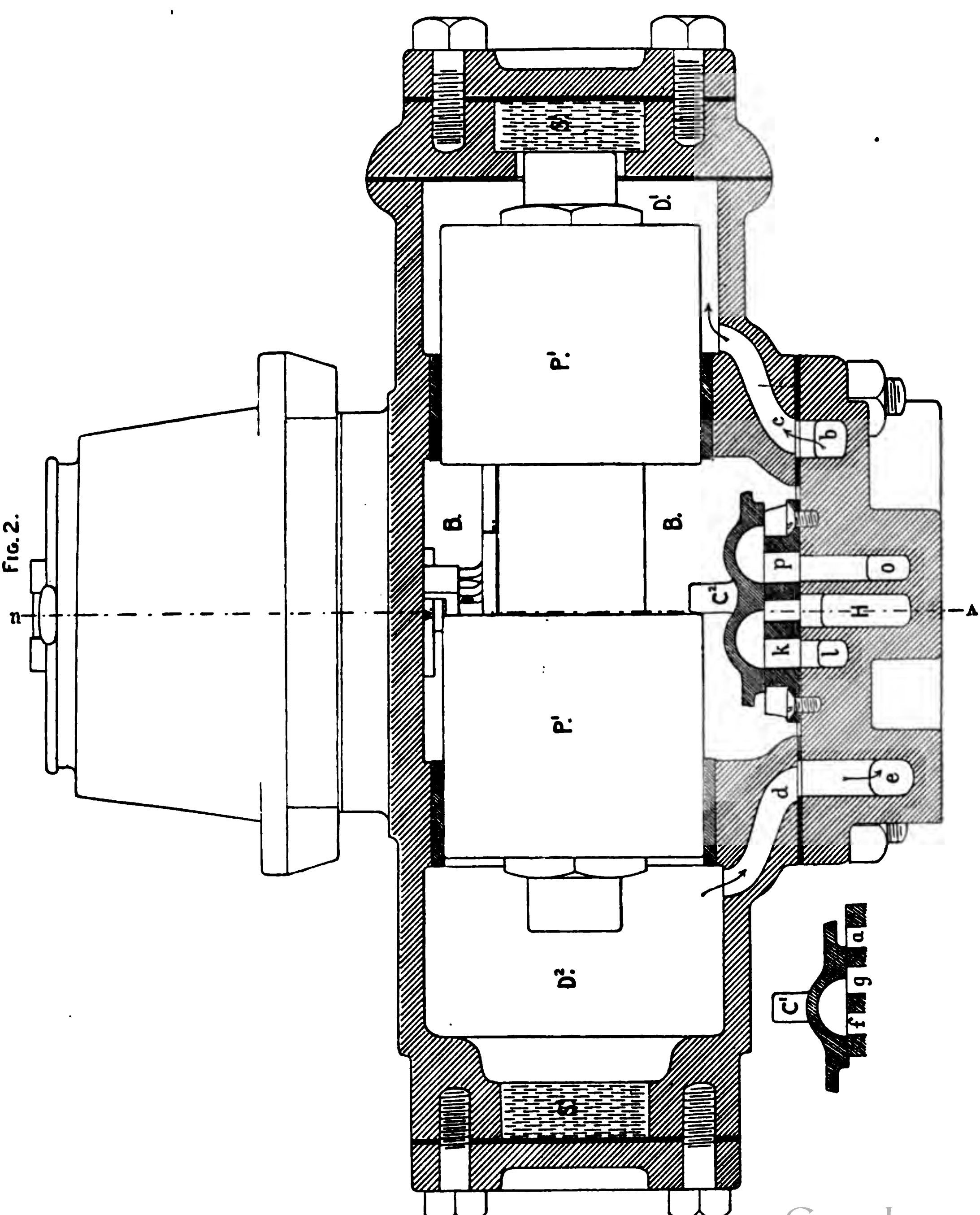
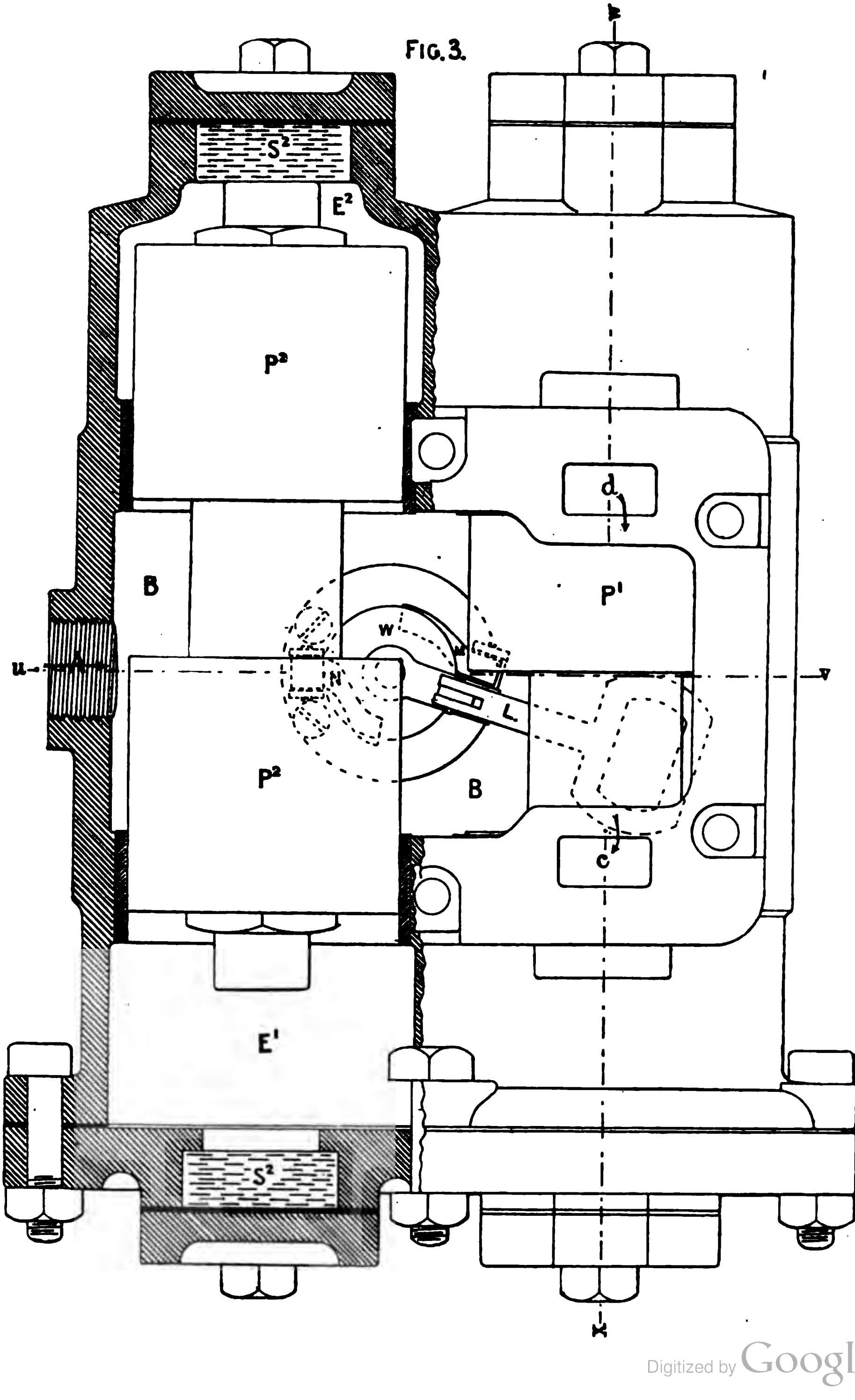


Fig. 2.

delivery, will depend in the Worthington upon the differences in length of stroke, the piston leakage, the action of the valves, &c. In the Kennedy there is a dependency upon, but slight differences in the length of stroke, upon leakage, etc. These

FIG. 3.



and similar meters are frequently termed *volume* or *positive* meters, in contradistinction to *velocity* or *inferential* meters. There is no general difference in the directness of transmission from the measuring part to the index.

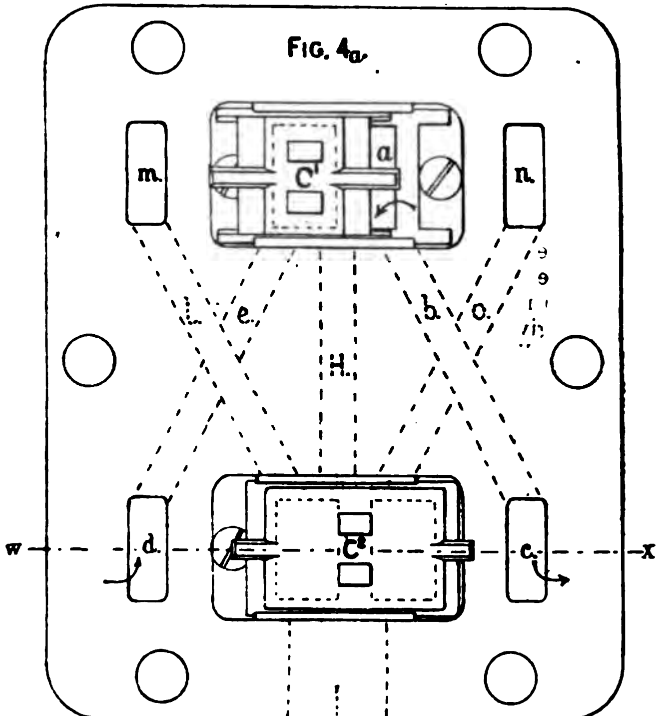


FIG. 4a.

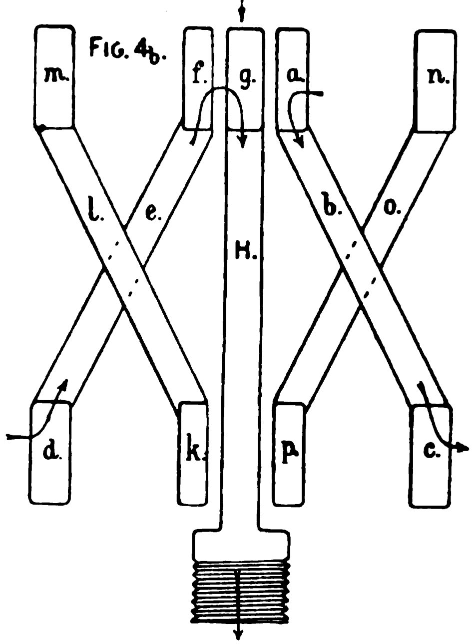


FIG. 4b.

No assumption of advantage is authorized by such a distinction. The approximate volume is registered in either case, and

Description of the Worthington Piston Meter (see Figs. 1, 2, 3, 4_a 4_b)—Letters of reference refer to the same parts in the

different figures. In this meter two piston plungers are closely fitted in parallel cylinders. By means of two slide valves, the water is admitted under pressure into the chamber at one end of each plunger alternatively, while connection is made between the chamber at the other end and the discharge pipe. Thus the piston in moving, displaces the volume escaping through the discharge pipe. The arrangement is such that the strokes of the two plungers alternate, the valve actuated by the one admitting the pressure to the other. The displacement, (area of piston times length of stroke) multiplied by the number of strokes will give approximately the volume of water delivered. The indexing apparatus is arranged to move the dial hands once every fourth stroke, 3 such movements registering $\frac{1}{10}$ cubic foot in the $\frac{5}{8}$-inch meter.

The water enters through opening A into chamber B. In the position of plungers shown, the water then passes through port *a* (of valve C^1), channels *b* and *c* into chamber D^1. Plunger P^1 is moved to the left, forcing the water of chamber D^2 through channels *d*, *e*, ports *f* and *g* into outlet H. In the last third of the stroke, valve C^2 is shifted to the left, establishing communication between chambers B and E^2, through port *k* and channels *l* and *m*—and at the same time connecting chamber E^1 with outlet H through channels *n* and *o* and port *p*. Plunger P^2 is moved to the right, shifting, in the last third of its stroke, valve C^1, and thus establishing communication between chambers B and D^2 through port *f* and channels *e* and *d*.

The pistons are brought to rest at the end of the stroke by rubber seatings S^1, S^2.

Piston P_1 imparts a reciprocating motion to lever L, which, in combination with the movable pawl M, ratchet-wheel W, stationary pawl N, and index gear R, causes the dial hand U to register for each four plunger strokes (single strokes) $\frac{1}{30}$ of a cubic foot,

Fig. 1 is a section through *uv* (see Figs. 2 and 3).

Fig. 2 is a section through *wx* (see Figs. 1, 2 and 4_a).

Fig. 3 is a view from below with base plate (Fig. 4_a) removed, showing the walls of chamber R partly in section.

Fig. 4_a is a top view of the base plate, showing valves, ports, etc.

Fig. 4_b is a plan of the valve ports, channels and outlet, showing how channel *o* passes under channel *b*, channel *e* under channel *l*, etc.

*Description of the Kennedy Piston Water Meters** (see Figs. 5, 6, 7).—The measuring cylinder (A) forms the base of the meter, and is fitted with a piston (B) made of vulcanite. The piston is made to move perfectly water-tight and almost free from friction, by means of a solid cylindrical ring (C) of pure "Para" rubber, which rolls between the body of the piston and the internal surface of the cylinder. Each end of the cylinder is fitted with an india rubber seating (D), on which the piston will form a water-tight joint, if back pressure should force it to either end of the cylinder; undue pressure is thus prevented from being thrown on the piston roller.

The piston rod (E), after passing through a stuffing box (F) in the cylinder cover (G), is attached to a rack (H) which gears into a pinion (K) fixed on the shaft (L). The shaft is turned in reverse directions, actuating the reversing and indexing gear (M) as the piston moves up and down. The rack is kept in gear and guided in a vertical line by an anti-friction roller, which is carried on a stud projecting from one of the shaft-bearing brackets. The cock-key (P), which directs the water alternately above and below the piston, is placed in the same axial line as the shaft, and is fitted with a duplex lever (Q), which is actuated by a weighted lever (R) carried loosely on the shaft, and caused to fall alternately on each arm of the duplex lever. The weighted lever, after reversing the key, falls on a buffer (S) faced with india rubber, which, yielding before it and traveling in the same curve, gradually brings it to rest.

Fig. 5 is a side section through the center shaft, cock-key, and piston.

Fig. 6 is a front section of cock-key (P) and water passages (U, inlet, and V outlet).

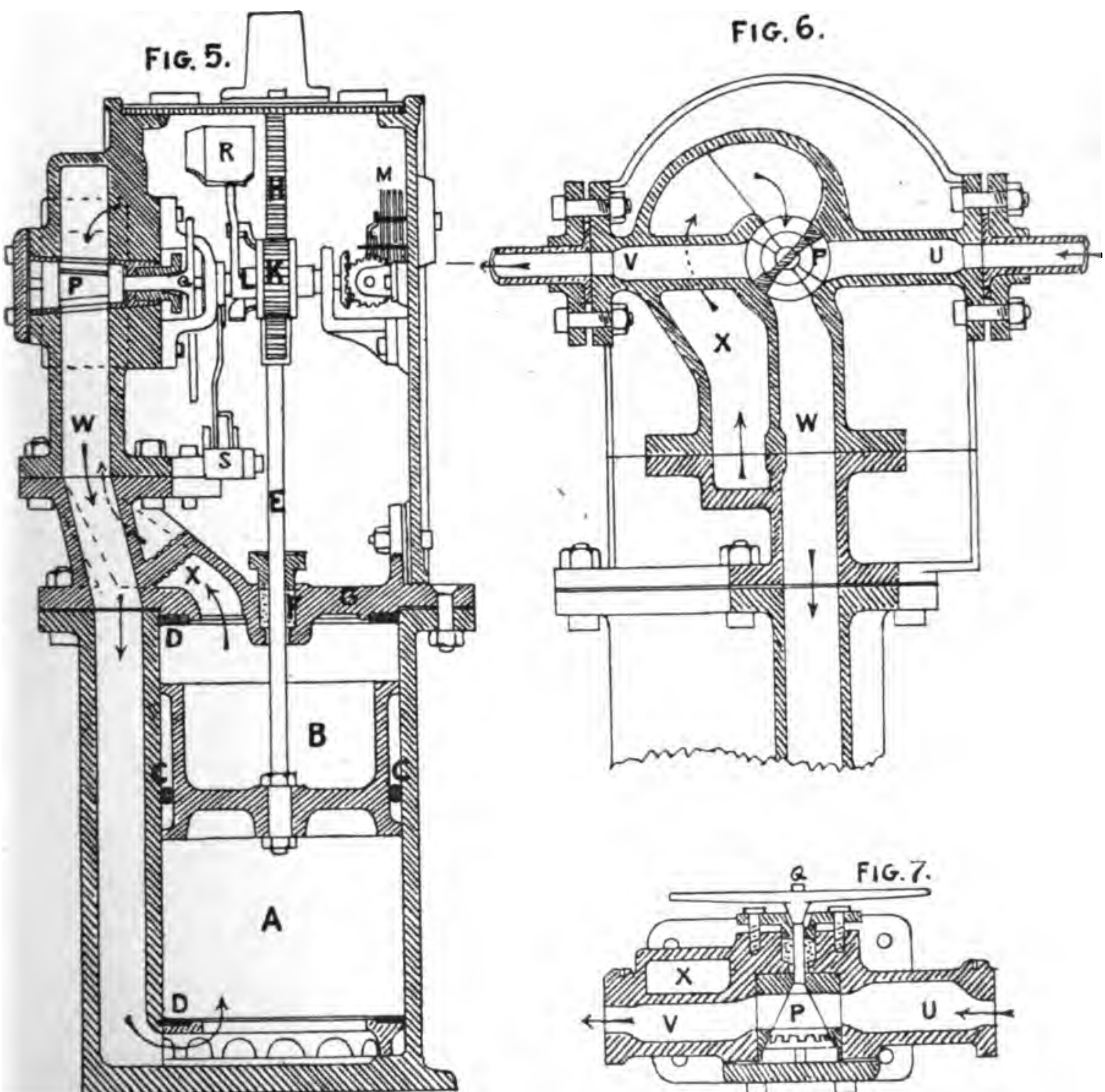
Fig. 7 is a horizontal section through line UV.

The meter is shown in the position of having nearly completed its upward

* Description and figures are taken with but slight changes from the circular of the Kennedy Patent W. M. Co., Kilmarnock, Scotland.

stroke. The water enters at the inlet (U), and is directed by the cock-key down the passage (W) to the bottom of the cylinder, forcing up the piston, which presses the water (which on the previous down-stroke entered above the piston,) up through the passage (X), passing behind passage (V), and is directed by the cock-key into the outlet passage (V). When the piston has moved up a little farther, the bob (weighted lever R) will pass its point of unstable equilibrium and fall on the key arm (arm of duplex lever Q) which it will send down until it is stopped by the buffer box (S). The key will then be at right angles to its position as shown in Fig. 6. The water will then be directed from U down X into the top of the cylinder, forcing the piston down, while the water admitted below during the last stroke is forced up the passage W and out by the outlet V. When the piston has arrived

KENNEDY METER.



near the bottom of the cylinder, the lifter will have lifted the bob from the left side of the buffer-box and raised it to the point of unstable equilibrium; from there it will have fallen on the right hand key-arm, and have brought back the cock-key to its former position, ready to begin another upward stroke.

It is unnecessary to illustrate here the method of converting the reciprocating motion of the shaft (L) into the circular motion (in one direction) of the index wheels (M), and thus causing to be registered a quantity approximately proportional to the distance traveled by the piston.*

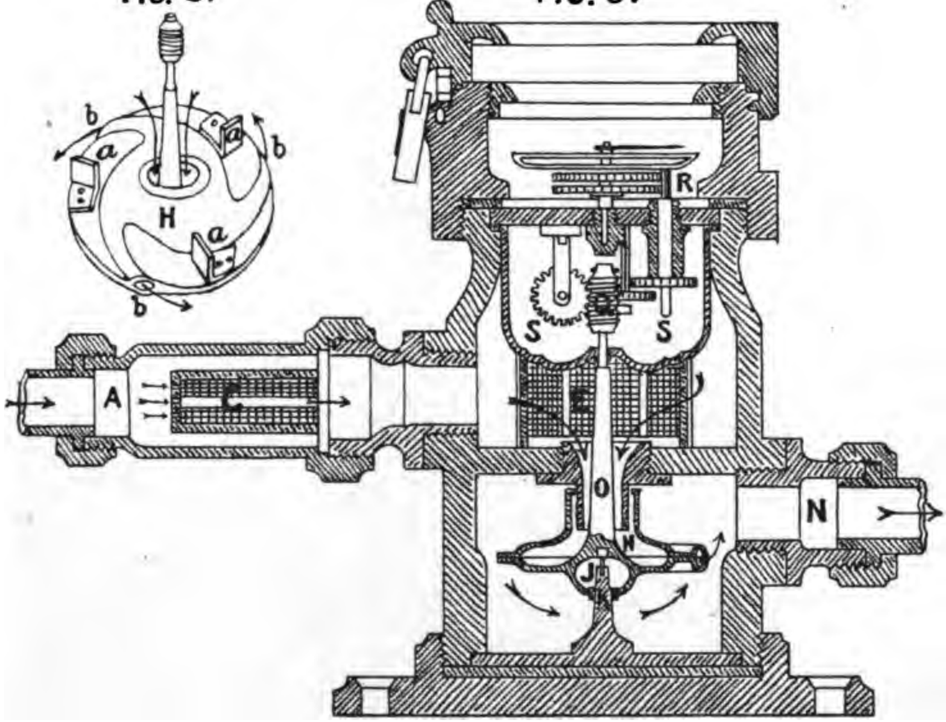
VELOCITY METERS.—In the velocity meters of the Siemens system a small wheel is driven by the passing water, and the number of revolutions is the measure of the quantity delivered. The frictional resistance, offered by the journals of the wheel spindle and the registering apparatus, cause important modifications of the velocity of the wheel. The Hesse meter practically overcomes the objectionable influence of this resistance, but it will be shown that, even if it could be wholly avoided, perfect accuracy of registration would not thereby be effected.

*Description of the "English" Siemens Velocity Water Meter** (see Figs. 8, 9).—Of the Siemens system there are two important forms, the one manufactured by Guest & Chrimes of Rotherham, England, and the other by Siemens & Halske of Berlin. The former will be termed the "English" Siemens, and the latter the "German" Siemens.

It will be seen from the figure of the English Siemens meter, that the water passes, as indicated by the arrows, from the inlet pipe through a funnel, into a

ENGLISH SIEMENS METER

FIG. 8. FIG. 9.



* A ratchet is interposed between the pinion and the registering gear, and the degree of approximation in indicating the "length of piston travel" depends upon the number of teeth in the ratchet.

* Description, in the main, and figures taken from the circular of Guest & Chrimes.

small reaction wheel, or Barker's mill (H), (constituting the measuring drum,) causing it to revolve. The water then passes on to the outlet pipe. The motion imparted to the measuring drum is communicated to the index, and thus a quantity proportional to the number of revolutions of the drum, registered.

Fig. 8 is a perspective view of the drum or measuring medium (H), showing the adjusting or regulating vanes *a a a*, and curved water ways *b b b*.

for the purpose of lubricating and protecting the dial wheels from the action of the water, etc.

*Description of the "German" Siemens Velocity Water Meter** (see Figs. 10, 11).—This meter differs from the "English" Siemens in so far as the motor is concerned, a small pressure wheel taking the place of the reaction wheel.

The water having entered the meter through the inlet pipe U, passes through the openings *a a a a*, in the brass casing

GERMAN SIEMENS METER

FIG. 10.

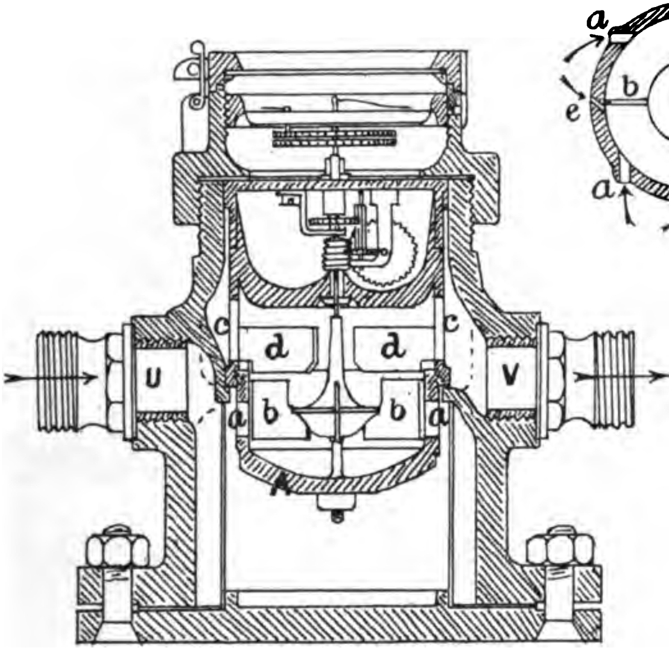


FIG. 11.

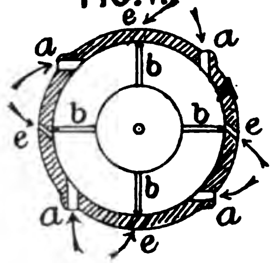


Fig. 9 is a section of the meter, filter and unions complete. A is the inlet. C and E are filters for the purpose of preventing foreign substances from passing into the drum H. The motion of this drum is retarded and suitably regulated by the vanes *a a a*. J is an oil-box for the purpose of lubricating the spindle K. N is the outlet. O is the spindle of the drum, with screw Q attached for the purpose of giving motion to the wheels of the dial work R. S S is an oil chamber

A, and coming in contact with the buckets *b b b b*, imparts motion to the wheel. The water then escapes through openings *c c*, and finally discharges into the outlet pipe V.

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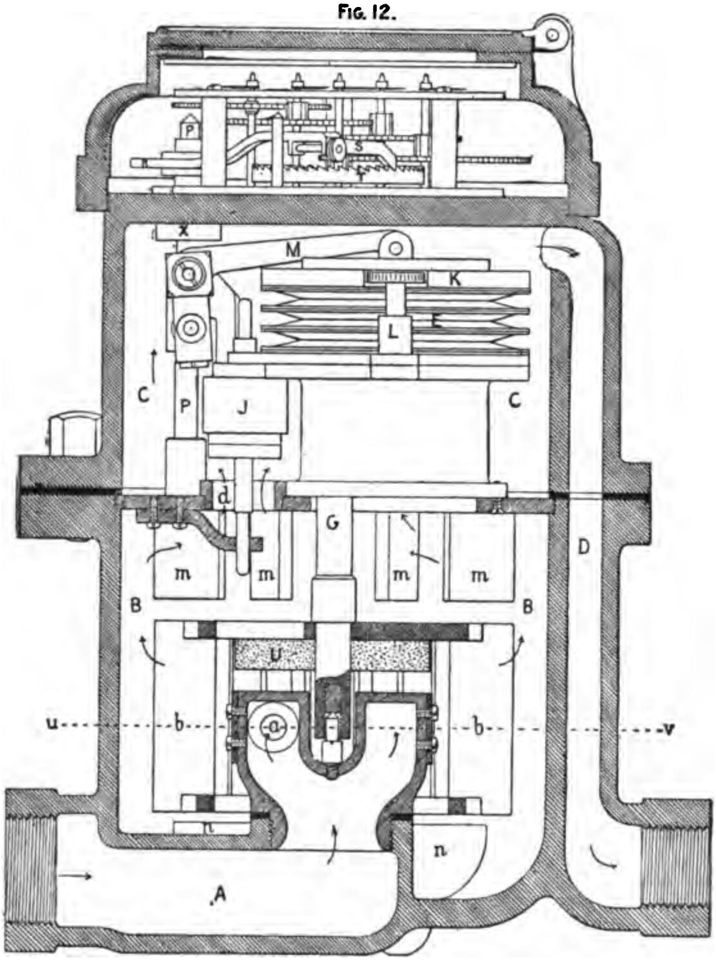
* The figures are taken from a valuable article, on the subject of water meters, in the German "*Civilingenieur*" of the year 1875, by B. Salbach, Kgl. Baurath at Dresden. At the instigation of the City Council of Dresden, an exhaustive set of tests were made of twelve "meters of the most modern construction"—among these the Kennedy, the "English" and the "German" Siemens. Further reference will be made to this article.

HESSE METER

½ INCH.

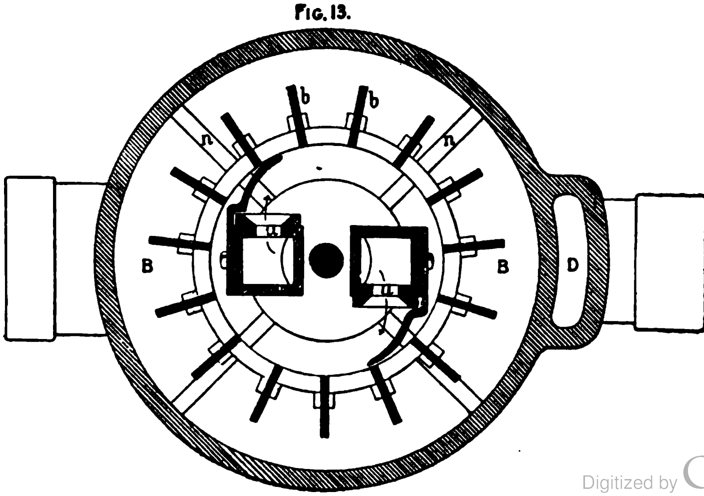
FIG. 12.



CAST IRON. BRASS. HARD RUBBER. SOFT RUBBER.

SCALE ½.

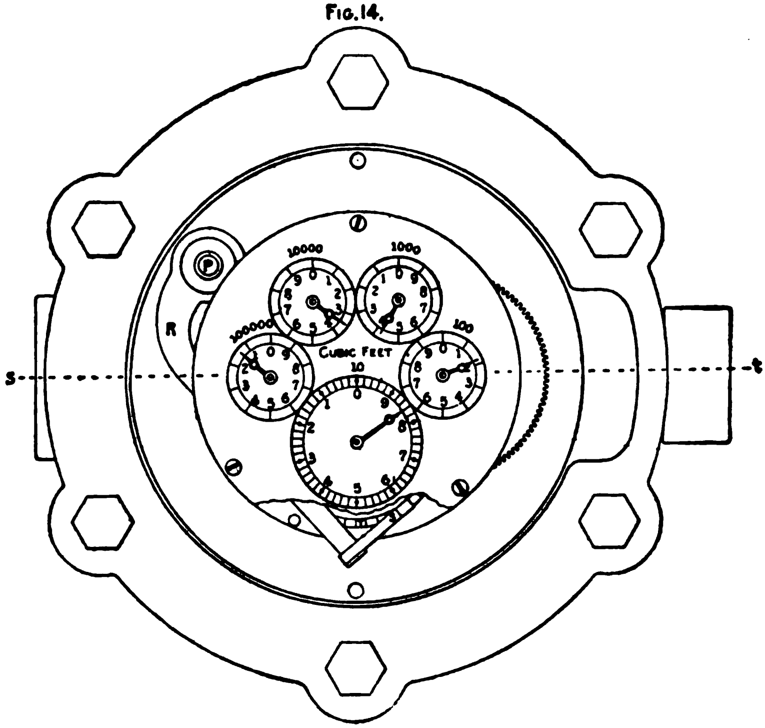
FIG. 13.



The manner in which the motion is imparted to the wheel is transmitted to the index, is the same as in the "English" Siemens. In order to keep out the coarse sediment, the water is made to pass through a screen before entering the meter. For the purpose of regulating the velocity of the wheel, four small openings *eeee* are bored into casing A, in a direction opposed to the motion of the wheel; and by closing or enlarging these, the velocity of the wheel may be increased or reduced. conical tube *h* (Figs. 16, 17), and is finally discharged through chamber D into the outlet pipe.

The motion of the measuring wheel is transmitted to the worm wheel H (Figs. 16, 17), by means of the endless screw F (Fig. 16), which is attached to the upper end of the wheel spindle G (Figs. 12, 16). This worm wheel, in revolving, interrupts intermittently the direct flow of that portion of the water which passes through chamber *g*. In the position of the worm

FIG. 14.



The stationary ribs *d d* counteract in part the tendency of the water to rotate.

Description of the Hesse Velocity Water Meter (see Figs. 12, 13, 14, 15, 16, 17, 18).—The water from the inlet pipe enters channel A (Fig. 12), passes through openings *a a* (Figs. 12, 13), and striking the buckets *b b* (Figs. 12, 13), imparts motion to the measuring wheel—a pressure wheel similar in character to that of the "German" Siemens. The water passes then from the wheel chamber B into chamber C, mainly through valve opening *d* (Fig. 12), and in part through chamber *e*, opening *f*, chamber *g*, and wheel shown in (Fig. 16), the solid arm of the wheel (the cause of this interruption) is just passing the openings *f* and *h*, hence direct flow through these openings is just beginning. During the interval of direct flow the action is comparable to that of the jet pump, causing the pressure in chamber *g* to fall below that in chamber C. The interruption of the direct flow causes the pressure to rise above that in chamber C.

The alternating high and low pressures in chamber *g*, thus induced by the slow rotation of the worm wheel, cause the cap K of the bellows-like rubber dia-

phragm E (Figs. 12, 16, 17) to rise and fall accordingly. The number of such pulsations or strokes is directly proportional to the number of rotations of the measuring wheel. The length of the stroke is limited by the heads of set screws LL (Figs. 12, 16, 17).

This reciprocating motion of the cap K is transmitted and converted into the circular motion of the gear wheels of the index, through lever M, rock-shaft N, shaft P, lever R, pawl S, and ratchet wheel T (Figs. 12, 14, 15).

The dial hands are thus made to register a quantity, which is directly proportional to the number of revolutions of the measuring wheel. The measuring wheel is made of hard rubber of specific gravity 1.2, and with the cork attachment (U*) has the same weight as an equal volume of water. There is then so little pressure upon the journals of the spindle G, that the frictional resistance offered is exceedingly small. The worm wheel H rests loosely in its bearings, offering but little resistance to rotation. Since in 38 revolutions of the measuring wheel, but one revolution is given to the worm wheel, the work and hence the effective resistance is minute. The frictional resistance due to the periodic pressure upon the journals of the worm wheel, as the arm passes opening *f*, was found by experiment to be very small.

When the meter is delivering water at rates above three or four gallons per minute, the valve J is lifted, but the difference of pressures in chambers B and C is far greater than that needed to operate the bellows. When the rate of delivery is small, the valve is seated, and the entire quantity is forced through

FIG. 15.

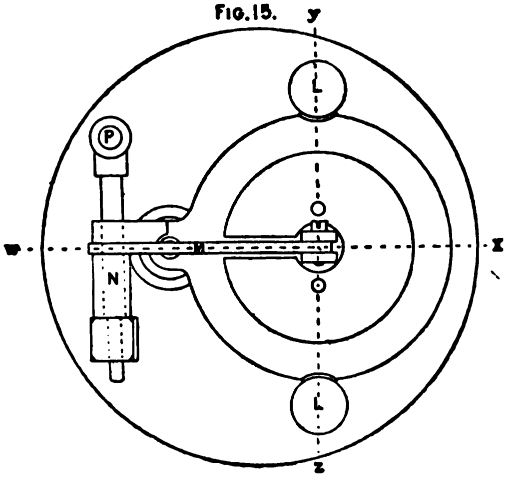


FIG. 16.

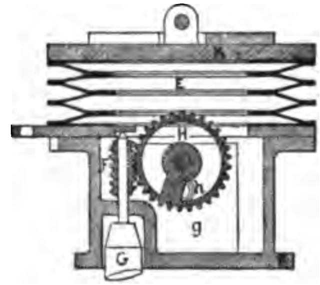


FIG. 17.

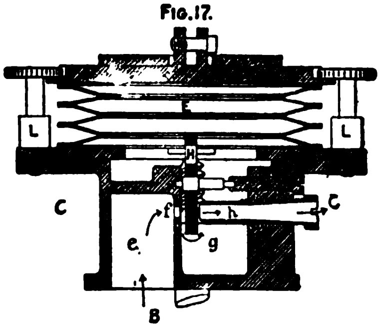
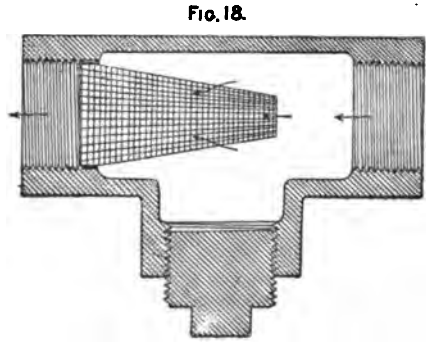


FIG. 18.



* The cork attachment is unnecessary, as the weight of the wheel in water is sufficiently small without it.

chamber g, insuring the action of the bellows.

The alternate rise and fall of pressure in chamber g, furnish an abundant surplus of power to provide against undue resistance in the stuffing box X (Fig. 12), and all differences due to the character of the workmanship, and to wear or corrosion of the parts of the registering apparatus.

The measuring wheel does not supply the power expended in moving the registering apparatus, hence the unavoidable changes in the latter do not affect the accuracy of the meter. This points to the distinctive feature of the Hesse meter.

m, *m*, and *n*, *n* (Figs. 12, 13) are stationary ribs provided for the purpose of checking in part the rotation of the water in chamber B, and offering additional resistance to the motion of the wheel. Such resistance diminishes the detrimental influence of the solid friction, and causes almost immediate stoppage of the wheel in case the water is suddenly shut off.

The meter tested is provided with connections for ½ inch pipe. Openings *aa* have $\frac{9}{32}$ inch diameter. Valve opening *d* has ⅝ inch diameter. Weight of valve J is 0.16 lb. Entire weight of meter is 21.4 lbs.

Fig. 12 is a vertical section through *st* (see Fig. 14).

Fig. 13 is a horizontal section through *uv* (see Fig. 12).

Fig. 14 is a top view of the meter with the dial box removed, showing the dials, etc.

Fig. 15 is a top view of that portion of the registering apparatus contained in chamber C (see Fig. 12).

Fig. 16 is a vertical section of the bellows-like diaphragm, etc., taken through *wx* (see Fig. 15).

Fig. 17 is another vertical section of the same through *yz* (see Fig. 15).

Fig. 18 is a section of the screen or filter which is interposed between the meter and the inlet pipe.

The material used in constructing the meter is shown by the manner of shading the sections. It may be found advantageous to make a different selection for some of the parts. The meter will not serve for the delivery of hot water without radical changes in the material.

The Kennedy and the Siemens Curves (see diagrams) were obtained from the experiments of Mr. Salbach (see foot note), who used such curves as a means of comparison.

The volume of water delivered was measured in an accurately constructed tank. The loss of head was given by the difference of the readings of two large quicksilver manometers, the one communicating with the inlet, and the other with the outlet pipe immediately adjacent to the meter.

Tests were made under mean heads* of about 45 and 150 feet. The change of mean head had no material effect upon the curves of registry of these meters, excepting in the case of the "German" Siemens meter II, which gave better results under the greater head, and even this was doubtless due to cleaning of the meter in the interval. The effect upon the curves of head lost was not important, and was probably due, as Mr. Salbach says, (mainly) to some imperfections in the manometers when under low pressure, The curves of registry were plotted from the tests made under the lower mean head, the curve from tests under the higher mean head being added for the "German" Siemens meter II—II_a lower, II_b higher mean head. The curves of head lost were plotted from the tests under the higher mean head.

The Worthington B, C *and* D curves of registry were plotted from the results of experiments conducted by Prof. Hesse, as chairman of a committee appointed by the Board of Managers the Twelfth Industrial Exhibition of the Mechanics' Institute 1878 (published in report). Meter B had never been in use, C had been in use 3 months, D 8 months. A number of others were tested showing the effect of wear, etc.

The Hesse and the Worthington A curves were plotted from the results of a set of tests conducted by the writer, with the assistance of Mr. H. Dikeman, a student of engineering in the University of California.

The Hesse meter tested, forms a part of some experimental apparatus in the Mechanical Laboratory.

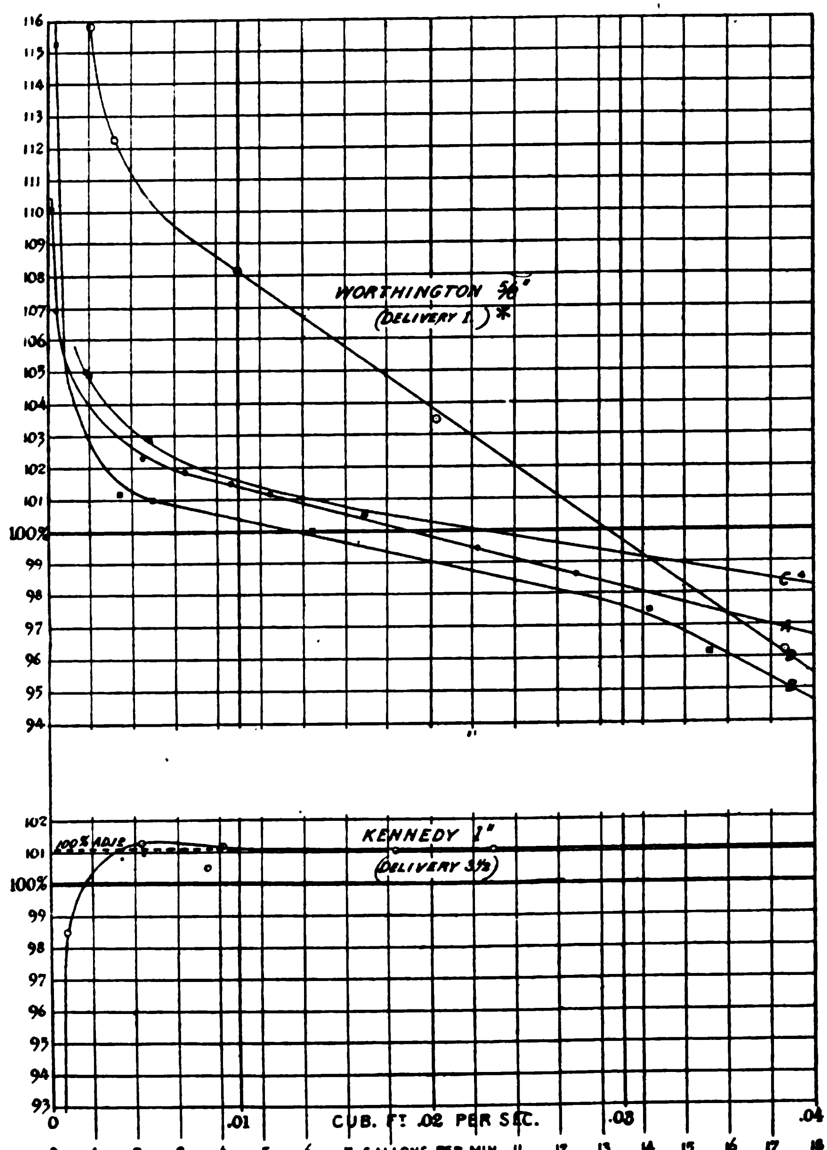
The Worthington A is a ⅝-inch meter which was specially selected by Mr. Pur-

* It is presumed that by "mean head" is meant the average head in the inlet pipe.

cell of the Oakland Water Co. to serve as a test meter.

The quantity of water delivered was weighed to $\frac{1}{10}$ lb., and the pressure measured by means of a sensitive gauge described in the *Mining and Scientific Press* of September 2, 1882, also in *Bulletin No. 1* of the College of Mechanics. With this gauge the heads could easily be measured to $\frac{1}{10}$ of a foot. The time was observed only to single seconds. The results of these tests are tabulated below. The diameters of the inlet and outlet pipes were the same at the points where connected with the pressure gauge, so that it was unnecessary to make allowance for the velocity heads at these points in order to obtain the effective difference of heads. The temperature of the water varied 2° F., averaging 55° F.

CURVES OF REGISTRY

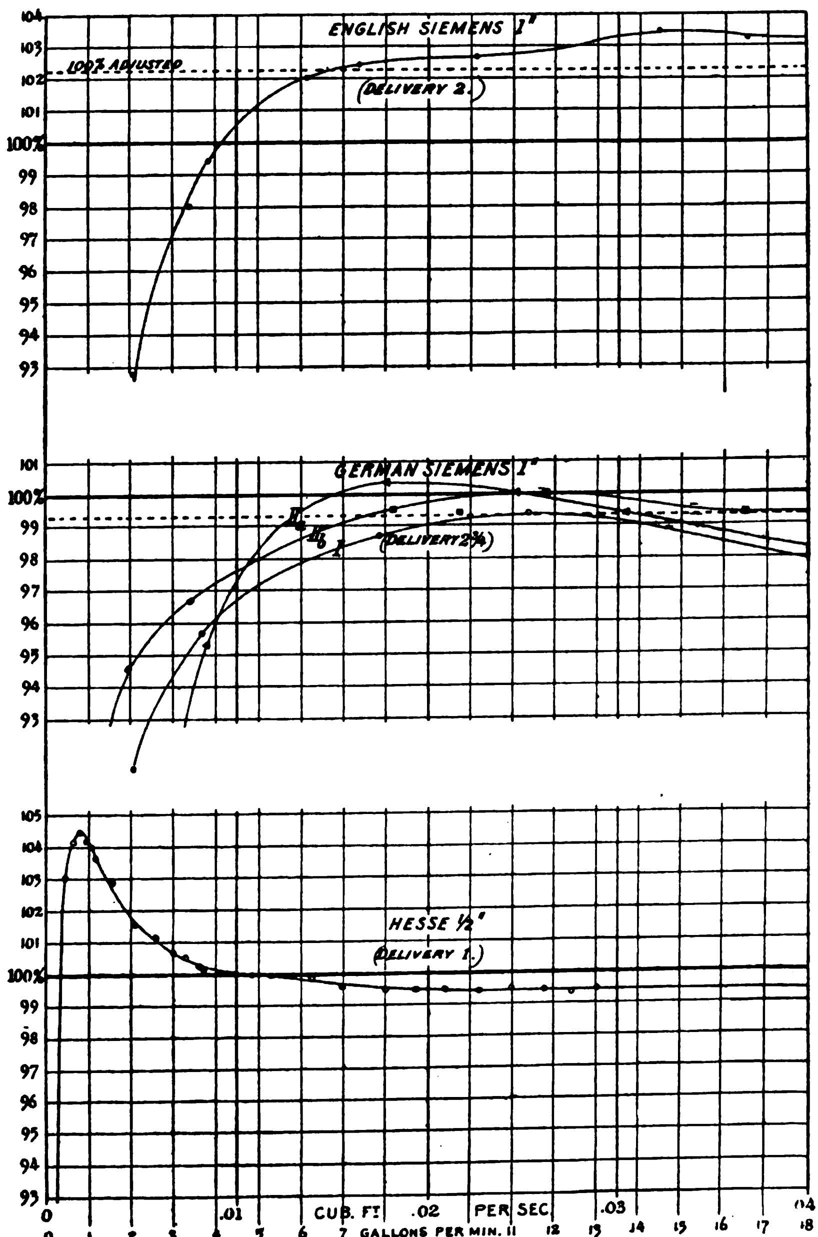


* UNIT OF DELIVERY 9 GALLONS PER MIN. WITH 20 FEET LOSS OF HEAD.

The possible error in these tests, it is thought, may be safely taken at 1 per cent. in the rate of delivery, at $\frac{1}{4}$ per cent. in the ratio of registered to actual delivery, and at one foot in head lost at high rates, and $\frac{8}{10}$ foot at low rates.

The Hesse meter, experimented with, had previously been subjected to various tests by the Oakland Water Co., and had delivered during four months, 62,000 cubic feet (in great part at the rate of $12\frac{1}{2}$ gallons per minute), without being in the least damaged. This is a greater quantity than would be drawn in 5 years in the average service. However, it must be borne in mind that this test involved the important element of time only to a small extent.

CURVES OF REGISTRY



Tests of the Accuracy and Delivery of the Hesse One-Half Inch Meter.

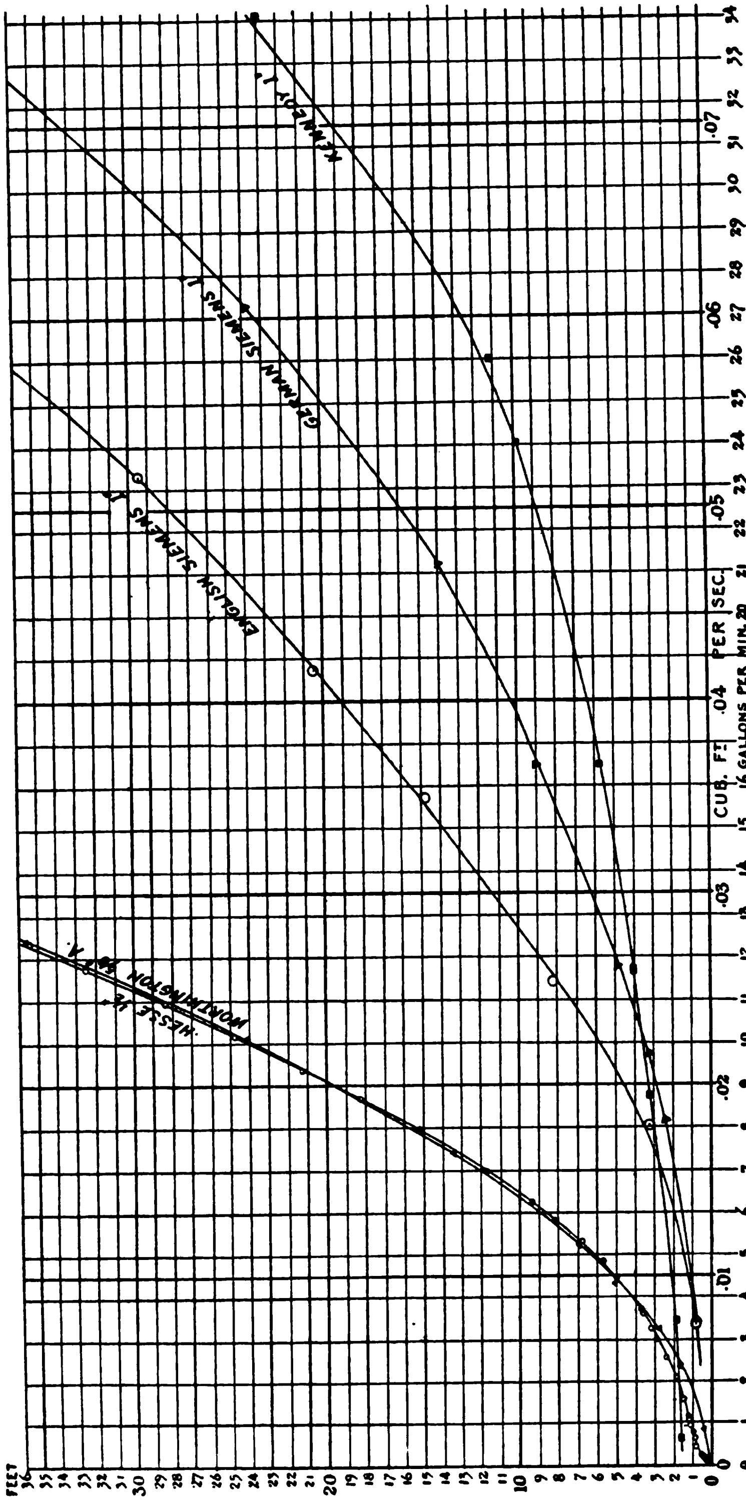
No. of Test.	Hydraulic Heads in Feet. Inlet H.	Hydraulic Heads in Feet. Outlet h.	Time in Sec's.	Weight in lbs. avoirdupois G.	Cubic Feet. Actual $\frac{G}{62.4}$	Cubic Feet. Registered.	Rate of Delivery. Cubic ft. per sec.	Rate of Delivery. Gals. per min.	Head lost in Feet, H-h.	Ratio of Regist. to Actual Volume in per cent.
1	—	—	—	—	—	—	.00015	.07	—	—
2	59.85	59.40	1308	31.3	0.502	0.4	.00038	.17	.45	79.7
3	59.50	59.00	2543	72.2	1.157	1.0	.00046	.21	.50	86.4
4	59.00	58.15	3660	218.0	3.494	3.6	.00096	.43	.85	103.0
5	59 40	58.55	1392	119 8	1.920	2.0	.00138	.62	.85	104.2
6	50.40	58.45	1102	119.5	1.915	2.0	.00174	.78	.95	104.4
7	50.40	58.30	915	119.8	1.920	2.0	.00210	.94	1.10	104.2
8	59.20	58.05	802	120.0	1.923	2.0	.00240	1.08	1.15	104.0
9	59.10	57.90	1125	180.6	2.894	3.0	.00257	1.15	1.20	103.7
10	59.0	57.5	840	182.0	2.917	3.0	.00347	1.56	1.5	102.9
11	58.8	56.9	641	184.3	2.954	3.0	.00461	2.07	1.9	101.6
12	57.1	54.7	524	185.0	2.965	3.0	.00566	2.54	2.4	101.1
13	56.8	53.9	452	186.0	2.981	3.0	.00660	2.96	2 9	100.6
14	56.5	53.3	441	198.6	3.183	3 2	.00722	3.24	3.2	100.5
15	56.3	52.7	375	186.8	2.994	3.0	.00798	3.58	3.6	100.2
16	55.8	52.1	365	187.0	2.997	3.0	.00821	3.69	3.7	100.1
17	55.9	50.9	304	187.3	3.002	3.0	.00987	4.43	5.0	100.0
18	55.6	49 9	563	374.6	6.003	6.0	.01066	4.79	5.7	100.0
19	55.2	48.4	507	374.7	6 005	6.0	.01184	5.32	6.8	99.9
20	54.7	46.5	465	374.7	6.005	6.0	.01291	5.80	8.2	99.9
21	54.3	44.9	432	375.0	6 010	6.0	.01391	6.24	9 4	99.8
22	55.0	45.8	434	375.0	6.010	6.0	.01386	6.22	9.2	99.8
23	54.5	42.8	390	376.0	6.026	6.0	.01545	6.94	11.7	99.6
24	53.5	38.3	340	376.4	6.032	6.0	.01774	7.97	15.2	99 5
25	52.8	34 5	312	376.5	6.034	6 0	.01934	8 68	18.3	99.4
26	50.7	29.4	290	376.5	6.034	6.0	.02081	9.34	21.3	99.4
27	49.7	24.9	267	376.7	6.037	6.0	.02261	10.15	24.8	99.4
28	49.3	20.9	256	388.8	6.232	6.2	.02434	10.93	28.4	99.5
29	47.6	15.1	231	376.7	6.037	6.0	.02613	11.73	32.5	99.4
30	46 5	11.1	220	377.0	6.042	6 0	.02746	12.33	35 4	99.3
31	45.7	6.8	210	376.7	6.037	6.0	.02875	12.91	38.9	99.4

In test No. 1 the limit was reached.

Tests of the Accuracy and Delivery of the Worthington Five-Eighths Inch Meter.
(A) No. 26,737.

No. of Test.	Hydraulic Heads in Feet. Inlet H.	Hydraulic Heads in Feet. Outlet h	Time in Sec's.	Weight in lbs. avoirdupois G.	Cubic Feet. Actual $\frac{G}{62.4}$	Cubic Feet. Registered.	Rate of Delivery. Cubic ft. per sec.	Rate of Delivery. Gals. per min.	Head lost in Feet, H-h.	Ratio of Regist. to Actual Volume in per cent.
1	—	—	—	—	—	—	.00015	.07	—	—
2	59.00	58.85	1622	17.0	.2724	.30	.00017	.08	.15	110.1
3	59.00	58.80	640	15.5	.2484	.27	.00039	.18	.20	107.0
4	58.85	58.40	492	59.4	.9519	1.00	00193	.87	.45	105.1
5	58.65	57.00	379	122.0	1.9551	2.00	.00516	2.32	1.65	102.3
6	58.15	55.40	272	122.5	1 9631	2.00	.00722	3 24	2.75	101.9
7	57.30	52 20	308	184.4	2.9551	3.00	.00960	4.31	5.10	101.5
8	56.5	49.5	255	185.0	2.9647	3.00	.01163	5.22	7.00	101.2
9	53.8	40.4	302	310 7	4.9791	5.00	01649	7.40	13.40	100.4
10	49.1	24.9	224	313.8	5.0288	5.00	.02245	10.08	24.20	99.4
11	43.7	8.1	184	316.3	5.0689	5.00	.02755	12.37	35.60	98.6

CURVES OF HEAD LOST



DISCUSSION OF THE CURVES OF HEAD LOST. —These are approximately parabolic, and may be represented roughly by the equation $H-h=A+BQ^2$, wherein Q is the quantity delivered in cubic feet per second, A and B constants. In the Worthington ⅝-inch A=0.4, B=46,000.

These curves simply serve the purpose of determining the deliveries of the several meters, prior to making comparisons of sensitiveness, cost, &c. By following the 20 feet line it will be seen that with this loss of head the Worthington ⅝-inch delivers 9.1 gallons per minute, the Hesse ½-inch 9.1; the English Siemens 1 inch 18.5; the German Siemens 1 inch 25.1; the Kennedy 1 inch 32.0. If then the delivery of the Worthington ⅝ inch, under effective head of twenty feet,* is adopted as unit, we have: Worthington ⅝″....1, Hesse ½″ ...1. English Siemens 1″....2, German Siemens 1″....2¾, Kennedy 1″....3½.

It would have been of great advantage in making the comparisons which follow, could the sizes of the several meters have been so selected as to make the deliveries the same under the same effective head.

DISCUSSION OF THE CURVES OF REGISTRY. —The notable properties of these curves are to be found in the degree of approximation to parallelism of the horizontal sweep to the axis of abscissæ, and in the proximity of the vertical sweep to the axis of ordinates. The former furnishes the true criterion for estimating the degree of accuracy which may be reached by adjustment, the latter indicates the degree of sensitiveness of the meter.

These curves may be shifted up or down, or the curves remaining stationary, the 100% lines may be so shifted by a simple adjustment of the meter. This may be effected, in each of the meters, by changing the number of teeth in one or more of the gear wheels leading to the index, since this number determines the ratio of the movements of the dial hands to the movements of the pistons in the piston meters, and of the measuring wheels in the velocity meters.

Such a change may be effected also, though to a smaller extent, as follows:

In the Worthington, by adjustment of the length of stroke.

* The velocity head in the ⅝ inch outlet is small, and is therefore neglected.

In the English Siemens, by change of the regulating vanes *a a a*.

In the German Siemens, by increase of the openings *a a a a* or *e e e e*, or by change of the buckets.

In the Hesse, by change of the number of teeth in worm wheel H, or by increase in size of openings *a a*, or by change of the buckets or stationary ribs.

An inspection of the curves will bring out some marked features.

The *Worthington curve*, for quantities above three gallons per minute, becomes practically a right line, strongly inclined to the axis of abscissæ, showing at low rates too great, and at high rates too small a registry. This is doubtless due, in the main, to the differences in length of stroke. This stroke is on the average a little above two inches. A ⅝ inch rubber seating is compressed at each end of the stroke by an amount, increasing with the momentum or with the velocity of the piston, hence with the rate of delivery. If the difference in the magnitude of this compression, between rates of 3 and 15 gallons per minute, is $\frac{1}{16}$ inch each seating, there results ⅛ inch difference in length of stroke. The meter registers the number of strokes, hence, if adjusted to register correctly at rate of 3 gallons, it will register about $\frac{1}{8}$=6% too little at rate of 15 gallons per minute.

As the rate becomes less than 3 gallons per minute, the diminution in length of stroke is more marked.

The *Kennedy curve*, when compared with the Worthington, shows the advantage of registering the approximate distance traveled by the piston, in place of the number of strokes, the main sweep being practically parallel to the axis of abscissæ. By proper adjustment, *i. e.*, by shifting the 100% line upward 1.05% (see dotted line), this curve is made almost perfect.

The *Siemens curves* were improved by adjustment (see dotted lines). They show inferiority in point of sensitiveness. This is due to the resistances of solid friction opposing the motion of the measuring wheels.

The *Hesse curve* shows a favorable adjustment, and a degree of sensitiveness nearly equal to that of the best piston meters. The effect of solid friction is not observable for quantities exceeding one gallon per minute. The curve, following

the law of combined fluid pressure and resistance, rises rapidly for quantities less than four gallons per minute. If there were absolutely no solid frictional resistance, the curve would mount to a great height as the quantity approached zero—see considerations which led to the adoption of the form of measuring wheel.

COMPARISON. — The meters described will be compared with reference to the considerations enumerated.

1st. The accuracy will primarily be compared by means of the adjusted curves, and without reference to permanency. Such comparison shows the Kennedy curve to be without doubt the best; then follow in order the Hesse, the Worthington A and B, the German Siemens, the English Siemens, the Worthington D.

2d. The necessity of special adjustment is greater in the Worthington than in the Kennedy, in the Siemens than in the Hesse. A close comparison is difficult without the experience of the manufacturers; it is apparent, however, from the curve, that even the Kennedy requires a special adjustment if great accuracy is sought.

3d. The difficulty of special adjustment is greater than it should be, in each of the meters excepting the Worthington. Provisions should be made for these adjustments outside of the casing. In the Hesse meter, for instance, this might be effected by suitable provision for shifting of one or more of the stationary ribs. The Worthington is easily adjusted by tightening or loosening the screws of the cap, covering one of the rubber seatings.

4th. A certain degree of sensitiveness is important. This is apparent from the fact that one gallon in three minutes can be made to supply a household by use of a small storage tank. The Kennedy 1″ will register a gallon in from 20 to 30 minutes; the Worthington ⅝″ and the Hesse ½″, a gallon in 15 minutes; the two one inch Siemens meters a gallon in from 1 to 1½ minute. A direct comparison of these figures would not be fair to the Siemens meters, as the deliveries (under given head) of the sizes tested were greater than those of the Worthington and the Hesse. However, it is safe to say that the Siemens meters are much inferior in point of sensitiveness. The wear of the Worthington piston will cause deterioration in this respect, unless the meter is judiciously used.

The sensitiveness of the Hesse meter may be greatly increased, but at the expense of the accuracy at small rates of delivery. However, a rate of one gallon in 15 minutes or 96 gallons in 24 hours, is about the minimum rate admitted in Providence, R. I., and only ¼ of the quantity passed in the average service. Such a degree of sensitiveness makes theft out of the question.

5th. With respect to permanency of sensitiveness and accuracy, it is confidently thought that the Hesse meter will stand foremost under a wide variation of wear, etc.

There is no leakage due to wear of valves and piston, no alteration due to change of friction by wear, rusting, or oiling of the registering apparatus. The only parts which might be regarded as sensitive in this respect, are the circular openings *a a*; but as these are made of hard rubber, no rusting can take place, and any tendency towards diminution in size of these openings, by deposition of sediment, is overcome by the rapid flow of water. Little of the wear which may take place in the meter is of a nature to effect its curve of registry.

It is probable that the Kennedy curve is reasonably permanent.

The effect of wear, upon the Worthington curve, is plainly shown by comparison of curves B, C and D. It must be remarked, however, that it is unfair to charge against this meter a deterioration which appears to be due to over taxation. The manufacturing company calls special attention to the fact that their ⅝ inch meter should not be taxed with a delivery greater than 7½ gallons per minute. This corresponds to an effective head of about 16 feet. In San Francisco, where the hydrostatic head runs up to two hundred and fifty feet and more, it does not seem likely that the ⅝″ Worthington is large enough for the average service.

That the Siemens curves are not particularly permanent is plain from the fact already mentioned, viz.: that the accuracy and sensitiveness depend upon the magnitude of the frictional resistance of the indicating apparatus, and this will vary constantly with rusting, wear, etc. Mr.

Salbach, who has given the German Siemens meter careful consideration, says it is capable of giving good results in every respect when new, but after a while the meter will deteriorate in so far as the accuracy is concerned in the measurement of quantities at small rate of flow. "The main cause," he further says, "is the oil which is contained in the first chamber above the wheel, and which in time adheres to the gear wheels. A further detrimental effect is produced by freezing or thickening of the oil in case the temperature sinks to 2 or 3° C. From these facts it is plain that the oil chamber is a bad feature of this meter, and one that there should be an energetic effort made to overcome." This has been effected in the Hesse Meter.

6th. The liability to obstruction was not tested in the Worthington and Hesse meters, as similar data was wanting in connection with the others. A suitable screen (see Figs. 9 and 18) should be provided for each meter to keep out the coarser obstructions, such as leaves, straw, chips of wood, wads of oakum, etc., which are easily withheld. The liability to obstruction is said to be a weak feature of some of the rotary piston meters, but not of the oscillating. To be sure a sandy deposit in the measuring cylinder will cause rapid wear in the Worthington, and a certain resistance to free rolling of the rubber ring in the Kennedy; but with reasonably clear water no serious difficulty seems likely to occur if the coarser obstructions are screened, and thus wedging of valves prevented. The sandy or muddy sediment is probably less detrimental to the velocity meters when properly constructed. It is suggested that the cylindrical wheel chamber in Hesse's meter should be extended a few inches in length and provided with a waste cock at the bottom, for convenient discharge of accumulated sediment, in case it should be used for the measurement of muddy water.

7th. The greatest advisable rate of delivery is least in the Worthington, and most in the velocity meters. As already stated the Worthington is not guaranteed for an effective head exceeding 16 or 20 feet, corresponding to a delivery of $7\frac{1}{2}$ gallons per minute by the $\frac{5}{8}''$, 15 gallons by the $\frac{3}{4}''$, etc. The safe limit of effective head in the Kennedy is, according to the manufacturer's statement, about 60 feet, the $\frac{5}{8}''$ delivering 20 gallons per minute, the $\frac{1}{2}''$ 30 gallons, the $1''$ 70 gallons, etc.

In the velocity meters the limit of head is exceedingly high. In the Hesse meter this limit is dependent almost solely upon the action of the rubber diaphragm. The difference of heads in chambers B and C, measuring about twice the effective pressure upon the diaphragm, will depend upon the square of the ratio of areas of the valve opening *d* and jet openings *a a*. In the meter tested the diaphragm is subjected to but $\frac{1}{20}$ of the total pressure lost in the meter, and this may be diminished at will by simple enlargement of the valve opening. By closing the opening *h* and fastening down the valve, the diaphragm was subjected to 25 feet of pressure without damage. The diaphragm then would not give way under a total loss of head in the meter of $25\times20=500$ feet. The further question which must be considered, is: Will the bellows operate under the rapid motion of the worm wheel due to high loss of head? A greater head than 60 feet was not available for trial, but in this case the time occupied in lifting cap K was only one-half the interval of high pressure. If the velocity of registering should be too great under very high heads, the difficulty is simply remedied by increasing the width of the arm, or the number of teeth, in the worm wheel. It is not thought that this would be necessary as the rate of supply of water to the bellows chamber is nearly proportional to the velocity of the measuring wheel.

8th. The compactness of the velocity meter is great as compared with that of the oscillating piston meter. This will appear from a comparison of the weights of meters of about the same delivery.

9th. The prices are given, as near as possible, in the following table: The price, as well as weight, increases much more rapidly with increase of delivery under the same head, in the piston meters, than of the velocity meters. In either form, the price increases at a smaller rate than the delivery. For example, a Kennedy 1 inch meter, delivering four times as much as the $\frac{5}{8}$ inch, costs less than $2\frac{1}{2}$ times as much; the English Siemens $1\frac{1}{2}$ inch, delivering five times as

TABLE OF DELIVERIES, GREATEST ADVISABLE RATES OF DELIVERY, WEIGHTS AND PRICES OF METERS.

Name of Meter.	Size of in and out lets. Inches.	Delivery unit 9 gals. per min. under eff. head of 20 feet.	Greatest Advisable Rate of delivery in gals. per min.	Greatest Advisable Effective head in feet.	Weight in lbs. avoirdupois.	Price $
Worthington......	$\frac{5}{8}$	1.0	7.5	16	59	17.00
	$\frac{3}{4}$	?	15.0	——	103	27.00
	1	?	30.0	——	175	33.00
*Kennedy.........	$\frac{3}{8}$	1.3	19.9	60	104	19.36
	$\frac{5}{8}$	1.8	30.0	——	162	27.83
	$\frac{3}{4}$	2.8	50.0	——	206	33.82
	1	5.4	70.0	——	322	46.00
	$1\frac{1}{4}$	11.0	150.0	——	564	75.00
English Siemens...	$\frac{3}{8}$	.2	* *	* *	?	12.10
	$\frac{5}{8}$	.5	——	——	?	14.76
	$\frac{3}{4}$	.8	——	——	?	18.15
	1	2.0	——	——	?	21.30
	$1\frac{1}{4}$	2.9	——	——	?	26.86
	$1\frac{1}{2}$	4.1	——	——	?	30.75
German Siemens..	1	2.5	——	——	83	26.00
Hesse............	$\frac{1}{2}$	1.0	——	——	21	* * *

* The deliveries and greatest advisable effective heads in the Kennedy and English Siemens meters, were calculated from data furnished by the manufacturers' circulars. The Kennedy ¼ inch meter is omitted as it is not recommended by the manufacturing company.

* * The greatest advisable effective head is many times greater in the velocity than in the piston meters.

* * * A careful estimate of the cost of manufacture of the Hesse meter, indicates a price considerably less than that of any other meter named.

much as the $\frac{3}{4}$ inch, costs less than twice as much. Any comparison therefore of the price of meters of different delivery should be made with caution. The fallacy is apparent in the claim made by the Kennedy Co. to the effect that their 1 inch meter, delivering 2.02 times as much as the Siemens 1 inch, has over double the proportionate money value, and as it costs about 1.63 times as much as the Siemens, is therefore $\frac{2.02-1.63}{1.63}=24$ % cheaper. Reference to the table will show that the Kennedy $\frac{1}{2}$ inch, delivering less than the Siemens 1 inch, costs considerably more.

The prices may be compared as follows:

(*a.*) Upon the basis of equal delivery, the list shows the Worthington to be cheaper for small sizes than the Kennedy or Siemens. As the deliveries increase, the Siemens become cheaper than the Worthington or Kennedy. The price of the Hesse has not been definitely ascertained.

(*b.*) Upon the basis of greatest advisable rate of delivery, the velocity meters are by far the cheapest, and the Worthington the most expensive. This is an important consideration only where there is on hand an abundant surplus of head for the house service. If, for example, the head is such as to make the capacity of the service pipe 10 or 12 gallons per minute, it becomes advisable to employ a $\frac{3}{4}$ inch Worthington, whereas a Siemens of far less delivery, or a $\frac{1}{2}$ inch Hesse meter will amply serve.

10th. The expense of repairs of the English Siemens meter is permanently guaranteed by the Manufacturing Co. for 5 % annually upon the original cost. The actual expense in this and the Kennedy, seems to be in the neighborhood of 3 or 4 % per annum upon the original cost. It is claimed that the average life of the rubber roller in the Kennedy is more than three years. If the Worthington is overtaxed, the wear of the piston, etc., will necessitate expensive repairs in order to maintain its sensitiveness.

In the Hesse meter the life of the rubber diaphragm remains to be ascertained. It is known that pure rubber will deteriorate quite rapidly when exposed to air and light; but it is claimed that in cool water, under the exclusion of light and

air, it will remain intact for an indefinite period. The rubber diaphragm is not taxed as is the roller of the Kennedy. There was scarcely a perceptible wear in the Hesse meter during the passage of 62,000 cubic feet of water. The velocity of the measuring wheel (190 revolutions per cubic foot) is far less than in the Siemens meters of equal capacity, and its weight is trifling. The spindle does not pass through a stuffing box. The shaft P, which does pass through a stuffing box, makes only $\frac{1}{14}$ revolution for each cubic foot registered.

Conclusion.—The following is an attempt to rank the meters according to their merits with respect to the more definite of the considerations enumerated. This is done with a certain reserve, as, in some cases, the information at hand is not sufficiently complete to admit of positive conclusions. Where two meters are placed in the same vertical column no comparison between them is attempted.

1. Accuracy.......K. H. $W_{(A)}$. S. $W_{(D)}$.
2. Sensitiveness ...K. $\left\{\begin{matrix} W_{(A)}. \\ H. \end{matrix}\right.$ $\left\{\begin{matrix} S. \\ W_{(D)}. \end{matrix}\right.$
3. Permanency....H. K. $\left\{\begin{matrix} S. \\ W. \end{matrix}\right.$
4. Greatest advisable rate of delivery. $\left\{\begin{matrix} S. \\ H. \end{matrix}\right.$ K. W.
5. Compactness........... $\left\{\begin{matrix} S. \\ H. \end{matrix}\right.$ W. K.
6. Price—(*a*). Upon basis of equal delivery under the same head............. $\left\{\begin{matrix} W. \\ S. \end{matrix}\right.$ K.

   Price—(*b*). Upon basis of greatest advisable rate of delivery............ $\left\{\begin{matrix} S. \\ H. \end{matrix}\right.$ K. W.
7. Expense of repairs......H. $\left\{\begin{matrix} S. \\ K. \\ W. \end{matrix}\right.$

The Worthington, though sufficiently accurate, and quite sensitive when new, has very small advisable rate of delivery, and unless judiciously used is subject to deterioration with respect to sensitiveness.

The Kennedy, while extremely accurate and sensitive, and an excellent meter in other respects, is the heaviest and most expensive.

The Siemens meters are very compact and admit of a high effective head without damage; but, while their accuracy is sufficient for considerable rates of delivery, they are inferior in point of sensitiveness.

The Hesse meter combines, in the main, the good features of the others, with an advantage in respect to permanency of sensitiveness, price and wear.

Construction of the Hesse Measuring Wheel.—The following is an abstract of the statement made by Prof. Hesse with regard to the considerations which led him to the present construction of the measuring wheel, and the introduction of the stationary ribs.

A is the area of opening, a.

r, the radius of the wheel.

v, the velocity of the wheel.

c, the velocity of the water in passing opening a.

P, the effective pressure (reduced to radius r) of the jet upon the wheel, including all pressures, positive or negative, directly due to the action of the jet.

R, the resistance (reduced to radius r) caused by the displacement of the mass of the water.

F, the solid frictional resistance (reduced to radius 1), offered by the spindle journals and the worm wheel.

M and N, constant coefficients of pressure and resistance.

For permanent (uniform) motion of the wheel—

$$P=R+\frac{F}{r} \qquad (1)$$

The actual quantity of water delivered, $Q=Ac$.

The quantity registered, $Q^1 = Bv$, wherein B is a constant determined by the gearing, etc.

The ordinate in the curve of registry, $\frac{Q^1}{Q}=\frac{Bv}{Ac}$, is dependent upon P, R and $\frac{F}{r}$

On account of the variability in the workmanship and degree of wear, etc., upon the spindle journals and the worm wheel, the magnitude of F is subject to an unavoidable fluctuation of say 50%. Therefore, by diminishing the influence of F upon $\frac{v}{c}$ the permanency of the curve of registry is increased. It is evident also that, by diminishing the value of $\frac{F}{r}$, the Q necessary to move the wheel is lessened, *i. e.*, the meter is made more sensitive.

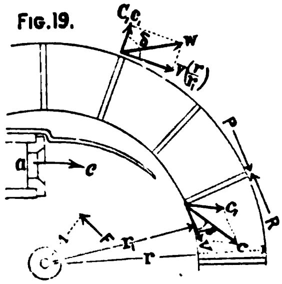
In order to show that velocity meters of this and similar forms are practicable, it is only necessary to point out that $\frac{v}{c}$ is approximately constant. If this ratio were strictly constant, such a value of B could be reached by adjustment as as to make $\frac{Bv}{Ac}=1$, and the meter would be perfectly accurate.

If the losses of head are assumed to be proportional to the squares of velocities, actual and relative, then

$$P\left(\frac{r}{r_1}\right)v=\frac{Q\gamma}{2g}\left(c^2-Cc_1{}^2-w^2\right),$$

wherein γ is the weight of unit volume of water, C a constant, c_1 the relative velocity of the water to the bucket, w the actual velocity of discharge. See Fig. 19. $Q=Ac$.

FIG. 19.



$$c_1{}^2=c^2+v^2-2vc\cos.\beta=c^2\varphi_1\left(\frac{v}{c}\right),$$

$$w_2=C_1{}^2c_1{}^2+\left(\frac{r}{r_1}\right)^2v^2-2\,(\cos.\,\delta)\left(\frac{r}{r}\right)C_1vc_1$$

Hence by transformation

$$P=\frac{A\gamma}{2g}\left(\frac{r_1}{r}\right)\left\{c^2\left(\frac{c}{v}\right)-c^2\varphi_2\left(\frac{v}{c}\right)-c^2\varphi_3\left(\frac{v}{c}\right)-c^2\varphi_4\left(\frac{v}{c}\right)\right\}$$

or

$$P=Mc^2\varphi\left(\frac{v}{c}\right)\qquad(2)$$

Wherein the function of the ratio of velocities, $\varphi\left(\frac{v}{c}\right)$, increases with decrease of v—*i. e.* the pressure upon the wheel increases when c remains constant and the velocity of the bucket, v, is forcibly diminished,

$$R=Nv^2$$

For $F=o$, by introducing values of P and R into equation (1).

$$Mc^2\varphi\left(\frac{v}{c}\right)=Nv^2,$$

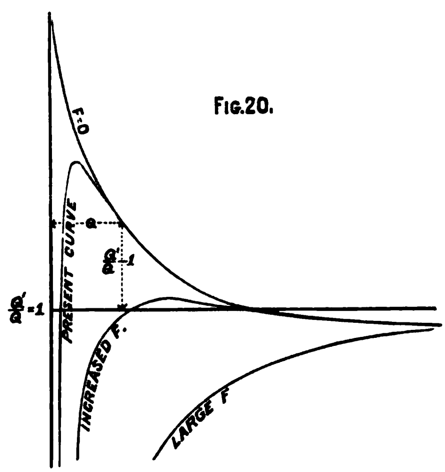
$$\frac{M}{N}\varphi\left(\frac{v}{c}\right)=\left(\frac{v}{c}\right)^2.$$

If then P were strictly proportional to c^2 and $\varphi\left(\frac{v}{c}\right)$, $\frac{v}{c}$ would be a constant for $F=o$.

However, the adopted law of loss of heads is a good approximation for considerable velocities only, therefore the result obtained indicates simply that, if the influence of F could be overcome, $\frac{v}{c}$ would not vary to any great extent, excepting for small rates of delivery.

The actual curve of registry, for $F=o$, is doubtless similar to that shown in Fig. 20.

FIG. 20.



For any value of F (see equation 1)—

$$P=Nv^2+\frac{F}{r},$$

$$v^2=\frac{P}{N}\left(1-\frac{F}{rP}\right)$$

The ordinate in the curve of registry—

$$\frac{Bv}{Ac}=\frac{B}{Ac}\sqrt{\frac{P}{N}\left(1-\frac{F}{rP}\right)}$$

This approaches the condition $F=o$, and the influence of a 50 % fluctuation of

F is the less, as the value of $\frac{F}{rP}$ is diminished—*i. e.* as F is diminished, or as r and P are increased.

It appears from the above that the meter will gain in sensitiveness and permanency:

(I.) by diminution of the solid frictional resistance (F);

(II.) by increase of the radius (r) of the measuring wheel;

(III.) by increase of the pressure (P) of the jet upon the wheel.

This increase of P may be effected:

(1) by reducing the area of the openings *a a*;*

(2) by increasing the resistance (R) of the water, and thus reducing the velocity (v) of the wheel. This increase of R is accomplished:

(a) by increasing the area of bucket, *i. e.*, the effective displacing area;

(b) by introducing stationary ribs, and thus checking in part the rotation of the body of water in the measuring chamber.

In one wheel tested, a high degree of sensitiveness was reached, by curving that portion of the bucket encountered by the jet, and arranging for outer feed, thus greatly increasing the pressure. However, such refinement was found unnecessary, and the small addition to the expense was not deemed advisable.

Due regard to the considerations enumerated, has led to sufficient sensitiveness and a high degree of permanency.

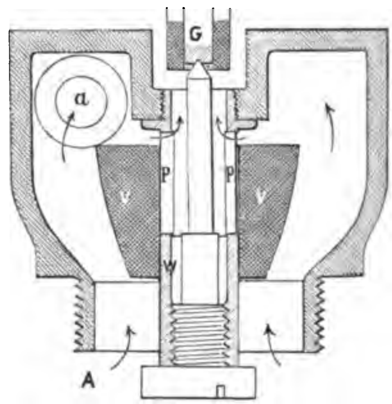
Improvement of the Curve of Registry.—There remains to be considered the question of a further improvement of the curve of registry. An immediate improvement of that portion of the curve corresponding to values of Q between 1 and 4 gallons per minute, may be effected by a small increase of F (see Fig. 20); but it must be borne in mind that such increase of F is necessarily accompanied by a considerable loss in sensitiveness.

Prof. Hesse devised the attachment shown in Fig. 21, with which the curve of registry was perfected without sacrifice of sensitiveness.

* This is a convenient method of improving the curve of registry, but is limited in its application on account of the corresponding reduction of the delivery of the meter under a given head.

The light hard-rubber valve V slips freely upon the outer surface of the tube W. If Q is considerable, say 6 gallons or more per minute, the valve V is lifted to its highest position and no water can escape through the openings *p p*. If Q, hence the effective head, is less, the valve

Fig.21.



will occupy a lower position of equilibrium, and a small portion of the water will escape through the openings *pp* without assisting in the rotation of the wheel. Thus $\frac{Q^1}{Q}$ may be reduced more and more as Q becomes smaller. This reduction is experimently regulated by the width of the openings *p p*, and by the curvature of the surface of valve V.

This is important in connection with a test meter only. The present curve of registry is all that may be desired in a house meter, and certainly no device should be attached to the meter which would detract from the permanency or add to the liability to obstruction.

The only place in America where graphite is now successfully mined is at Ticonderoga, N. Y. The output of the Ticonderoga mine in 1882 was 400,000 lbs., which will be increased to 500,000 lbs. this year. The mine is owned by the Joseph Dixon Crucible Company, of New Jersey, which is working a vein of graphitic schist, 15 feet, and carrying only from 8 to 15 per cent. of graphite. This is crushed and concentrated by a wet process, in which the "tailings" are the useful product. The Marquette *Mining Journal* believes that the graphite veins of the upper peninsula of Michigan, particularly those of Baraga County, are as pure and free from grit as either the New York, Canadian or Ceylon deposits.

WATER MOTORS *

BY PROFESSOR W. CAWTHORNE UNWIN, M Inst. C. E.

From "The Engineer."

THE lecturer remarked that water motors were not in this country so important as heat motors, but yet a larger amount of water power was utilized than was commonly supposed. Either a rise in the price of coal or greater ease in the electrical transmission of mechanical energy would make water motors more important.

At Windisch 1,000 horse-power were utilized, and at Bellegarde 3,700 horse-power. But it was in America that the largest installations were found. For instance, at Holyoke, a weir 30 ft. high and 1,000 ft. long had been built across the river, and the water, taken off above the dam in a canal 140 ft. wide and 22 ft. deep, supplied power to the mills. Two other canals, on lower levels, had also been constructed. The whole water power available was 20,000 horse-power in ordinary dry seasons, and the power was leased to the mill-owners at the rate of £1 per effective horse-power per annum.

When water was descending from a higher to a lower level, in a pipe, for instance, it would in general have acquired, at any point, velocity and pressure. Then if H was the total fall,

$$H=h+\frac{p}{G}+\frac{V^2}{2g}$$

where the terms on the right represent three forms the energy took, viz., unexpended fall, pressure head, and velocity head. Corresponding to these were three classes of water motors.

I. Cell or bucket wheels utilized the energy of an unexpended part of the fall. They were simple, had a fairly high efficiency, but were applicable only in a limited range of fall, and were cumbrous. They had one good point, that the efficiency varied little with a varying water supply.

II. There were water-pressure engines, which utilized water pressure, just as a steam engine utilized steam pressure. Water-pressure engines were used in mining districts, and hydraulic cranes and hoists were machines of the same kind. Still it was not generally best to utilize water power in this way. The reasons that a cylinder and piston acted so well with steam, and less well with water, were these: (1) The frictional losses in fluids were proportional to their weight for equal velocities of flow. If water were 500 times heavier than steam, then the friction at given velocities would be 500 times greater. Hence the piston speeds, which might be 400 ft. or 500 ft. per minute in a steam engine, were rarely greater than 60 ft. in a water-pressure engine. In the pipes the steam might flow at 100 ft. per second, but water only at 6 ft. to 10 ft. Hence the water-pressure engine was much more cumbrous than a steam engine. (2) From the incompressibility of water, the same volume was used in a pressure engine, whatever the amount of work to be done. The system of hydraulic-pressure-mains, introduced by Sir W. Armstrong, had proved entirely successful for intermittent work, such as lifting, but not so successful for ordinary power purposes. Probably the reason was that the consumption of water in pressure engines was extravagant. The only engine in which the consumption of water varied with the work to be done was in that invented by Mr. Hastie, and which, in an improved form, was being introduced in London by Mr. Ellington. In this, by automatic gear, the stroke of the engine was lengthened till the resistance was just overcome. (3) There were other difficulties which arose out of the weight and incompressibility of the water. The whole column of water between the engine and the supply-reservoir virtually formed part of the piston, so that a pressure engine had a very heavy piston, and this in general tended to render the effective effort variable. In certain cases the action of the friction and inertia of the water were favorable to particular

* Abstract of a paper read before the Institution of Civil Engineers.

operations. Mr. Tweddell's hydraulic riveter furnished an example of a machine in which the inertia of the parts was virtually that of 300 tons acting at the riveter-ram. To control such a mass, powerful brake action was necessary, and this was supplied by the automatic action of the friction of the water in the supply pipe. The friction, while preventing the velocity becoming excessive in the early part of the stroke, when there was little resistance, diminished when the velocity was checked in closing the rivet, and allowed the development of a high effective pressure at the end of the stroke.

III. There were motors in which the head was allowed to take the form of energy of motion, and in which the water acted in virtue of its inertia. It was first necessary to consider in what way this energy might be wasted. There was a waste of energy if the water broke up into eddies or irregular motions. Energy might be rejected from the machine into the tail race, and lastly, energy was lost in friction against the surfaces of the machine. In a good motor the loss due to breaking up could be almost completely avoided, if the passages changed gradually in section, if the surfaces had continuous curvature, and if the inclination of the receiving edges of the vanes was in the direction of relative motion. The loss due to rejection of energy could also be made very small, but by reducing this beyond a certain point, the loss from skin friction was increased.

The nature of a turbine would be best understood by considering first some simpler machines. In an ordinary undershot wheel about 25 per cent. of energy was rejected into the tail race and about 25 per cent. was lost in shock, besides the losses due to shaft friction and leakage. Suppose that instead of striking normally a flat float the water struck a hollow cup. Then both the losses in shock and in energy rejected were greatly reduced. An interesting motor of this kind had been introduced in California. Water power was there obtained from canals or ditches built to supply the mines, at elevations of from 1,000 ft. to 3,000 ft. above the valley. The fall was too great for an ordinary turbine, and a kind of undershot wheel with cup-shaped floats was used. Thus, at the Idaho mines, seven of these wheels were used, working to 320 horse-power. The water was brought in a 22in. wrought iron main, the head being 542 ft. The water was delivered on to the wheels from nozzles $\frac{15}{16}$in. to $1\frac{15}{16}$in. in diameter. These wheels were said to give 80 per cent. efficiency, and in the special circumstances, where the jets were of small diameter under very high pressure, a high efficiency was probably obtainable. Such a wheel was the simplest form of an impulse turbine.

Next, consider a machine well known at one time as the Scotch turbine. The water, entering the center of the wheel, was discharged from tangential jets, and drove the wheel by reaction. The water issuing from the jets had a backward velocity, and energy was therefore rejected. It was the study of a wheel of this kind which led Fourneyron to the invention of the turbine. Fourneyron saw that the only way of reducing the backward velocity of discharge, and to prevent the waste of energy, was to give the water an initial forward velocity. The Fourneyron turbine was a reaction wheel in which initial forward velocity was given by fixed curved guide-blades. In the Fourneyron turbine, the water descending into the center of the wheel issued radially outwards. Its greatest defect was the form of the regulating-sluice, which diminished seriously the efficiency for diminished water supply. It was followed by the Jonval turbine, in which the water flowed parallel to the axis, and by the inward-flow turbine first perfected by Professor James Thomson. In the ample space outside an inward-flow turbine, better regulating apparatus could be arranged than with any other form.

In all turbines as first constructed, the water issued from the guide blades with a velocity less than that due to the head, and, therefore, there was a pressure in the clearance space between the guide blades and the wheel, and such turbines were called pressure turbines.

M. Girard was the first to receive the advantage of departing from Fourneyron's practice, and to allow the water to enter the wheel with the whole energy of motion due to the head. Such turbines were called impulse turbines. In

pressure turbines the water must enter the whole circumference of the wheel simultaneously, and fill the wheel passages, or the pressure in the clearance space could not be maintained. Further, there must be rate of flow through the wheel to maintain the distribution of pressure and velocity for which the turbine was designed. In impulse turbines each particle of water acted for and by itself; the wheel passages were not filled, and the water might be shut off from any part of the wheel circumference without affecting the action of the water. Regulation was therefore easier in impulse than in pressure turbines.

The consideration of the action of water in a turbine was facilitated by replacing the turbine wheel by a turbine rod, that was by supposing the turbine to have straight instead of circular motion. From such a rod, the transition to the case of a turbine wheel by simple projection was easy. The water entering vertically, was deflected by fixed guide blades, till it had a horizontal velocity, w_i; it then entered the wheel, the vanes of which were so placed as to receive it without shock, and was deviated back to a vertical direction, having given up all its horizontal energy of motion. Hence, applying Newton's second law $\frac{w_i}{g}$ was the horizontal pressure driving the wheel due to each pound per second of water. But V being the velocity of the wheel, the work done on the wheel, $\frac{w_i\, V}{g}$ foot-pounds per second. Now the work might also be written η H, where H was the effective fall, and η the efficiency. Hence

$$\frac{w_i\, V}{g} = \eta\, H$$

was the fundamental equation in designing a turbine.

There was one point in the action of a turbine which was unfavorable. The efficiency varied with the speed. There was a speed of maximum efficiency. But with pressure turbines the speed of maximum efficiency diminished with the closing of the sluice. Now, in general, a turbine must be run at one speed. If it was run at the speed of greatest efficiency for a full sluice, then for a partially closed sluice the efficiency was reduced both by the action of the sluice and by the fact that the speed was not the best for that position of the sluice. In the mode of regulating turbines great differences of efficiency arose. The worst mode of regulation was by a throttle valve, which acted by destroying part of the head. Next to this perhaps the worst was the circular sluice, used with the Fourneyron turbine. Some modern turbines gave, in experiments which appeared to be reliable, much better results. The best mode of regulation for a pressure turbine was by means of movable guide-blades, invented by Professor James Thomson. In a Girard turbine the regulation was not so difficult, and results of an experiment carried out under the direction of Professor Zeuner on a 200 horse-power Girard turbine gave 79.5 per cent. efficiency with full sluice, and 80.1 per cent. with half sluice.

ON THE ANTISEPTIC TREATMENT OF TIMBER.

By SAMUEL BAGSTER BOULTON, Assoc. Inst. C. E.

From Papers of the Institution of Civil Engineers.

I.

In January, 1853, a paper upon Timber Preserving was contributed to this Institution by the author's partner, the late Mr. Henry Potter Burt, Assoc. Inst. C. E. Since that date, the use of Antiseptics for the treatment of timber has largely increased, and is year by year increasing. For engineering purposes, the process called creosoting, which consists in the injection of the coal-tar oils, has in this kingdom entirely, and in other countries to a very considerable extent, displaced the other well-known methods.

Concurrently with this development, a series of remarkable discoveries in chemical science has raised the manufactures connected with the residual prod-

ucts of gas-making to a position of great and growing importance.

It is proposed in the present paper, to give a short account of the history and development of the use of antiseptics for preventing the decay of timber. A reference to the processes employed in coal-tar distillation will be pertinent to the subject, in so far as it will indicate what are and have been the usual constituents of the tar oils used for injecting wood. The author proposes to add some results derived from his thirty-four years' experience in connection with this group of manufactures, together with the outcome of some research, and of a number of experiments specially undertaken with a view to the elucidation of questions referred to in the paper.

EARLY HISTORY OF TIMBER PRESERVING.

Timber was naturally the first material employed by man for the purposes of constructive engineering. If it be true that the first models of Grecian architecture were copied from, and retained some of the distinctive features of, buildings in wood, then may still be seen recorded, upon the columns of the five great orders of architecture, proofs that the Greeks or their precursors took special expedients to preserve timber from decay. The wooden pillar was placed upon a block of stone to preserve it from the humidity of the soil, and it was covered at the top by a slab or tile to throw off the rain. These contrivances are supposed to have been copied in the base and capital of the column, when wood came to be replaced by stone. Scamozzi imagines also, that the mouldings represent metal hoops, placed around the wooden pillars to prevent them from splitting.

Allusions to various substances employed for preserving timber and other vegetable fibers from decay, are frequent in the writings of the ancients. Tar and pitch were used for painting or smearing wood from periods of the most remote antiquity. Greek and Roman authors narrate, that the astringent portions of the oil expressed from olives (Amurca 16), also oils derived from the Cedar, the Larch, the Juniper and the Nard-Bush (Valeriana) were used for the preservation of articles of value from decay, or from the attacks of insects. The magnificent statue of Zeus by Phidias was erected in a grove at Olympus where the atmosphere was damp; the wooden platform upon which it stood was therefore imbued with oil. The famous statue of Diana at Ephesus was of wood. If its origin was believed to be miraculous, no standing miracle was relied on for its preservation. Pliny asserts upon the authority of an eye-witness, Mucianus, that it was kept saturated with oil of Nard by means of a number of small orifices bored in the woodwork. The same author remarks that wood well rubbed with oil of Cedar, is proof aginst wood-worm and decay. The art of extracting and preparing oils, resins, tar and pitch from various trees and plants, and from mineral deposits, is mentioned by Herodotus, and at great length by Pliny. This last author describes in detail, the manufacture of no less than forty-eight different kinds of oils. Of the employment of the oxides or salts of metals by the ancients, for wood preserving, there is no direct evidence.

EGYPTIAN MUMMIES.

Of all the methods employed by mankind for the artificial preservation of organized substances, there are perhaps none which have equaled in success the processes of the ancient Egyptians. The durable results of these processes are amazing, and although the topic is a hackneyed one, it is nevertheless inseparably connected with the subject of this paper. The descriptions by co-temporary writers of the Egyptian art of embalming the dead are somewhat conflicting; moreover, they do not adequately explain the appearances presented by many of the mummies themselves. The bodies are said to have been imbued, either with resinous or odoriferous gums, or more frequently with bitumen or with oil of cedar, or commonly with natrum, and often with several of these substances in succession. So far, these statements are confirmed by modern investigation. By reading Herodotus and Diodorus Siculus, however, it would perhaps seem that the body was first steeped in the natrum for seventy days, and then subjected to the oily or bituminous preparation. In other places it might be gathered that the oily preparation came first, and the steeping

in natrum afterwards. Without further explanation, neither of these processes would appear to be practicable. At ordinary temperatures, the steeping in the one preparation would interfere with the absorption of the other. Natrum is supposed to have been a natural substance, obtained from some briny lakes, still existing in the neighborhood of Cairo, and consisting principally of a mixture of sodium-sesqui-carbonate, sodium-chloride and sodium-sulphate. Rouyer, who accompanied the army of Napoleon to Egypt in 1798, expressed his conviction that the mummies had been placed in ovens in order to eliminate moisture, and to facilitate the penetration of the bitumen. But no ancient author mentions any such process, nor is there any record of it amongst the numerous and detailed pictorial representations which have been discovered in tombs and temples.

Pettigrew, in his valuable work on this subject, whilst giving the results of his examination of various mummies, and of analyses of embalming materials, expresses his opinion that the bodies must have been subjected to a very considerable degree of heat, as even the inmost structure of the bones is penetrated by the antiseptics. By some it has been supposed that this was effected by steeping the body in a cauldron of heated bitumen. Pettigrew's most striking experiment was made with the heart of a mummy, from which he succeeded in withdrawing by maceration the preservative substances, when, after 3,000 years of perfect preservation, the heart began at once to putrefy. This is a striking proof, both of the efficacy of the substances employed, and also of the fact, that the immunity from decay was not due to a chemical transformation produced once for all, but that it depended upon the abiding presence of the antiseptic. In recent anatomical practice, carbolic acid has been used for injecting bodies for purposes of dissection. When this is done, however, it is found necessary to renew the process after the lapse of a few weeks, a contrast to the antiseptics employed by the Egyptians. Pettigrew's description showed that the worst preserved of the mummies are those prepared with natrum alone, the most perfect being those in which solid resins or bitumens remain incorporated. Natrum is frequently found accompanying the bitumen in some of the most successfully preserved specimens. It is probable that some astringent or other substances were also used, the secret of which has hitherto eluded modern investigation.

The author has caused some experiments to be made with pieces of timber, in order to test a theory which suggested itself to his mind. The wood was first thoroughly impregnated with a mixed solution of the three salts of sodium of which the natrum brine is composed. Afterwards the wood was steeped in tar oil, heated to 230° Fahrenheit. The heat of the tar oil volatilized the water of the soda solution, and the oil took the place of the water. The timber remained impregnated with the saline particles, and saturated with the tar oil. May not this have been the method used by the Egyptians to impregnate both with natrum and oils?

There is no doubt that the ancients had, by observation and experience, acquired considerable practical knowledge of antiseptic substances. They were also of opinion that those woods lasted the longest which were most odoriferous, or, in other words, those which contained the greatest quantity of resin. They knew that timber continually kept under water was less liable to decay than when exposed to the atmosphere. They observed the ravages of the *Teredo navalis* upon timber placed in the sea. But it is useless to seek amongst the writings of the elder Classics for any reasonable theory in explanation of these phenomena.

Growth of theories upon the causes of Putrefaction.—It is not until the eighteenth century of the present era that anything beyond the merest trace can be detected of serious analytical research into the causes of decomposition. After the fanciful dreams of the alchemists had been dissipated, the more solid portion of their labors, facts arrived at in the course of their experiments, remained for the uses of science. Investigations were undertaken respecting the phenomena of fermentation and of putrefaction, animal and vegetable. It was at one time declared that putrefaction was due to the escape of an element called phlogiston, an imaginary substance which was believed in by such eminent chem-

ists as Scheele, the discoverer of chlorine, and Dr. Priestley, the discoverer of oxygen. Later on Dr. Macbride propounded a theory that carbonic acid gas had a special power of promoting cohesion, and that putrefaction was due to its being given off. None of these theories explained why putrefaction did not attack the tissues until after the vital movement had ceased. By the commencement of the present century, however, it began to be generally believed that the putrefaction, at least of vegetable matter, was a species of fermentation, although it was not admitted that ferments of any kind were the products of living organisms. Little by little the similarity of the natural processes connected with the fermentation of alimentary substances, the decay of vegetable tissues, and the putrefaction of the bodies of animals began to be recognized; and, to the great advantage of scientific progress, these three classes of phenomena have ever since been studied in close connection with each other.

In the meantime practice stole a march upon theory. About the year 1770 Sir John Pringle published a list of antiseptics, in which example he was followed by Dr. Macbride. Many of the substances proposed by these and other theorists, particularly the alkaline bodies, are absolutely injurious to timber. But towards the close of the last century and at the beginning of the present, experiment was greatly stimulated by the wants of the British navy. During the colossal struggles of Great Britain with hosts of adversaries, the very existence of the nation appeared to be staked upon her fleets. The great prevalence of dry-rot in the timbers of British men-of-war assumed the proportions of a national calamity. It was said that a single 70-gun ship required for its construction the oak of 40 acres of forest, and that the supply would fail. It was in 1812 that Lukin tried, in the Woolwich Dockyard, his disastrous experiment with the injection of resinous vapors. More practical suggestions were soon forthcoming, and the use of the salts of various metals began to be recommended. Sir Humphrey Davy suggested corrosive sublimate; Thomas Wade (in 1815), the salts of copper, iron, and zinc. The opinion gained ground that poisons of various kinds were correctives to the decay of timber.

From the year 1768 up to the present time, the records of the Patent Office contain lists of almost every conceivable antiseptic, suitable or unsuitable, for the preservation of wood.

Progress during the Railway Era.—But it is since the birth and growth of the railway system that the antiseptic treatment of timber may be said to have received its most important development. The stone blocks and other solid supports, at first used for the permanent way of railways, were found to be too rigid, and had to be replaced by a more elastic material. The wooden sleepers which were substituted decayed so rapidly that some artificial method for prolonging their duration began to be considered as an engineering necessity. By the year 1838, four several systems of antiseptic treatment were fairly before the public, and competing for the favor of engineers. These were: Corrosive sublimate, introduced by Mr. J. H. Kyan; sulphate of copper, by Mr. J. J. Lloyd Margary; chloride of zinc, by Sir William Burnett; heavy oil of tar (afterwards called creosote), by Mr. John Bethell.

Corrosive Sublimate, or bi-chloride of mercury, was successfully used by Homberg, a French savant, in 1705, for preserving wood from insects. It was recommended by De Boissieu in 1767. In 1730 the Dutch Government tried it upon wood immersed in sea water as a remedy against the *Teredo navalis*, but for this purpose it failed. In the "Encyclopædia Britannica," in 1824, it is recorded that Sir Humphrey Davy recommends its use for timber. Kyan's first patent, for the employment of corrosive sublimate for wood-preserving was taken out in 1832. His first success was gained by the preservation of the woodwork of the Duke of Devonshire's conservatories. Kyanizing was for a long time by far the most popular of the timber-preserving processes in this country, and the name is to this day frequently applied erroneously to other systems. Used in sea-water, however, by the British Admiralty, this process turned out a failure, as it had done under similar circumstances with the Dutch government a century earlier. Kyanizing has met with a considerable amount of success in compara-

tively dry situations; but in water, and particularly in sea-water, it appears to have invariably failed, as have all the salts of metals. Corrosive sublimate is somewhat volatile at ordinary temperatures; it also has the drawback of producing injurious effects upon the workmen employed in handling it.

Sulphate of copper.—The use of this and of other salts of copper was recommended by De Boissieu and by Bordenave in 1767, and by Thomas Wade in 1815. In 1837 Mr. Margary took out a patent for the use of sulphate and ascetate of copper. Sulphate of copper has perhaps been the most successful of all the metallic salts as an antiseptic for timber. Applied in various ways it was popular in France long after it had been given up in this country. It is still in use in France, to a limited extent, for sleepers and telegraph poles.

Chloride of zinc.—This was recommended by Thomas Wade in 1815, and by Dr. Boucherie in 1837; and a patent for its application was taken out in this country by Sir William Burnett in 1838. The process of Burnettizing was at one time much patronized by the British Admiralty. For railway sleepers it was extensively adopted in France by the author's firm, principally on the railways from Orleans to Bordeaux, and from Caen to Cherbourg. It is no longer used in France, but it is still employed in Holland and in Germany. Chloride of zinc is a powerful antiseptic, but its weak point for wood-preserving consists in its extreme solubility in water.

Heavy oils of Tar, commonly called Creosote.—As early as 1756 attempts were made, both in England and America, as described by Knowles, to inject or impregnate timber with vegetable tars or with extracts therefrom. The first mention of the products of the distillation of gas-tar, to be used separately for impregnating timber, appears to be by Franz Moll. This inventor took out a patent in 1836 for injecting wood in closed iron vessels with the oils of coal-tar first in a state of vapor, and next with the heated oils in the ordinary liquid state. He recommended the adoption both of the oils lighter than water, and of the oils heavier than water, calling the former "Eupion," and the latter "Kreosot." He relied upon the Kreosot for its antiseptic qualities, but proposed to use the light oils separately, at the commencement of the operation, for the purpose of facilitating the absorption of the heavy oil. This plan has never been acted upon, as it would be obviously wasteful and unpractical to inject the lighter oils, or crude naphthas, which would immediately evaporate.

The practical introduction of the process is due to Mr. John Bethell. His now celebrated patent, which is dated July, 1838, does not mention the words "Creosote" or "Creosoting." It contains a list of no less than eighteen various substances, mixtures or solutions, oleaginous, bituminous, and of metallic salts. Amongst them is mentioned a mixture consisting of coal-tar thinned with from one-third to one-half of its quantity of dead oil distilled from coal-tar. This is the origin of the so-called Creosoting process. Creosote, correctly so called, is the product of the destructive distillation of wood, and coal-tar does not contain any of the true Creosote, which has never been used for timber-preserving. But a substance, since called carbolic acid, or phenol, had been discovered in coal-tar; it was thought by some to be identical with the Creosote of wood, hence the process came to be miscalled, after a time the creosoting process. It is in this popular sense only that the word Creosote is to be understood in the remainder of this paper. The two substances, Creosote and Carbolic acid, are described and contrasted, and their varying properties delineated in Dr. Tidy's "Handbook of Chemistry."

Competition of the Processes—Theory of Eremacausis.—In addition to the four processes already mentioned, a patent for a fifth was taken out by Mr. Charles Payne in 1846. His plan consisted in the injection into the timber, first of a solution of a sulphuret of barium or calcium, and next of a solution of sulphate of iron, the object being to form an insoluble sulphuret in the pores of the wood. This process was tried to some extent both in England and in France, but it was a complete failure, and is mentioned only by way of reference.

From 1838 to 1853, at which last date the paper of Mr. H. P. Burt was read at this institution, the four processes, Kyanizing, Margaryizing, Burnettizing and

Creosoting had been in active competition. The prevailing theory at that time as to the causes of the decay of timber was shaped by the opinions of the great chemist Liebig. Liebig taught that the processes of fermentation in certain fluids and of the putrefaction or decay of organized bodies, animal and vegetable, were caused by a species of slow combustion, to which he applied the term eremacausis. He held that this decomposition could be produced by contact with portions of other bodies already undergoing eremacausis. That it required for its ordinary development the presence of moisture and of atmospheric air; that its action was provoked by oxygen, and that its method of action was by a communication of motion from the atoms of the infecting ferment to the atoms of the body infected. He denied that fermentation, putrefaction and decomposition were caused by fungi, animalcules, parasites or infusoria, although these organisms might sometimes be present during the processes.

But he also stated that the phenomena of decomposition might be suspended by extreme heat or cold, that they were accelerated by the action of alkalies, and retarded by that of acids, and that they might be arrested by the use of certain antiseptics. If, however, the theory of eremacausis be accepted, and if its phenomena be due entirely to a communication of molecular motion, and not at all to the action of living germs, does any adequate explanation remain of the effects produced by antiseptics? With regard to timber, theorists were ready with an answer to this question, and they deduced their theories from further teachings of the great German chemist. Liebig, enlarging upon the views of previous investigators, had proclaimed the identity in composition of the animal and vegetable albumens. The blood of animals and the sap of plants are, during life, the circulating media of the vital growth; after death they are the portions of the respective bodies which putrefy most rapidly; both are largely composed of albumen. The sap freshly drawn from a tree will commence to putrefy within twenty-four hours. It was proclaimed (although probably not by Liebig), that the coagulation of the albumen was the true specific against the decay of wood. Corrosive sublimate, sulphate of copper, chloride of zinc, and the tar oils were all powerful agents for that purpose. It was claimed for all four of these processes that they coagulated the albumen contained in the wood, and that they formed insoluble compounds therein, thus arresting decay.

Prolonged experience has, however, proved that the salts of metals are not so permanent in their effects as the tar oils. The discussion which took place at this institution in January, 1853, upon the occasion of the reading of Mr. Burt's paper, was an interesting one, and was joined in by most of the leading engineers of the country. Whilst the other processes were admitted, in many instances, to have done good service, the Creosoting process was generally held, after fifteen years' experience, to have proved the most stable and reliable. In many subsequent discussions, the prolonged duration of creosoted timber had been a matter of constant and reiterated testimony. Gradually the Creosoting process took the place of the others by a species of "survival of the fittest," until in England it entirely extinguished its rivals. The author's last experience of Kyanizing in England was carried out in 1863.

In France, the Creosoting process was later in establishing itself, partly owing to the difficulty which at one time existed in procuring Creosote in that country, partly, also, to the popularity of the sulphate of copper process, enhanced, as it was by the ingenuity of the method employed for its injection by Dr. Boucherie. But it was discovered even in France, and notwithstanding the theories of insoluble compounds being formed in the timber, that the salts of metals were gradually washed out of the wood in moist situations. In 1861, the French chemist Payen reported that sulphate of copper could be almost entirely removed from wood by repeated washings with water, and in 1867 he reported that the whole could be so removed. This has been confirmed by the testimony of Maxime Paulet.

The experiments of Mr. Forestier, undertaken for the French Government, and the prolonged and exhaustive experiments of the Dutch Government, are conclusive as regards the efficiency of

creosoting against the ravages of the *Teredo navalis*, in cases where the timber has been efficiently prepared, and with a suitable kind of creosote. These experiments are referred to in the Minutes of Proceedings of this Institution, vol. xxvii. The experiments undertaken by Mr. Crepin on behalf of the Belgian Government, and the independent testimony of many of the leading engineers of this country, have also from time to time been brought to the notice of this Institution, in confirmation of the success of the Creosoting process against the ravages of marine insects. On the other hand, there are distinct and well authenticated instances of failure. An inquiry into the causes of such failures is one of the main objects of this paper.

Origin and properties of the Tar Oils.—As the tar oils gained in usefulness, their varying qualities became subjects of increasing interest. A brief digression may here be useful, in order to show the process of manufacture by which these tar oils are procured. It will be seen that from coal, as it is carbonized at the gasworks, four well-known products are obtained, viz., illuminating gas, ammoniacal liquor, coal-tar, and coke. Gas liquor, or ammoniacal liquor forms the basis of a separate industry; the ammoniacal products are of no utility for timber preserving. The antiseptic substances are all obtained from the distillation of coal tar, a black, viscous substance of a consistency resembling treacle. The tar is subjected to the heat of a furnace placed beneath the still, the operation being aided sometimes by the injection of steam, sometimes by the application of an exhausting air pump. The products of distillation come over very nearly in the order of their respective volatilities, those of lightest specific gravity being followed in succession by heavier and yet heavier ones as the heat increases. The temperature during the distillation ranges from 180° to 758° Fahrenheit. This preliminary process, although now carried out with more skill and economy than formerly, has not varied much during the last fifty years in its main object, which is to break up the tar into three groups of products, viz., oils lighter than water (crude naphthas); oils heavier than water, pitch, the residuum of distillation, which last product is run out from the bottom of the still, and solidifies, upon cooling, into a hard, black substance. It is in connection with the component parts of the two groups of oils, and their separate and subsequent treatment, that some of the best known and most brilliant discoveries of modern industrial chemistry have been developed. The oils lighter than water, however, have no part in the preservation of timber. It is not uncommon to hear inquiries as to whether the discovery of the aniline dyes has not, somehow or other, interfered with the quality of the Creosoting liquor. There exists a singular and unfounded prejudice on this subject. The materials for the aniline dyes are not, and never have been, taken from the Creosoting liquor or heavy oils; they are taken exclusively from the oils lighter than water, which last have never been employed for the Creosoting process, and are valueless for timber-preserving. The benzols, toluols, &c., from which the aniline dyes are produced, are extremely volatile, like alcohol.

The heavy oils of tar, or dead oils heavier than water, constitute the "Creosote" of the timber yards. They contain numerous substances, some of them liquid, some semi-solid, varying considerably in their properties, but most of them are now recognized as antiseptics. Formerly, the whole mass of these heavy oils was used for timber-preserving as they were collected from the still, but each portion can, if required, be separated as it comes over, according to its volatility, or the solid matters can be separated by filtering, for subsequent treatment.

It has been seen that Mr. Bethell's original patent recommended the use of the mother liquor, or coal tar, thinned with a portion of heavy coal-tar oil. So late as 1849, Bethell's licenses for the use of his patent described the process as "saturating timber with the oils obtained by the distillation of gas-tar, either alone or mixed with gas-tar." The author remembers how, in the early days of Creosoting, inspectors frequently refused to allow the thinner and lighter dead oils to be used without being thickened with tar. Tar, the mother liquor, necessarily included all the substances contained in the dead oils, plus the naphthas and the pitch. The reasons for not

adopting the tar in its entirety are simply that the crude naphthas are useless as antiseptics, and would immediately evaporate, whilst the pitch, from its too great solidity, would form an impediment to the injection. The dead oils, therefore, came into use alone, and there crept into some of the specifications the contradictory prescriptions that the wood was to be Creosoted according to Bethell's patent, but that the Creosote was to be free from adulteration with coal tar.

The dead oils made in London, and in all places where the tar is produced from the carbonization of the coal of the Newcastle district, are, as compared with other dead oils, the richest in semi-solid substances (naphthalene, anthracene, pyrene, &c.), and they require a higher temperature to volatilize. They are generally called "London oils." The dead oils of the Midland Districts are lighter, thinner, and more volatile, and contain usually a larger proportion of the ordinary tar acids. They are usually called "Country oils." The Scotch oils are, many of them, still lighter, thinner, and more volatile, sometimes lighter than water. Some Scotch oils, however, have been proved to be of excellent quality.

As regards the question of thick or thin oils, there is no doubt as to the opinion and practice of the earlier introducers of the Creosoting process. In January, 1853, Mr. Bethell stated that "the product of Newcastle coal contained a quantity of naphthalene, and that he was an advocate for its use." In November, 1864, he said that "the Creosoting process was not, as often described, a chemical process entirely"; that Creosote did coagulate albumen in the sap of the wood; "but that was not his only idea when he introduced the process: his object was to fill the pores of the wood with a bituminous asphaltic substance, which rendered it waterproof," &c.

The late Mr. H. P. Burt, whose labors in connection with the preservation of timber will be remembered by many of the elder members of the engineering profession, was in the habit, for many years, of using, by preference, the heavy London oils, mixed at times with a small percentage of the country oils, the latter as solvents or diluents of the more solid material. The author, whose connection with Mr. Burt commenced in 1850, remembers, among his first experiences of creosoting, the solid masses of naphthalene contained in the tanks before heating.

When the construction of railways commenced in India in 1850 and 1851, it was speedily discovered that the timber found in that country was subject to very rapid destruction by decay and by the attacks of insects. A serious difficulty was encountered by engineers in procuring suitable sleepers, and the experiment was tried of sending creosoted Baltic timber from this country. The first consignment of this material was sent out in December, 1851, for the East Indian Railway Company. The results were promising from the first, and the exportation of creosoted sleepers to India continually increased. The Minutes of Proceedings of this Institution contain numerous records of the rapid decay of unprepared timber in tropical climates, and also of the very great general success of creosoted timber exposed to the same influences, chequered, however, with a few instances of partial failure, which should be as instructive as the successes. It may be interesting to refer to the two papers by Mr. Bryce McMaster, upon Indian Permanent Way materials, one read in 1859, and the second in 1863, in which the success of creosoted timber in India is fully set forth. Mr. Juland Danvers, in his annual report to the Secretary of State for India for the year 1863, remarks that it is cheaper to send out creosoted Baltic sleepers than to use those of indigenous wood. The printed report of the East Indian Railway Company for the year 1867 again records the success of creosoted sleepers, after sixteen years' experience of their use.

It becomes a matter of interest to ascertain the kind of Creosote which was used for these earlier Indian sleepers. When the exportation first began there was a custom's duty upon the importation of Baltic timber into this country equal to about 20 per cent. on the value of the sleepers. The author's firm made early arrangements for creosoting in bond, and for this reason, and with trifling exceptions, all the sleepers sent abroad, although supplied by various contractors, were for many years creo-

soted at the works of the author's firm at Rotherhithe and at the Victoria docks. Their books contain accurate records of the origin of all the creosote used. As may be anticipated, by far the greater bulk was London oil, up to 1863 comparatively little country oil, and in some years none at all being used. In January, 1853, Mr. Burt, in describing to this Institution the process which he used, spoke, as a matter of course, of Creosote becoming a hard, compact mass at a temperature below 35° Fahrenheit. Ten years later, in February, 1863, speaking with reference to the Creosoting of some sleepers, the success of which in India had just been announced, he described Creosote as becoming solid at a temperature below 40° Fahrenheit, and added that, in consequence, he had introduced a heating apparatus inside the Creosoting cylinder.

With the exception of a small experimental shipment of larch and Scotch fir, all the sleepers sent to India have been of Baltic fir timber from the Polish and Russian ports. The shipments were of the ordinary kind of wood, such as was in use at first for sleepers in this country, and were mostly of triangular section. Amongst them, for the first three or four years, were considerable quantities of white-wood, a wood somewhat liable to split in hot countries. Subsequently, red-wood was stipulated for, and with good reason, in all Indian specifications. The quantity of Creosote injected into these sleepers was at first from 35 to 40 gallons to the load of 50 cubic feet, as compared with the 50 and 60 gallons of the present day. At present not only is a larger quantity of Creosote injected, but more care is also expended in the selection of the wood than was formerly the case. If, therefore, the earlier sleepers shipped to India behaved well, it might be assumed that the quality of the Creosote, at least, was suited to the climate. Such Creosote, however, as was then used would now be rejected under the requirements of many of the specifications at present in force for the preparation of timber for tropical countries. It is a question for grave consideration whether the change has been for the better.

It is a matter of notoriety, that for many years an increasing demand has arisen for the thinner and lighter Creosotes. "Country oil" became more popular, and began to be mentioned in specifications. Inspectors preferred these thinner oils; they were injected with less trouble, and the timber looked cleaner and less "muddy" after the process, especially in the winter, when the London oils are more solid. Contrary to the opinion of the introducers of Creosoting, the thin, light "Country oil" came to be considered by many as the supreme type of excellence.

This view was adopted by the late Dr. Letheby, who was further influenced by the growing recognition of the wonderful antiseptic powers of carbolic acid. Discovered in coal tar by Runge, a German chemist, in 1834, carbolic acid had gradually achieved the important position which it still holds as one of the most valuable of antiseptics for sanitary and surgical puposes. Carbolic acid in varying quantities was present in the tar oils; the other constituents of those oils were imperfectly understood; some of them, now well known, had not then been discovered. The success of the creosoting process was therefore by *a priori* reasoning attributed mainly, if not solely, by Dr. Letheby to the presence of the tar acids. In June, 1860, Dr. Letheby published his views on this subject in the "Journal of the Society of Arts." He considered carbolic acid to be the most effective constituent of the tar oils, and that the efficiency of the latter in preserving timber depended mainly upon the percentage of carbolic acid which they contained. He therefore concluded that the lighter portions of the dead oils were the best, viz., those portions distilling between 360° Fahrenheit and 490° Fahrenheit, as they contain the tar acids in greatest abundance. Naphthalene and para-naphthalene he desired to exclude as much as possible, as he held them to be of no value in the preparation of timber. He had found the proportion of carbolic acid in tar oils to range from 6 per cent. down to as low as 0.5 per cent. In a letter of his in the author's possession, dated 5th June, 1863, he alludes to two samples as containing "unusually large" proportions of tar acids; the quantities were respectively 6.4 per cent. and 10.1 per cent. In a lecture at Nottingham, in 1867, Dr. Letheby described

a specification which he had drawn up for an Indian railway. This specification, dated 1865, contains the following stipulations: The creosote is to have a specific gravity as near to 1,050 as possible, ranging from 1,045 to 1,055. It is not to deposit naphthalene or para-naphthalene at a temperature of 40° Fahrenheit. It is to contain 5 per cent. of crude carbolic and other coal-tar acids (by the caustic potash test). It is to yield 90 per cent. of liquid oil, when distilled from its boiling point to a temperature of 600° Fahrenheit.

From an examination of upwards of seventy timber-preserving specifications in the author's possession, ranging from 1849 to the present year, it is manifest that a new departure was thus inaugurated by Dr. Letheby. For the first time a boiling-point is fixed, a certain percentage of tar acids insisted upon, whilst the use of naphthalene and the heavier distillates is discouraged. This specification has long ceased to be used, but its stipulations have been copied, and in some cases carried to greater lengths, in more modern specifications, 10 per cent. of tar acids being occasionally required. Such specifications exclude the London oils if taken in their entirety as they come from the still. It is to be regretted that, at the period mentioned, there is no record of experiments having been made by any English chemists as to the actual effects produced upon timber by the various constituents of the tar oils taken separately. For want of such a test, it would appear that an important element in the question was for some years overlooked in this country.

So early as 1848 the French Académie des Sciences received a communication from De Gemini, detailing a series of experiments upon wood prepared with various antiseptics. This investigator endeavored to prove that timber cannot be permanently preserved by the use of antiseptics which are themselves soluble in water, and for that reason he preferred the use of heavy oils, or bituminous substances. The Academie rejected the conclusions of De Gemini, more especially as he denied that solutions of sulphate of copper formed insoluble compounds with woody fiber.

In 1862 Mr. Rottier presented a paper to the Academie Royale de Belgique giving the results of a number of experiments as to the effects upon timber of the various constituents of coal-tar oil. He arrived at the conclusion that although carbolic acid (L'Acide Phénique) was a very energetic antiseptic, yet that, owing to its volatility, the durable success of the Creosoting process was not due to its agency. He attributed that success to the heavier and less volatile portions which came over at the later periods of the distillation, and considered that the heavier they were the better.

Later on this investigation was taken up by Mr. Charles Coisne, who was then, and still is, an engineer in the service of the Belgian Government. In 1863 Mr. Coisne commenced a series of experiments, the object being to determine, in a practical manner, which portions of the tar oils best preserved the timber. The results were so instructive, that in 1866 he inaugurated a new series of experiments, still more carefully conducted, which lasted until 1870. He procured samples of Creosote from England, Scotland, Belgium and France. Four of these samples contained, respectively, 15 per cent., 15 per cent., 8 per cent., and 7 per cent. of tar acids by the usual test. The fifth was an oil of heavy specific gravity, specially prepared, and containing no tar acids. Yet this last sample produced better results than any of the others. Each sample was divided into portions. Wood shavings were saturated with these oils in the following different ways:

1st. With the Creosotes as received.

2d. With the Creosotes, supplemented by additional quantities of tar acids.

3d. With the Creosotes, supplemented by some of the heavier portions of the same oils distilling over at a temperature exceeding 320° Centigrade (628° Fahrenheit).

4th. With the original Creosotes divided into the lightest, the medium, and the heaviest portions, with each of which the shavings were separately saturated.

A putrefying pit (*pourrisoir*) was prepared, in which the shavings were placed on the 10th of November, 1866, together with other shavings not prepared. After four years' sojourn in the *pourrisoir*, they were removed and ex-

amined on the 16th November, 1870. The results were strikingly in favor of the heavier oils, and adverse to the tar acids, which last bodies appeared to have been wholly ineffective. The shavings which had been prepared with the lightest portions of the oils, although they had contained the largest portions of the tar acids, were, nevertheless, in the worst condition. Those prepared with the oils somewhat heavier were in most cases better preserved. Best of all were the shavings prepared with the heaviest oils, procured by distilling at the higher temperatures even when containing no tar acids; these last were all perfectly sound. The un-creosoted shavings were all rotten. Mr. Coisne believed that the best portions of the oils were the "green oils," distilling at high temperatures.

These experiments are recorded at length in the "Annals des Travaux Publiques de Belgique," also in separate pamphlets. Their results have considerably influenced the practice of railway engineers on the Continent. The Belgian Government accepted the conclusions arrived at by Mr. Coisne, and for many years has based its creosoting specifications thereon, with highly satisfactory results. The specification for the Belgian State Railways does not stipulate for any tar acids; it requires that at least two-thirds of the Creosote must have been obtained by distillation at a temperature exceeding 250° Centigrade (482° Fahrenheit), and the remainder at a temperature exceeding 200° Centigrade (392° Fahrenheit). It allows 30 per cent. of naphthalene, which is calculated at the ordinary temperature. In a recent correspondence with the author, Mr. Coisne, who has for more than twenty years superintended the Creosoting operations of the Belgian Government, confirms the results of those experiments by his subsequent experience.

So far, the experiments and the experience of De Gemini, Rottier and Coisne appear to be in absolute contradiction with the theory that the Creosoting process owes its success to the tar acids. Yet the fact cannot be doubted, that the tar acids are powerful antiseptics, and that their presence arrests decay. What, then, is the explanation of this apparent anomaly?

The authorities on the tar acids are many and reliable. From amongst the learned and voluminous treatises which have been written respecting these bodies, fifteen references have been made to authors in England, Scotland, France, Belgium, Germany and America. None of them disagree as to the following facts: That carbolic acid is volatile at ordinary temperatures. That it is soluble in water. That its combinations are not stable. That it is a powerful germicide, but that its efficacy ceases so soon as it evaporates or is washed out of the substances intended to be preserved. Professor (now Sir Joseph) Lister, whose adoption of the antiseptic system for surgical purposes has revolutionized hospital practice, speaks from his large and valuable experience as to the importance of carbolic acid in the treatment of wounds, but he also remarks that its volatility is sometimes an evil as well as a good. Dr. Sansom, whose recent work on antiseptics so ably epitomizes the results arrived at by previous investigators, as well as those due to his own researches, speaks of it as the "aerial disinfectant" *par excellence.*

If this substance can be washed out by water, and if its volatility is one of its great merits, and occasionally a defect, for sanitary purposes, can it at the same time be considered as a durable agent amongst the oils injected into railway sleepers? Especially can this be the case in those tropical countries where extreme heat or torrential rains, or alternations of both, are prevalent? For piles and other timbers used for harbor-work, the comparative solubility in water of the antiseptic agents employed is also a matter of vital importance. What is true respecting carbolic acid, will also apply, to a great extent, to cresylic acid, the last substance being, however, somewhat less volatile and less soluble than the former. Do these bodies become stable by entering into combination with woody fiber? Their instability in this connection is apparently pointed out by Mr. Coisne's experiments. It may, however, be objected that these experiments were not conducted under the conditions to which railway timbers are exposed. This point also has been very fully investigated.

In 1867 Mr. Coisne obtained some Creosoted sleepers which had success-

fully resisted decay during periods of from eighteen to twenty years. The wood was crushed, and the substances obtained therefrom tested. He found no tar acids; if they had ever been there, they were no longer present. He found, however, a quantity of naphthalene; also of an oil which did not commence to distil until 230° Centigrade (446° Fahrenheit).

In 1882 the author caused some similar experiments to be made. Through the kindness of the authorities of the London and North-Western Railway Company, eleven pieces of old Creosoted sleepers were sent from their permanent way. They had been in use for the following periods:

1	specimen	16	years.
1	"	17	"
2	"	20	"
2	"	22	"
1	"	28	"
2	"	29	"
1	"	30	"
1	"	32	"

Sleepers were also received from the Taff Vale Railway, the South-Eastern Railway, and the Great Eastern Railway, which had been in use periods varying from fourteen to twenty-three years. A portion was also taken from a Creosoted pale fence, which had been fixed in the Victoria docks in 1855, and which is still in place, perfectly sound and strong, after twenty-nine years' use. A careful analysis of these seventeen specimens, all of ordinary Baltic fir, gave the following results:

1st. In no cases were any tar acids detected by the ordinary tests.

2d. In fourteen out of the seventeen specimens the semi-solid constituents of the tar oils were present; in twelve of them was naphthalene, this body being in some cases in considerable quantity.

3d. Only small percentages remained of oils distilling below 450° Fahrenheit. In the majority of instances from 60 per cent. to 75 per cent. of the total bulk of substances retained in the wood did not distil until after a temperature of 600° Fahrenheit was reached.

It is clear, therefore, that these timbers had been preserved by the action of the heaviest and most solid portions of the tar oils, and that the other constituents had disappeared.

4th. In some of these specimens acridine was searched for and detected. This substance is one of the alkaloids or bases now known to exist in the Creosote oils. This is probably the first occasion upon which acridine has been publicly mentioned in connection with the injection of wood; but the author is persuaded that it will come to be recognized as one of the most valuable constituents of the tar oils for timber-preserving purposes. It was discovered by Graebe and Caro; it is a powerful germicide, and solidifies within the pores of the timber, from which it neither evaporates nor washes out. It is intensely acrid and pungent.

Portions of the same specimens of wood, fifteen in number, were sent to Mr. Greville Williams, whose original researches with relation to coal derivatives have been for so many years known to the scientific world. Mr. Greville Williams tested the samples of wood for tar acids, naphthalene, and the alkaloids. For the tar acids he found all ordinary tests fail, until he employed the extremely delicate one by bromine and ammonia. In some cases, even by this test, no phenols could be detected, but in most cases he succeeded in detecting faint traces of those bodies; generally less than one part in three thousand; minute portions, probably of the heaviest particles of the tar acids which had been incorporated and retained by the heavier portions of the oils. It is needless to say that these infinitesimal quantities could be of no practical value in preserving the wood. In all the specimens, save two, he found naphthalene. The presence of the antiseptic alkaloids was distinctly proved, and one of these bodies, called cryptidine, which he had discovered in Creosote oils, in 1856, was detected by him in one of the specimens. Mr. Greville Williams concludes that the preservative action of the Creosote oils is due more to the bases or alkaloids than to the tar acids, as the former remain after the latter have disappeared. These researches were published in the "Journal of Gas Lighting," and also in a pamphlet in the possession of this Institution.

First, and most volatile of all the Creosote oils, are the carbolic and cresylic acids, which are also freely soluble in water at ordinary temperatures; they

come over from the still, incorporated with the lightest portions of the oils. Pure carbolic acid would entirely disappear by evaporation, if not secured in a stoppered bottle. Next in order comes naphthalene, which is much less volatile than the tar acids. It is not soluble in cold water, and almost insoluble in boiling water. As it comes from the still it is of a yellowish color, and mixed with the heavy oils, it gradually becomes black on exposure to the atmosphere. It forms the principal constituent of the thick, muddy-looking substance which sometimes forms on the surface of Creosoted timber, and which may often be seen adhering to the ends of railway sleepers for several years after they have been placed in the line. When sublimed by the action of heat and a current of air, it forms the beautiful frost-like substance well known in Creosoting yards. It becomes quite solid at a low temperature, and in that condition would be an impediment to the injection of the timber—a difficulty removed by heating the oils to about 100° Fahrenheit, at which temperature naphthalene becomes liquid. After injection it solidifies, and greatly assists in filling up the pores of the wood.

The following simple experiments, which have been tried and repeated in many different ways at the author's laboratories during the last few years, are in strict accordance with the now well-known characteristics of naphthalene and the tar acids:

1. If tar acids and naphthalene be separately exposed either at the ordinary temperature, or at the tropical heat of 130° Fahrenheit, the tar acids will evaporate with much more considerable rapidity than naphthalene.

2. Injected into timber the same results follow.

3. Light, thin oils, containing large percentages of tar acids, evaporate more quickly than heavier oils containing less tar acids and more naphthalene, when tested by methods Nos. 1 and 2.

In weighing after these experiments great care must be taken to allow for the absorption of moisture from the atmosphere. The tar acids absorb moisture before finally evaporating. Wood also absorbs a large amount of moisture when injected with oils containing these acids.

4. By repeated washings with cold water, all the carbolic acid, and all or nearly all the cresylic acid, can be washed out, both from country and from London oils. These experiments assume especial importance in considering the durable effects of various kinds of creosote for protecting timber immersed in sea-water from the attacks of marine insects.

Dr. Meymott Tidy has published the results of his experiments upon naphthalene. He injected pieces of wood with this substance, and exposed them to a temperature of 150° Fahrenheit. He found that the evaporation was only superficial, and that it practically ceased after forty-eight hours, the naphthalene below the surface remaining within the pores of the wood. Naphthalene is now recognized as an antiseptic, not so powerful in its immediate effects as the tar acids, but more durable. It is probable that tar acids of a heavier and less volatile type than carbolic or cresylic acids, may be more reliable as antiseptics for preserving timber.

Following in the series of distillates, amongst the Creosote oils are the alkaloids or bases of the quinoline or leucoline group, amongst which chemists are searching, not without fair promise of success, for a febrifuge similar to, if not identical with, the quinine derived from the cinchona plant. In this group occurs the substance called cryptidine, already alluded to as one of the valuable antiseptics discovered in those portions of the oils which were formerly characterized as "inert."

Para-naphthalene, mentioned in Dr. Letheby's specification, has since then become the basis of one of the most interesting chemical discoveries of the age. It was excluded by Dr. Letheby from the oils intended for timber preserving, and is probably without value for that purpose. It is now called anthracene, and is extremely valuable as the substance from which alizarine is manufactured, thanks to the brilliant discoveries of Perkin in England, and of Graebe, Liebermann and Caro in Germany. Alizarine is the coloring matter used by Turkey-red dyers and printers; for ages it had been extracted from the madder root. It is now made from the coal-tar product anthracene, of a far higher degree of purity, and at an enormously decreased cost. The madder root has gone

almost entirely out of cultivation. The quantity of anthracene contained in tar is relatively small.

Amongst the green oils, distilling between 550° Fahrenheit and 750° Fahrenheit, is found the acridine already alluded to as a valuable germicide and stable antiseptic. Phenanthrene, carbazol, pyrene, chrysene and benzerythrene, by no means complete the list, which is constantly being added to by new discoveries of the numerous bodies in which these dead oils are so prolific. The properties of many of these heavier bodies are still imperfectly understood; but from the fact that they will not evaporate except at exceedingly high temperatures, they are valuable ingredients for timber-preserving.

By the light of the evidence now accumulated, it may be advisable to review the question as to the relative value of these various bodies contained in the heavy oils as regards the preservation of timber. Some of them are becoming valuable for other purposes. Which of them should the engineer retain for injecting wood?

Can the conclusion be resisted, that for this purpose the efficacy of the tar acids has been overrated, and this at the expense of the more stable and enduring portions of the tar oils? The London oils as they come from the still are not sufficiently volatile to meet the exigencies of some modern specifications, nor do they comply with these exigencies as regards the percentage of tar acids. They do not, as a rule, contain more than from 4 to 7 per cent. of tar acids, and they will not yield 90 per cent. of their bulk by distillation below 600° Fahrenheit. Therefore a pressure is put upon the manufacturer to meet the fashion by "taking out" some of the heavier portions, and in some instances this is done. By this means the bulk is rendered lighter, and the proportion of tar acids to the diminished bulk is increased. For these heavier portions, especially for the green oils, a market is found for lubricating and other purposes. But in the author's judgment the efficacy of the oils as antiseptics for wood is thereby diminished. The green oils, after the anthracene has been removed from them by filtration, should be returned to the Creosote tank. The percentage of tar acids to be used remains a contested matter of opinion. But the author ventures to express the hope that at least the lighter portions of the tar acids, and especially carbolic acid, may soon be relegated altogether to their important functions as sanitary antiseptics, for which they are so valuable, instead of being wasted by the attempt to use them as antiseptics for timber, for which their peculiar properties render them unreliable. Upon the whole it would be wiser to revert, to a larger extent and with increased knowledge, to the plan of using the London oils mixed with the country oils, and encouraging instead of discouraging the use of the heavier portions. The whole of the Creosote oils manufactured from ordinary gas tar in this country are required for preserving timber, and to exclude one considerable portion of the supply is to enhance unnecessarily the cost of the rest. No oils, however, should be used as Creosotes which are lighter than water. Both bone oil and shale oil are sometimes offered as Creosote oils.

In 1881 Professor (now Sir Frederick) Abel and Dr. Tidy drew up a joint Creosoting specification, in which, as the result of direct experiment, they resolved to exclude no semi-solid bodies which completely melt at 100° Fahrenheit. They further changed the standard of volatility from 90 per cent. at 600° Fahrenheit to 75 per cent. Subsequent and prolonged investigation induced Dr. Tidy to go still further in the same direction, and not only to withdraw the clause limiting to 25 per cent. the oils distilling at a higher point than 600° Fahrenheit, but even to require that at least 25 per cent. of those non-volatile oils must be present. The author's experience leads him entirely to agree with the progress made in this direction.

CONFLICTING THEORIES ON PUTREFACTION—THE GERM THEORY.

If experiment and experience should lead to clearer views as to the relative value of various antiseptics, it may be advisable to test those views by reference to the recent development of theory upon the causes of decomposition in organized bodies. How do antiseptics act upon timber? Is the coagulation of albumen a sufficient explanation of their

preservative action? Surely not. Many substances, boiling water included, which will effectually coagulate albumen, will not prevent the decay of wood. Coagulation retards, but does not prevent, the decay of albumen itself. Again, the quantity of albumen in fir timber is exceedingly small, if the tree be cut down, as it generally is and always should be, during the season when the sap is not circulating. From a number of experiments made upon ordinary fir sleepers, the author arrives at the conclusion, that the quantity of nitrogenous matter or albumen which they contain does not usually much exceed 1 per cent. of their weight. Any watery fluid containing from 2 to 3 per cent. of tar acids would effectually coagulate this quantity of albumen. In some cases it is found that a portion of this albumen is actually coagulated by substances naturally contained in the timber. But the coagulation does not of itself preserve the wood. Leibig's theory of decomposition has already been alluded to. He maintained that putrefaction was due to eremacausis or slow combustion, produced by contagion, the infected bodies communicating a molecular motion to the atoms of the bodies with which they come in contact, and that these phenomena are not caused by the action of germs or living organisms.

The modern germ theory distinctly traverses this last assertion. Pasteur affirms, that without the presence of living germs, the phenomena of organic decomposition do not accomplish themselves, and that these germs are the veritable agents of the decomposition. The laborious experiments, and the lucid deductions of Professor Tyndall confirm the experiments and theories of Pasteur. Professor Tyndall explains that the air is laden with clouds of germs, agents of decomposition, ever ready to settle down and develop upon matter suitable to their growth. He finds that the contents of tubes filled with the most putrescible materials, animal or vegetable, can be preserved from putrefaction indefinitely, by the exclusion of germs. But that it is not sufficient merely to poison or neutralize one generation of organisms, the incursions of fresh myriads must be excluded, or putrefaction will ensue.

After reading the "Essays on the Floating Matter of the Air," in which Professor Tyndall describes how the germs gradually fell into the open tops of the test-tubes, let the comparison be made between the mouths of these tubes and the gaping orifice of a crack produced by the sun in a piece of timber. Through it the germs will descend, and if there is nothing to arrest their action, and if the crack is deeper than the portion of the wood charged with antiseptics, they will carry destruction into the center of the log. But if the antiseptic be of an oily or bituminous nature, it will flow into the cracks when they first develop themselves, and seal up the orifices against the enemy. Examine a crack or a wound in the trunk of a living fir tree; it will be found that by a natural process, a resinous substance exudes, which closes the wound against the agents of destruction.

The bodies of mammoths preserved in ice through countless ages, the trees of primeval forests excluded from the air beneath thick deposits of peat, the fragments of wooden piles which have endured undecayed for centuries when driven deeply below the surface of water, all confirm the experiments of Pasteur and Tyndall, and prove that the exclusion of germs prevents putrefaction. Specimens are exhibited of a wooden pile from the remains of the bridge (destroyed by fire) which was constructed by Charlemagne across the Rhine at Mayence; of pieces of piles from the foundations of the bridge across the Medway at Rochester, which was destroyed by Simon de Montfort in 1264, and which was probably then about one hundred years old; also from the new bridge erected to replace the former one in 1283.

It is not for the author to draw the dividing line between the decomposing action of germs and the action of oxidation. It is sufficient for his purpose to submit that all influences which either destroy or exclude germs, will prevent decay so long as those influences endure; but that permanent effects must not be relied upon from agents which are not themselves permanent and abiding. The germ theory then becomes a severe but a salutary test in choosing antiseptics for the treatment of timber.

Such treatment is of little value unless its effects will endure for long periods. Reliance, therefore, must not be placed upon those germicides, however potent, which will readily volatilize in air, or dissolve in water. A growing skepticism arises from experience as to insoluble compounds being formed between woody fiber and substances which are themselves soluble in water. In short, the substances to be employed should by preference be antiseptics in a double sense; they should be both germicides and germ excluders. From the long list of germicides must be especially excluded such as injure or weaken the fiber of the wood; amongst these latter must be classed all solutions with very strong acid or alkaline reactions; also some of the metallic salts. It has been seen, that the salts of zinc, mercury, and copper have been to some extent successful; of these the author's experience induces him to prefer sulphate of copper, as less soluble in water than chloride of zinc, and not volatile like corrosive sublimate. Even sulphate of copper cannot be permanently relied upon, when exposed to the continuous action of water; but it may be found useful in comparatively dry situations, or as a protection against dry-rot to timber under cover. From its properties as a germicide, sulphate of copper might be usefully employed in conjunction with oily or bituminous fluids, even with oils which do not possess great potency as germicides.

From all research and experience it would, however, appear that the same conclusions may be derived, viz., that the best antiseptics for timber are to be found amongst oils and bitumens which fill up the pores of the wood. Of such bodies, those which contain germicides are to be preferred. And, other properties being equal, those which either solidify in the pores of the wood or which require an extremely high temperature to volatilize them, and which are insoluble in water, must surely be the best of all.

Apparatus for Timber-Preserving.— Of the apparatus employed for applying antiseptics to wood, the most ancient and the most popular is the tar brush or the paint brush. During the last century, and in the earlier portion of the present, steeping in tanks was extensively adopted, the various liquids being employed either cold or heated. A marked improvement was introduced in 1831 by Mr. Breant, a director of the Mint of Paris, who invented the first apparatus for injecting timber by means of vacuum and pressure, in a closed iron cylinder; he employed, by preference, linseed oil and resin. The cylinder was fixed vertically, an inconvenient arrangement not necessary to the efficiency of his process. The iron cylinder and the process by vacuum and pressure were adopted by Mr. Bethell, and greatly improved by him and by Mr. H. P. Burt, who were associated together for some years. The cylinder was enlarged, its fittings strengthened and simplified, and an interior heating apparatus added. In Mr. Burt's paper of January, 1853, there is a full description of this machinery; its main features are still the same in the usual Creosoting apparatus of the present day. These cylinders, being of wrought-iron, were applicable to Creosote oils and to chloride of zinc, but not to salts having a corrosive action upon iron, such as sulphate of copper and corrosive sublimate. In 1842, Mr. Timperley described to this Institution a method which he had adopted on the Hull and Selby Railway, for lining the iron cylinder, in order to preserve it from the action of corrosive sublimate. This method and the expedient of smearing the inner surface with pitch were proposed and tried for sulphate of copper injections with but partial success. The author had several cylinders materially injured or destroyed by the corrosive action of these salts. In 1857 Messrs. Lege and Fleury Peronnet introduced an apparatus of which the cylinder, trucks and pressure pumps were entirely of copper, and machinery of this costly description is still used for the Compagnie des Chemins de Fer du Midi, at Labouheyre. In 1865 the author took out a patent for the following apparatus: Inside the iron cylinder he placed a wooden tank, which contained the timber to be operated upon, and in which was the sulphate of copper solution. It was an open wooden tank, inside a closed iron cylinder. The pressure applied was that of condensed air, a condensing air-pump being used, capable of maintaining an effective pressure of 200 lbs. to

the square inch. By this means the timber was injected with the copper solution without injury to the iron cylinder.

The process of Dr. Boucherie was at one time largely used in France. It consisted in the injection of newly-fallen timber in the forest by the vertical pressure of a column of the antiseptic solution, generally sulphate of copper, which was conducted through a pipe from a small reservoir fixed at a height of 30 or 40 feet. The tube was attached by an ingenious arrangement to the end or middle of the log; the antiseptic liquid expelled the sap from the softer parts of the timber, and took its place. The process is still used to a small extent in France, principally for telegraph poles.

Various attempts have been made to imbue timber with the vapors of oils, either by employing the tensions of the vapors themselves, or by the use of the pressure-pump. The first experiment of this kind appears to have been made by Lukin, in the dockyard at Woolwich in 1812, when the apparatus exploded, with fatal consequences to the workmen employed, and the attempt was abandoned. The patents of Franz Moll in 1836, of Bethell in 1864, and other subsequent patents, claim the invention of the principle of injecting Creosote oils in a state of vapor. If this could be conveniently or safely carried out, the system might possess some advantages. But there is a fatal objection to its employment. Timber is weakened by exposure to a temperature much exceeding 250° Fahrenheit, whilst at 300° Fahrenheit, or a little above, it commences to decompose, and becomes seriously injured. Now the boiling point of the Creosote oils ranges from a little below 400° Fahrenheit up to 760° Fahrenheit. As with the steam of water, so is it with the vapor of oils—no pressure can be obtained with them, except at a temperature exceeding their boiling point. The vapors of the Creosote oils cannot, therefore, be injected into timber except at temperatures, and under conditions of pressure, which would destroy the value of the timber as an engineering material. The process has been tried in France, and it failed, owing to the complete deterioration of the timber.

A modification of this system has, however, been carried into practice. Super-heated steam was passed through Creosote oils, and then injected into the sleepers (which had been previously warmed by steam) with the idea that the mingled vapors of water and Creosote might be injected into the timber at a temperature of from 290° Fahrenheit to 320° Fahrenheit. With this modified process, the author's firm carried out some extensive operations for the Compagnie des Chemins de Fer de l'Ouest, it being the desire of the engineers of that company to economize the Creosote, and to try whether in a finely divided state, a smaller quantity might not suffice by being more deeply injected. The operation was supplemented, however, by an injection of Creosote in the usual fluid state.

After prolonged trials, the first part of the operation was discontinued by order of Mr. Bouissou, the company's engineer of the permanent way. It was found, whenever the cylinder was opened before the second operation, that a small portion of the lightest particles of the Creosote had been carried over mechanically into the cylinder by the superheated steam. Once within the cylinder, however, the two fluids obeyed the laws which govern their respective volatilities: the Creosote oil sank to the bottom of the cylinder, and the vapor of water only was injected into the timber. The sleepers on examination and testing by the ordinary tests, contained neither tar oils nor tar acids.

An analogous experiment tried at the Timber Preserving Works of the Austrian North-West Railway, is described in the journal of the Architects and Engineers' Institute of the Kingdom of Bohemia for 1880, by Herr J. Seidl, and the process has been condemned for very similar reasons.

Condition of Timber at Time of Preparation.—Getting Rid of Moisture by Stacking or Artificially.—The hygrometric condition of timber at the time of injection is an important element in the success of the operation, all important with the Creosoting process especially. Neglect on this point has often been the cause of partial or total failure. Woody fiber in itself is heavier than water, its specific gravity being generally considered as equal to 1.5, water being 1.0. It is, therefore, owing

to the looseness of their texture, that so many kinds of timber are lighter than water. The specific gravity of fir timber varies ordinarily between 0.5 and 0.8; the difference arising as often from the varying density of the timber itself, as from the quantity of water contained. As fir timber can, under certain conditions, absorb so much moisture as to become water-logged, or actually heavier than water, its powers of absorption can be calculated from its specific gravity. It can take up as much as from 60 to 150 gallons of water to the load of 50 cubic feet, the maximum quantity being, of course, an exceptional possibility. Fir and pine, however, frequently contain as much as from 15 per cent. to 20 per cent. of water, after from two to three years' stacking. The question of the pernicious effects of an excess of moisture in the timber at the time of Creosoting, has been from time to time brought before this Institution by Mr. Bethell, by Mr Burt and by the author. Large logs taken out of timber ponds, or sleepers freshly imported, are not in most cases in a fit condition for Creosoting until after having been stacked for from four to six months. The author, in common with most of the earlier operators in this process, has tried various methods for artificially drying the timber. Steam, ordinary and super-heated, currents of hot air, and drying stoves or ovens, have been used for this purpose, but have all, in this country, been abandoned. To subject timber to a dry heat, elevated enough to remove its moisture with the necessary rapidity, will invariably result in injury to the wood. Timber piles stoved before Creosoting, prove brittle when driven. The action of the air-pump in the ordinary process assists the operation by withdrawing air from the pores of the wood; but it is a mistake to suppose that it has much effect in withdrawing moisture.

These difficulties have perplexed the author for many years. He has recently devised a method by which to get rid of the moisture as part of the timber-preserving process, and without injury to the wood. An experiment easy to reproduce, and which explains the nature of this operation, is made as follows: An ordinary glass flask, in which are placed some pieces of wood saturated with water is connected by glass tubes with an experimental air pump. By working the pump the air is extracted from the pores of the timber, but however efficient the vacuum may be, no perceptible moisture is withdrawn, nor would the water be removed from the wood except by a slow evaporation prolonged beyond practical limits. This represents the ordinary action of the air pump upon timber in a Creosoting cylinder. If sufficient heat be now applied beneath the flask, the water will become volatilized, and will be withdrawn rapidly in the shape of steam by the action of the air pump. But the wood will be found to crack, and open to an extent which is not desirable. This illustrates the result of applying dry heat.

Now take a similar flask with a condensing apparatus added; moreover, the flask should contain Creosote oil, in which the wet timber is submerged. It must be constantly borne in mind that at the ordinary tension of the atmosphere, the boiling point of the Creosote oils ranges from about 380° Fahrenheit to 760° Fahrenheit, as compared to water at 212° Fahrenheit. These boiling points are, however, lowered, according to a well-known law, by the effects of a vacuum. Let the Creosote in the flask be now heated to 212° Fahrenheit, whilst the air pump is put into operation. The heat being communicated through an oily medium will not injure the timber, from which the water is volatilized, and drawn out by the air pump. The Creosote oils are not volatilized, as the temperature is far below their point of ebullition. The water is speedily and effectually removed, and the Creosote takes its place.

By the ordinary and well-known process, after the timber has been placed in the cylinder and the air-tight door closed, the air is exhausted from the cylinder, the Creosote is then introduced heated to a temperature of from 100° Fahrenheit to 120° Fahrenheit, when the air pump ceases to work, and the pressure pump is put into operation.

Referring now to the new process, it will be seen that a large dome is placed on the top of the cylinder, to which the exhaust-pipe of the air pump is attached. The exhausting process is continued after the Creosote has been introduced into the cylinder. The Creosote during

this part of the operation should not be allowed to rise quite to the top of the vessel, a free space being preserved, and the dome kept empty, so that the Creosote is not drawn through the exhaust pipe. The Creosote is raised to a temperature a little exceeding 212° Fahrenheit instead of 120° Fahrenheit as heretofore. The exhausting process is continued until all the water is extracted from the timber in the form of vapor, drawn through the dome, condensed by passing through the worm of the condensing apparatus, and collected in the receiving tank, where the quantity extracted can be measured. With charges of very wet sleepers, the author has succeeded in withdrawing water equal in volume to 50 gallons per load of timber, and replacing this water with an equal volume of Creosote by the action of the air pump alone. If necessary, however, the pressure pump can be afterwards applied in the usual way.

A slight additional cost, and a few hours' additional time are necessary for dealing with very wet timber by this process as compared with the ordinary method. But the expenditure in time and money is not so great as would be required by stoving the wood before Creosoting. If, in the absence of artificial methods, timber be stacked for six months, as it should be, the interest on capital represents a certain expenditure also. The author ventures to suggest that this is not always taken sufficiently into account, in giving out contracts for creosoted timber. Other conditions being equal, dry timber is at a disadvantage in the competition, as far as price is concerned, with timber just landed. Yet a small extra expenditure in this particular would frequently be repaid to the consumer twenty or thirty-fold in the prolonged duration of the wood.

Conclusion.—In conclusion the author would remark that with regard to certain points mentioned in this paper, upon which some controversy has at times arisen, he has been careful to advance no opinion which he has not confirmed, either by the opinions and investigations of eminent authorities, or by careful and reiterated experiments. Many hundreds of experiments have been in fact carried out at the laboratories of the author's firm at Silvertown during the last five years, with the especial object of investigating the properties of the tar oils and other antiseptics, and their behavior in contact with timber. To Mr. Royle, Mr. Bendix, and Mr. Holmes of the chemical staff of his Silvertown Works he has to return his best thanks for their skilled assistance, and particularly to Mr. Bendix, who has been more especially entrusted with the conduct of these experiments. To Mr. Gabbett he is indebted for the drawings exhibited.

The Treatment of Timber by Antiseptic Methods has been acknowledged by some of the greatest engineers of this country to have been useful to the art of constructive engineering. It may be made even more useful in the future than it has been in the past. All that the advocates for its still more extended development can desire to claim will be, that their methods and investigations may be seriously examined, and from time to time decided upon, in accordance with the results which science and experience may bring to light.

THE CLYNDOGRAPH.—The clyndograph of M. Moessard is a new panoramic photographic apparatus, which by a simple rotation of the objective gives the cylindrical perspective of the earth. A view furnished by the apparatus embraces an angle of 170°, so that a complete turn of the horizon is obtained in two views and a fraction of 20° range. The instrument is based on the principle that a lens or combination of lenses, constituting a photographic objective, may be subjected to any movement whatever without the image it produces on a screen changing its form or position, provided that the movement takes place around the nodal point behind, which is maintained immovable. This follows from the known property of the nodal point being the point of view of the perspective produced. Suppose, then, there be (1) an objective suspended horizontally and turning round a vertical axis passing by its after nodal point; (2) two vertical shutters fixed behind to right and left of the objective, to limit the field in the horizontal direction and arrest rays too oblique; (3) a screen, of cylindrical form vertically centered upon the axis of rotation, and having for radius the distance of the nodal point from the principal focus of the objective. In any position whatever of the objective the lie of the country comprised in the field of the instrument will be projected on the screen. If the objective be put in motion one gets successively for each point of the panorama an immovable image which impresses the eye or sensitive paper whilst the point remains between two shutters. In M. Moessard's actual apparatus Thiebaut sensitive plates are used to receive impressions.

THE PROPERTIES OF MALLEABLE IRON, DEDUCED FROM ITS MICROSCOPIC STRUCTURE.*

From "Iron."

Although there can be no doubt that the chemical and physical properties of iron are closely connected, the one cannot, however, be directly deduced from a knowledge of the other. This deduction may be performed with the most certainty in the case of pig or cast iron, which possesses but a low degree of toughness, while the difficulty increases with the decrease in the percentage of carbon, and the increase in tensile strength and ductility. In the case of malleable irons (steel and iron, ingot and weld steel and iron), previous experiences have shown that no basis exists on which the connection between chemical and physical properties may be determined with even the slightest degree of certainty. Even the attempts to determine, from their chemical composition, how rails will behave during use, have been entirely unsuccessful. Indeed, rails which are made by the same metallurgical process from the same material, and of the same chemical composition, do not present the same properties, although they are throughout apparently homogeneous. This is even more the case when any of the different varieties of iron occur together—as, for example, in the case of compound armorplates; or when similar varieties of iron are produced by different processes—for example, soft forge pig iron, open-hearth iron, and Bessemer iron from acid or basic converters. All these varieties of iron can have a perfectly identical chemical composition, and yet behave entirely differently on working and in use; and also, after quite similar treatment by hammering, rolling, &c., behave in an entirely different manner from that expected when submitted to tension, pressure and percussion.

Impossible as it is to say from the chemical composition found by analysis what the physical properties are, it is equally impossible to deduce the chemical composition from the physical properties, for example, from the tensile strength, elongation, and contraction obtained on testing. Frequently, too, the results of the tests of tensile strength, elongation and contraction are not even sufficient to explain the behavior in use. It has frequently been observed that rails of ingot iron possess the peculiarity of unexpectedly breaking on being suddenly cooled, or on being exposed to a very low temperature; while rails of weld iron, possessing chemically the same composition, and mechanically the same tensile strength, elongation and contraction, or even when they have given more unfavorable results, remain unaltered. The inconvenience of a sudden fracture of an apparently perfectly sound ingot-iron main shaft of a screw steamer, has in many cases led to the preference being given to an intrinsically inferior one made of weld iron.

In order to explain these contradictions, and to fill up the apparent gaps in the scientific metallurgy of iron, recourse must be had to the microscope, which reveals properties that cannot be discovered either by an analysis or by mechanical tests. The investigations conducted by me, which I here present by the kind invitation of the President of the Iron and Steel Institute, my friend Dr. John Percy, have not in any way exhausted the subject. They are but the commencement of the path into a wider field of research, and are intended to serve merely as an inducement for followers in the same path. Microscopic investigations of iron have long been made, but only systematically in a few cases. The most complete are the investigations of pig iron, especially the researches of the present Manager of the Royal Prussian Mechanical Testing Institute, Mr. A. Martens. He has devised a very satisfactory method of distinguishing the separate constituents of a piece of iron by etching and tempering. He has also, at my request, prepared for the Royal Mining Academy of Berlin a collection of 120 sections, upon which the following investigations are based.

* A paper read before the Iron and Steel Institute by Dr. Wedding.

Besides these, several test pieces from the well-filled museum of that institution were also examined. The sections are prepared in the following manner: The small test pieces, obtained from the main iron mass by breaking, planing, filing or crushing, are first ground in a grinding machine with a coarse emery wheel, and are then evenly and finely ground upon cast-iron plates on which emery is spread. Coarse emery is first employed, and is replaced by finer and finer emery as the grinding proceeds. The pieces of iron are then fastened with a cement of resin and wax to a thick piece of looking-glass. In order to guard againt the removal of the cement during the grinding by becoming hot, water is added. The polishing is then effected by hand, with polishing agents washed as carefully as possible, such as ferric oxide, putty powder, tripoli, &c. The polished section is then etched with very dilute acid; for this purpose, platinum chloride, nitric acid, hydrochloric acid, acetic acid or salicylic acid is employed. A mixture of tincture of galls and acetic acid is also used. After the etching, the section is carefully heated, whereupon the portions attacked acquire varying tints, mostly golden yellow, purple, red, violet, or dark blue. A subsequent faint gilding has also been employed by Martens. It must be borne in mind that it is not the colors that are characteristic, but their differences. In each section of my paper I give the reasons for the conclusions which I have drawn from my investigations—reasons which, with proper judgment, may be easily deduced by any observer who wishes to continue the investigations on the same basis.

FORMATION OF GRAINS AND FIBERS.

Iron, in a pure state, as well as in combination and mixture with the amounts of amorphous carbon, silicon, phosphorus or sulphur which occur in a wrought iron of technical value, and with a small percentage of manganese, crystallizes in the regular system. When it is possible for crystals to form freely in cavities, the crystals present an octahedral form, resembling a pine tree, with an embodied development of the crystal axes. In the compact iron mass, on the other hand, the individual crystals do not become complete, but press one another, and form grains which are, for the most part, bounded by pentagonal planes. Evidence of this is afforded by each section of a test-piece of iron that has been allowed to cool quietly, and without being disturbed by external pressure, from the fluid or viscous state of aggregation, but is equally obtained in iron produced by an oxidizing process from pig iron, or by remelting. The size of these grains, which, as they belong to the regular crystal system, appear, when the piece of iron is broken, to be on all sides of the same form, and of the same size, is dependent upon two circumstances—firstly, on the rapidity of the cooling; secondly, on the nature and amount of the other elements, either admixed or chemically combined with the pure iron. In the case of malleable irons, the presence of graphite ought, as a rule, not to be taken into account; it is present more especially in the varieties of steel rich in carbon, but in the malleable irons in practical use it occurs only exceptionally in such amounts as to interfere with or influence the formation of grains, as always in the case of gray pig iron. Other things being equal, the size of the grains increases in proportion to the slowness of cooling. On the other hand, the size of the grains decreases, with the same kind of cooling, with the proportion of carbon up to 2 per cent. Above this amount, when the percentage of carbon rises or falls, the grains increase in size. Silicon, sulphur, and small amounts of manganese, titanium, chromium, and tungsten, favor the smallness of the crystals, whilst phosphorus increases their size. Evidence of this is best afforded by pieces of crucible cast steel containing various amounts of carbon, or containing the same amount of carbon and varying amounts of the other substances mentioned. In this case the microscope is hardly necessary, for even the naked eye can detect the truth. In tungsten steel, containing 2 per cent. of carbon, an almost amorphous fracture is exhibited.

The only element that changes the regular crystal form of iron containing carbon into a rhombic, or, as the end faces have not with certainty been determined as in right angles, into, at any rate, a crystal form not belonging to the regular system, is manganese in consid-

erable quantities. It is not astonishing that inconsiderable amounts of manganese effect no change. An analogous phenomenon is frequently observed in the crystallization of salts from aqueous solutions, in which small quantities of other crystallizing substances effect either no change, or but an inconsiderable one, in the form of salt crystals. As soon as manganese occurs in large quantities, from 2 per cent., according to my investigations, the regular crystal form of the iron is changed. This may easily be seen in the case of pig iron; from granular iron a radiated white iron is obtained; with a larger percentage of manganese, foliated spiegeleisen; and with a still higher percentage of manganese, columnar ferromanganese is got. Malleable iron with more than two per cent. of manganese occurs only in unsuccessful Bessemer castings. Under the microscope, too, it is difficult to detect the influence of manganese, as the crystal grains, described later on, appear to acquire only, when the percentage of manganese increases, a long columnar form. At all events, these influences may be better detected, as also in the case of pig iron, with a larger field of vision under a magnifying glass than with the microscope. Each individual grain in malleable iron is ductile. The malleability of the entire piece of iron depends upon the ductility of the separate grains.

If pressure is exerted on an individual grain in the direction of but one axis, as occurs, for example, when a piece of iron is hammered on an anvil, there is formed from the round, or, more correctly speaking, many-sided grain, bounded by pentagonal planes, a plate technically known as "scale" (Schuppe). If, on the other hand, the pressure acts in the direction of two axes, either at the same time, as in the case of rolling with a diagonal groove, or at different times, as in the case of hammering an ingot or rolling a bar, turning it round 90° after every passage through the rolls, the grain is converted into a column, which belongs apparently to the tetragonal system, and which is, in practice, termed a "fiber' (Sehne). Fibers are thus elongated grains. Confirmation of this assertion may be obtained under the microscope with sections cut from rough and worked dieces of iron, partly parallel, partly at right angles to the plane of pressure. The phenomena are, for reasons that are explained below, most distinct in the case of quite soft weld iron rich in slag. In sections parallel to the fibers the separate columns may be very clearly followed, while in sections at right angles to the fibers no elongation is noticeable. This fact explains why a fracture at right angles to the fibers appears granular to the naked eye. A fiber cannot, however, extend in any possible length without again breaking up when given conditions are brought into play. The percentage of carbon has the greatest influence on this phenomenon. If the percentage reaches or exceeds 0.5 the fibers split up into grains, even with slight stretching. The same effect is produced also with a low percentage of carbon by a very small amount of phosphorus, a large amount of silicon, or a not inconsiderable amount of sulphur.

Under such circumstances the fibers, on being stretched, split up into grains, which must always be smaller than the grains from which they originated. This phenomenon is better shown under the magnifying glass than under the microscope, as the field of the latter is not large enough to show several grains at the same time if they are not very minute. The fact that steels rich in carbon, and finely granular iron (Feinkorneisen), form no fibers, is well known in practice. The formation of grains by phosphorus is so characteristic, that it is employed for detecting very small amounts of this element in basic Bessemer iron. Moreover, this fact shows that from a fibrous iron a coarsely granular iron cannot be formed by any influence, with the exception of elevated temperature. The theory of a conversion of this kind by means of faint concussions must consequently be relegated to the domain of fable. The fracture of a fibrous iron can only exhibit a grain equal to the section of the fibers, or a finer grain when, through concussion, an elongation has been effected. This is confirmed, I may add, by the experiments of Wobler and Spangenberg.

CONDITIONS FOR THE FORMATION OF FIBERS IN IRONS POOR IN CARBON.

Although iron poor in carbon is alone adapted for the formation of fibers, yet every iron poor in carbon, when com-

pressed in the direction of two axes, does not form fibers. It is a known fact that weld iron, during the rolling process, very easily forms fibers, but that ingot iron very rarely does so. The reasons for this remarkable phenomenon are also explained by means of the microscope. Microscopic examination of sections of fibrous iron parallel to the direction of the fibers, shows that the individual fibers form wires which lie parallel to one another. But they never, even in the case of the softest weld iron, have a very long extension, but always give place to new fiber heads, which rarely lie in the same direction, being generally more or less displaced, though always parallel. From this it may be concluded that the strength of fibrous iron depends on the fact that, like the individual hemp fibers in a rope, the fibers lie with their ends in various sections. The microscope shows further that none of these wires or fibers is directly connected with its neighbors, either in a lateral or longitudinal direction. In fact, each fiber may, by careful etching, be picked out like those of a muscle of the human body. On examining into the cause of the separation of a fiber from those immediately surrounding it, a separating intermediate layer is distinctly observed. This intermediate layer is composed of slag or iron scale (Fe_3O_4). These intermediate layers accompany the fibers, in every case, as far as their ends, and there surround the fiber heads as a very fine envelope, either joining the following fibers, or undergoing a short disconnection. In the latter case a granular structure immediately occurs at the point in question, that is to say, an agglomerate of crystals may be seen, which are shifted towards one another and intimately entangled.

This latter phenomenon is so frequently the rule that, with a small field and a high power, and with a section parallel to the fibers, the observer imagines that he is looking at a granular iron, while with even a slight shifting of the object in a longitudinal direction, the elongated crystals again appear as fibers. Just as this phenomenon of grain formation between the fibers only occurs when no separating slag envelope is present, in the same way in the formation of fibers the slag envelope is never wanting. From this it must be concluded that the formation of fibers does not take place without the admixture of slag. Proof of this is afforded by the results obtained with the Bessemer process on a small scale at Avesta, in Sweden, where a perfectly fibrous ingot iron was produced in the Bessemer process by an intentional admixture of slag. Further proof is afforded by the tests to determine whether the iron, in the basic process, is free from phosphorus. The iron, which is ladled out for the test, is mixed with slag, and consequently, when under the hammer, the formation of fibers is effected, which gives a silk-like structure to pure iron, in contradistinction to iron containing phosphorus, which remains crystalline. By this means it may very easily be seen whether the iron is free from phosphorus. Moreover, the microscope shows that the slag portions in fibrous iron are intimately mixed with the iron, since, even within the separate fiber skeins, portions of slag may, with a sufficiently high magnifying power, be always discovered. Varieties of iron that are very free from slag, as, for example, crucible cast steel, are hence the most averse to the formation of fibers.

CONSTITUTION OF THE INDIVIDUAL IRON CRYSTALS.

While, under the microscope, weld iron is very suitable for the study of the formation of fibers, it is, on account of the slag mixed with it, unsuitable for the investigation of the grains and the individual iron crystals. For the latter, ingot iron alone is quite suitable. In the microscopic examination of the various varieties of iron, the only variety appearing approximately homogeneous, and composed of grains of the same size, is crucible cast steel that has cooled comparatively quickly. In all other varieties, even in test pieces from large, and therefore slowly set, ingots of cast steel, on etching the thin section, two different varieties of iron are exhibited, one of which is interstratified in the other, by which it is in a manner surrounded, so that the smooth cut plane of fracture acquires a porphyritic appearance. The closer the iron approaches to raw iron, that is to say, to the original state in which it was produced, the more distinctly do the two kinds of iron separate from each other, and exhibit a network

enclosing angular bodies. In the following portion of my paper the angular inclusions are termed crystalline iron, while for the iron forming the network the term homogeneous iron is reserved, merely for the sake of clearness and brevity. The form of the crystalline iron bodies is that of regular polygons, only in the interior of iron ingots that have cooled uniformly. In other cases the crystalline iron bodies are, as a rule, extended in one direction, and this longer axis is at right angles to the cooling surface in the case of iron simply set, and not submitted to any subsequent treatment. In the case of pieces of worked iron, on the other hand, it follows the course of the homogeneous iron portions, or corresponds to the axis that is not compressed—generally the longitudinal one. Bodies of this kind often occur together, and thus give rise to forms resembling letters, and apparently quite irregular. These forms, however, under a sufficiently high power, may always be further split up, exhibiting the manner in which they are formed of single regular bodies. The crystalline iron bodies occur the more frequently, but, at the same time, of smaller dimensions, the more closely the proportion of carbon approaches the limit of 2 per cent. In soft iron, low in carbon, they are often widely separated from one another, but are of considerable size. The crystalline iron bodies of the latter kind appear to consist again of various parts. The impurer the iron investigated the more they exhibit surfaces not unlike that of a checkered cloth. On the other hand the homogeneous iron forming the network is very uniform, even when seen under a very high magnifying power.

It is remarkable that the homogeneous iron is sometimes harder and sometimes softer than the crystalline iron. If the iron has been produced by a decarburizing process, it is softer. If the iron (as for example, cement steel) has been produced by a carburizing process, it is harder. This may easily be seen from the fact that, with etched test-pieces of the former kind, the network is depressed from the surface, while in test pieces of the second kind the network projects from the surface. Hence, the homogeneous iron appears to be the conductor of oxidation and carburization, by which means a molecular change to crystalline iron is effected. If these phenomena have been made sufficiently clear from the study of ingot iron, the same may also be discovered in the case of weld iron. Here the homogeneous iron passes through the iron mass, frequently like leaves, in a direction inclined to the axes of the fibers, forming, to a certain extent, the cement between the fiber bundles. An explanation of these phenomena, hitherto studied but little, if at all, can only be found in the want of uniformity in the iron. Obviously, particles of the same kind collect together and separate out in the remaining mass that is still fluid. For the present it is, however, not clear why the homogeneous iron, which apparently represents a pure metal, forms the ground mass that remains fluid to the last; as one might be inclined to assume that, as in the puddling process, the grains of pure iron separate out first and are drawn through or surrounded by the impure fluid iron mass. This phenemenon consequently requires further investigation. It might, perhaps, be possible, by careful etching of large masses of iron, to dissolve sufficient material for an analysis.

WELDING.

The scientific explanation enunciated by me some years ago that welding represents the transition from adhesion to cohesion has been somewhat generally adopted as a satisfactory one. This explanation, however, has been considerably modified by the results of microscopic investigation. The fibers of weld iron are situated like parallel wires in a bundle of wire, only connected by slag, or, better, separated by slag; but where the fibers are replaced by grains, the homogeneous iron again occurs surrounding them, or, at least, flowing through them. The corners and bends formed by the homogeneous iron increase in proportion to the size of the crystals, and this explains, as is shown by the microscopical examination of cold short iron, the weakness of iron containing phosphorus. In the case of ingot iron, on the other hand, the homogeneous iron forms an evenly surrounding layer around the crystalline iron, and forms, in this manner, the connecting mass, so that the fracture of a very finely-granular steel

resembles in every way that of an amorphous substance, such as glass, although the individual crystal grains produce the roughness of the fractured surface. The greater irregularity of ingot iron, corresponding to the distribution of the two varieties of iron it contains, as compared with weld iron, does not, however, prevent the mass of the metal showing, on the whole, a greater resemblance to a homogeneous substance than weld iron does. This fact explains the greater tensile strength of ingot iron as compared with weld iron. In the case of the latter, traction acts separately upon each individual fiber, just as it would upon each individual wire contained in a bundle of wires. When the limit of elasticity is passed, each fiber stretches separately, and breaks on passing the limit of resistance, the consequence being that weld iron breaks gradually and not suddenly. In the case of ingot iron the grains stretch independently, and change their relative positions without, however, losing their connection with one another, which is produced by the homogeneous iron, until the limit of resistance is reached; then, however, the fracture is a sudden one. This property of ingot iron is, if properly applied, an advantage as compared with weld iron, and not a disadvantage, as is supposed by many engineers, who find it impossible to break loose from old-fashioned ideas.

The uniformity of ingot iron as a whole is only interrupted by the presence of blowholes. It is a well-known fact that it is impossible to produce castings of ingot metal which are entirely free from blowholes. The difficulty increases in inverse proportion to the percentage of carbon. Again, it is well known that the formation of blowholes may, to a considerable extent, be prevented by allowing the metal to stand quietly, by pouring at a definite temperature, by the addition of silicon, &c.; or that, by the application of pressure, a uniform distribution of the blowholes may be effected. The microscope, however, shows that blowholes never disappear entirely, although they are to a great degree eliminated in the case of crucible cast steel; they are most abundant in Bessemer steel, open-hearth steel holding an intermediate position between the two. The blowholes appear to be always surrounded by homogeneous iron, and never to occur in the crystalline variety, nor even to pass through the crystalline iron bodies. The microscopic examination of blowholes occurring at some distance from the surface of an ingot iron casting is of some interest. The connection of the external layer of the casting with the internal portion is, in this case, always represented by a network of homogeneous iron, never by the crystalline iron bodies which occur as elongated, distinctly-defined curves placed perpendicularly to the cooling surface, while continuous veins of the homogeneous iron may be traced from the edge to the interior.

What I have hitherto stated refers to the connection or welding together of the parts of iron in the case of one piece. It is somewhat different in the case of *two* pieces of iron which have been welded together. In this latter case the joins are always discernible. In weld iron, however, at the point where two fibers of the two separate pieces simultaneously cease, it is evident that there has been a mingling of the crystals. There are probably those points which appear crystalline when a weld seam is torn apart, and in which the adhesion of the two pieces of iron has really changed to cohesion. It is different in the case of two pieces of ingot iron. In this metal the joins can always be traced. It is true that the crystalline portions of the two piece, after being worked considerably, are pressed into one another, and consequently hold together somewhat like hooks; but the homogeneous iron, which always occupies the surface, appears never really to combine. From this it may be concluded that, for a true welding of ingot iron it is necessary to heat the metal almost to its melting point. This explains the imperfection of all ingot iron welds, and it would be better for engineers to avoid entirely all welds of this kind, and to replace them by iron suitably shaped. It also explains why a very soft ingot iron containing slag may be welded just like weld iron. The strength of a finished piece of iron depends on the sectional area of the mass of iron it contains. From the total sectional area of a piece of weld iron the slag inclusions, and, in the case of ingot iron, the blowholes must be deducted.

This calculation is decidedly in favor

of the ingot iron, though it can only be superficially effected, even with our present knowledge of microscopy.

CONCLUSION.

However inexhaustive my observations may have been, seeing that the examination was confined to, at the most, a few hundred microscopic sections, still I trust that they have yielded interesting explanations, or, at all events, suggestions; and it would afford me much pleasure to have an opportunity of showing, in Berlin, to the members of the Iron and Steel Institute, proofs of the statements made in my paper.

Sir Henry Bessemer—With regard to the crystallization of iron or steel that had undergone fusion, very different results were obtained according to the size and form of the crystals, and this again was dependent upon the perfect stillness or otherwise of the mass under operation. In England the crystallization of sugar was allowed to go on unobstructed, and, therefore, large and distinct crystals were formed; whereas in France it was stirred during the crystallization, and so an amorphous mass of saccharine was produced, and the crystals were less bold and distinct. Nearly thirty years ago he was anxious to see how the presence of phosphorus affected crystallization, for he suspected that the large crystals from which his process at that early period was suffering were due to the presence of that deleterious element. He determined to allow the mass to cool as slowly as possible, and for that purpose he had a large hole made in the ground four feet in diameter, lined with brick. A charcoal fire was kept in it for three or four days, and when it was heated to a white heat a mass of Bessemer iron, wholly decarbonized, was poured into it. It was then covered with hot sand, so that the escape of heat was rendered almost impossible except by slow percolation. At the end of five or six days the mass was taken out and allowed to cool, and he found that a piece 15 inches in diameter was readily broken through by a single blow of a large hammer. By taking it in one hand and striking it with a 2-lb. hammer he could detach showers of crystals. The cohesion of the mass had been rendered almost *nil* by the perfection of the crystals. Some of them were cubes of nearly the size of common dice, and were beautifully polished and white. They could be hammered out into discs as large as a sixpenny piece, showing that the metal was malleable, and it owed it excessive weakness only to the perfect crystallization which had been encouraged by that particular process. That, perhaps, was one of the points which practical men would, with a little consideration, no doubt apply in some shape or other. It was quite impossible, when iron was produced in a malleable state, and recarburetted by putting very highly carburetted metal into it, that the simple pouring of these two elements together could produce a homogeneous mass. In every ingot of ordinary iron or steel there were strata of the malleable iron mixed with the carbonizing metal, and it was only by a thorough admixture of those parts before casting took place that they could hope to get anything like a homogeneous result. Even that, as Dr. Wedding had shown, was not sufficient to prevent the formation of different kinds of crystallization, as though some particles elected to form crystals of themselves, and, having abstracted certain atoms to form that crystal, they left in their immediate neighborhood and surrounding them another compound of iron containing more or less carbon than the central nucleus was formed of.

Mr. Bauermann regretted the absence of illustrations of the structures described in the paper. A Russian observer had stated that in all he had examined the crystals belonged to the cubic system, but Professor Malliard, of the Ecole des Mines, Paris, stated that the cubic form was retained only up to when the alloys contained less than 50 per cent. of manganese. The paper stated that by rolling the grain was converted into a column belonging apparently to the tetragonal system. It would, therefore, seem that it was not a really crystalline change, but something analogous to what took place when any homogeneous material, such as glass, was strained.

According to the census returns of 1881 the Civil Service employed 50,245 persons, and the police 32,508. Soldiers, with yeomanry and militia number 87,168.

—*The Engineer.*

RENDERING WOOD FOR BUILDING PURPOSES NON-INFLAMMABLE.*

By THOMAS MANSON RYMER-JONES, M. Inst. C.E., F.R.G.S., and JOHN RYMER-JONES, M. Inst. Tel., Eng.

From "The Building News."

The rendering of wood for house construction non-inflammable should be a subject of great interest to all. There is, however, little information obtainable from published works. The Japanese light structure is, perhaps, a necessary evil, but also has its advantages, and the Japanese would probably be unwilling, either from conservative feelings or from a question of expense to build more costly, durable and fire-proof houses. Let us see, then, how these wooden buildings may be preserved from destruction by fire, though the cost in the more thorough processes may be possibly sufficient to preclude their use among those whose poverty is the reason for their not building more durable houses. Secondly, it must be remembered that an individual house, though it would not burn by itself, yet if surrounded by a conflagration of the adjacent buildings would smoulder away, though it would not burst into flame. Hence, it is necessary that a block or small number of prepared houses should be together, in which case only those nearest to the advancing fire would be damaged and all the rest remain intact. This might be done by Government order, private agreement amongst themselves, or a fire insurance company, who would take insurances on any block so prepared at moderate rates. Let us then proceed to pass the various processes in review, with their advantages and disadvantages fully set out, and with their capabilities for preserving fabrics as well as timber from fire. Should it be decided to impregnate the wood with chemicals, this can be done, or a superficial coating of fireproof and waterproof paint over the wood may be put on, or both; but the Japanese do not paint their wood much, and might not care to do so. In either case it would be necessary to use a preservative, which, in places exposed to rain or washing, would not wash out. How far those non-inflammable salts which unite chemically with the albuminous and nitrogenous matter in the wood are capable of resisting wet it is difficult to say with certainty; but there are some which we shall proceed to give further on which do effectually resist water and soap. Again, it must be a *sine qua non* that, whatever the fireproofing material used may be, it must be one which, when exposed to the action of fire, does not give off injurious or suffocating smoke. Also, if the impregnation of the timber is to be thorough—*i. e.*, through the whole of the wood—it would be most advantageously done whilst the wood is green and the sap uncongealed, otherwise the tubes become clogged, and the injection of the chemicals only extends to a certain depth in the wood if the latter is dry. Impregnating the timber when green, and operating on large balks, would perhaps be more economical than upon smaller pieces when cut up. Moreover, the coarse-grained quickly-grown timber required in this case is most suitable and cheapest; but then salts are said to make wood soft, and cut across the grain, so that it might be difficult with the ordinary handsaw to cut out the more delicate work which is so largely used in Japanese houses. Lastly, if the wood is impregnated while green, the non-inflammable solution acts chemically on the sap and destroys the germs of rot; for although most of the sap is driven out and replaced by the solution, still a little remains in the moist wood, and this would tend to dilute the injected solution, and also cause subsequent decay. Of course, the "shortness" of the wood caused by salts applied to balks, whether prepared when green or well seasoned. These remarks are by way of preface. The following has reference principally to the preservation of telegraph-poles from decay, but is equally

* A paper read before the Civil and Mechanical Engineers' Society.

applicable to our subject. Gavey and Douglas say: "The physical formation of a tree is made up principally of cellular tissue-woody fiber and vascular tissue. Cellular tissue consists of little colorless bladders or vesicles of various figures adhering together in masses and filled with liquids. Woody fiber is an elongated form of cellular tissue incrusted and hardened by various substances, which give the distinguishing characteristics to different classes of timber. Vascular tissue consists of small membranous spiral tubes or vessels which aerate and transmit some of the fluids in the plants. If a transverse section of a piece of exogenous timber be examined, it will be found to consist (1) of a small portion of central pith; (2) the woody portion, divided into the heartwood and sapwood; (3) the bark, which latter is divided into the true bark and epidermis; and, lastly, the medullary rays, which are thin vertical plates connecting the bark and the pith, and radiating from the center to the circumference. In a young shoot, the pith, which consists wholly of cellular tissue, appears to serve as a vehicle for the ascending sap, which, rising from the roots to the leaves, there comes in contact with the air, where it combines with the elements necessary for the formation of the tree. Woody or ligneous fiber and vascular tissue extend vertically downwards, and at the same time cellular tissue is formed horizontally. The ligneous fibers are attached to each other, as it were, in bundles, by their respective coats, whilst the cellular tissue is forced into the thin vertical plates, termed the medullary rays, which connect the pith with the bark (known as silver grain). The residue of the sap descends through the inner bark to the roots; thus a layer of wood is gradually formed between the medullary sheath and the bark, and this continues until the approach of winter and the fall of the leaf stop the operation. On the following spring the sap again ascends, and during the year a second layer of wood is deposited; and as each season advances towards winter the deposit takes place more and more slowly, so that well-defined rings distinguish each year's growth. The number of the rings indicating the age of the tree in years, and the thickness of each ring the rate of its growth. Sometimes in hot climates there is so little difference in activity at various seasons that the rings cannot be accurately defined. In temperate seasons the sap rises during the spring, and leaves are developed. In summer the sap almost ceases to flow, and vegetation remains stationary. In autumn the sap descends, and the leaves fall off. In winter the tree becomes again inactive. In tropical climates the dry season is the period of inactivity. When wood is going to be seasoned in the ordinary way, the best season for felling trees is when the circulation is least active, viz., the middle of the summer or winter periods of quiescence, and during the dry season in tropical climates. After a few years the ligneous fibers deposited nearest the center become darkened, hardened, more dense, and apparently impervious to the sap, which latter circulates upwards through the layers of woody matter last deposited; hence the distinction between the heartwood and the sapwood." Sabine and Douglas say: "Dr. Boucherie has discovered that no connection exists laterally between the tubes of a tree, and that by applying, under a moderate pressure, a colored solution to certain tubes at one end of a tree, the same tubes at the other end of the tree, and only those, are colored. In this way, at one end of a felled tree he applied a colored solution to certain tubes, forming the name 'Faraday;' the name was transmitted to the other end and was perfect at every intermediate section, showing that there was no lateral diffusion." On the other hand, Langdon says, "That there is a lateral connection between the fibers may be proved by reference to a creosoted pole, where, under pressure, the creosote penetrates between one and two inches." It is generally understood that there is some, but very little, lateral connection, so that in order to do it thoroughly the injection should take place from the ends. Inasmuch as the sap in timber contains the elements of decay, in the higher class of construction it is necessary to use wood most free from sap, or to use special means to eliminate it by the internal application of preservative solutions which either expel or neutralize the fermentative portion of the sap. Although the solution used

generally for the preservation of timber from rot need not be the ones best suited for rendering the same fireproof, yet the method of application is similar. We will therefore describe the system most commonly used. It is better and generally cheaper to select soft woods, such as Scotch pine, which is admirably adapted for injecting processes. It is of very coarse grain, its annular rings are wide apart, tissue soft, and capable of great absorption. The idea in the ordinary methods adopted for preserving timber from rot is to introduce into the pores of the wood some salt, which, uniting chemically with the albumen of the sap, is stated to convert it into an insoluble compound. The best known of these processes are Burnettizing, Kyanizing, and Boucherizing (creosote is, of course, out of the question), Burnettizing consists in impregnating the timber with a solution of chloride of zinc, and Kyanizing with corrosive sublimate (chloride of mercury). Langdon says: "In both processes open tanks are filled with the solution with which the timber is required to be charged; the timber is then submerged, well fixed, and allowed to remain soaking until it has absorbed the proper quantity of the solution." Culley says: "Kyanizing, or steeping the timber in a solution of bichloride of mercury or corrosive sublimate, has not been extensively tried for poles because of the expense, and because it does not succeed unless the timber is dry. The poisonous nature of the salt is a very serious objection." Spagnoletti says, with regard to the chloride of mercury process: "Two pounds of corrosive sublimate at 3s. per lb. will be sufficient for 50 cubic feet of timber. The time for preparing should be one day per inch in thickness, and one day over. Thus, two days for one inch, three days for two inches, and so on; one advantage of this preservative is that the timber may be cut down and cast into the tank at once, the sooner the better, and no preparation or seasoning is necessary." He thinks it an excellent preservative of wood. The superficial coating obtained by mere soaking is perhaps good enough for telegraph poles, when the latter are thoroughly dry to begin with, and the heartwood hard; but to make wood fireproof it would be necessary to thoroughly saturate the wood with the solution, and the following mode of injecting telegraph poles is much better. The operation generally takes place in strong cylindrical tanks in which are fitted hemispherical ends, removable at will; to save labor they are usually provided with a line of rails, which communicates with tramways running through the preserving yard. The poles being stacked on light trucks or trollies, are thrust into the tanks, the ends of which being then closed, the interior is exhausted with powerful air-pumps driven by steam. The object of this is to draw out any moisture that may remain in the wood (the timber must be thoroughly dry before it undergoes the operation), and also to create vacuums in the pores, which may be filled by liquid. The liquid is then allowed to flow into the tank, and exposed to pressure varying from 100 to 150 lb. per square inch. The contents of the tanks and the quantity of timber in them being known, the exact quantity of salt injected into the wood per cubic foot can easily be calculated. The salts evidently combine with the elements in the sap which tend most readily to ferment, and by precipitating them and forming new and insoluble compounds check this tendency and lengthen the life of the timber. If a transverse section of a pole thus treated be examined it will be found that the sapwood alone is permeated by the salt, the heartwood remaining untouched. The elements of decay, however, are far more fully developed in the sappy portion of the timber than in the heartwood, and the latter is rarely attacked (speaking of rot, of course, not of fire), until the former is rotten. Sabine says: "The posts are put into wrought-iron cylinders 4½ ft. to 6 ft. diameter, and 34 ft. to 60 ft. long, closed at one end, and covered at the other with tightly-fitting tops. The cylinders are provided with manometers, safety valves, &c., and connected with air and pressure pumps and a reservoir of the preservative solution. The wood is preserved by being subjected to a great pressure of steam, which, penetrating into the interior, not only tends to displace the sap from the pores and prepare them for the preservative solution, but also to coagulate the albumen in the sap and in this way retard the subsequent

rolling. After this the cylinders are exhausted, and immediately filled with a solution of 1 part chloride of zinc and 30 parts of water (for burnettizing), which is kept under a pressure of 8 to 10 atmospheres = 120 to 150 lb. for three hours; but it is questionable if this method is so good as that of Dr. Boucherie (boucherizing), as it is necessary to force the solution into the wood at right angles to the tubes, thereby injuring its strength and letting the sap, which is the immediate cause of decay, remain; the coagulation of the albumen in the sap to any material depth below the surface being a matter of doubt." From this it would appear that the timber need not necessarily be dried prior to the operation, as other men state, and in coarse-grained woods, where the injection is complete throughout the whole bulk. This, then, is the degree of saturation when ordinary strong timber, consisting partly of sap wood and partly of heart wood, even when expensive cylinders with powerful air pumps, &c., are used, so that the following method of injection, which is employed for boucherizing, would (should no after consideration prevent it) be the most suitable for the purpose, should it be wished to completely saturate the woods throughout, since the apparatus is comparatively cheap and portable, the timber best suited being coarse grained, quickly growing, and consequently less costly, and, in fact, of the kind mostly used for house purposes, the operation performed in the forest where the timber is cut down, and whilst it is yet green and full of sap. The method follows: Newly cut green timber (coarse grained is most suitable), before its bark is removed, is exposed at the butt ends to a slight pressure of a liquid column of sulphate of copper: the liquid is usually arranged in tanks at a height of some 50 ft. above the level at which the tree is placed. The pressure forces the liquid through the longitudinal pores of the timber till it drops out at the other extremity, both driving the sap before it and forming the chemical combinations which effect the preservation of the wood from decay. Poles should be exposed to the operation without the slightest delay after they are felled, or the process will probably fail as the resinous substances rapidly harden and prevent the movement of the liquid salt through the pores. The plant needed is inconsiderable, and can be set up in any locality, the only requirement being a clear space of open ground; this gives this system an advantage over others which demand expensive and powerful machinery. The arrangements in a boucherizing yard may be thus described: Any open tank of any convenient capacity is erected on poles at a height varying from 30 ft. to 50 ft. Some prefer 23 ft. to 26 ft. from the ground. Culley says: "24 ft., since, if it is too high, the liquid flows too rapidly through the timber to produce the best effect." From this tank descend two leaden pipes about 1½ in. in diameter, one of which is connected with the force pump designed to fill the tank with liquid, the other serving to convey the liquid to the poles; the latter are laid side by side on racks placed horizontally, and are arranged at right angles to a passage running the whole length of the yard. Down this passage is carried the leaden pipe from the tank, and at regular intervals of 18 in. along this pipe small branch pipes with stopcocks are fitted. As each pole is felled and hauled into the boucherizing space a section is cut off the butt end to expose a fresh uncoagulated surface of wood. Near the circumference of this newly-cut surface a strip or ring of india-rubber is nailed; then a flat board somewhat larger than the base of the pole is screwed against it by means of iron dogs (tenon bolts). A hole in the center of the board admits the insertion of a hollow boxwood plug, which is connected with one of the small branch pipes previously alluded to by means of a short length of flexible india rubber tubing. This plate or disc, together with the india rubber ring just mentioned, forms a watertight chamber for the reception of the solution of sulphate of copper, communicating with the vertical pressure pipe. The air must be carefully expelled from the chamber formed at the butt end of the pole. This is best done by inserting a wire between the pole and the packing, which forms the side of the chamber. After the tap has been turned to admit the solution, the wire is withdrawn, leaving a small hole, and when the liquid spurts out freely, showing that the air has escaped, this hole can be

closed by a blow from a hammer. Neglect of this precaution has frequently prevented the injection of the upper side of the entire pole. The tap being turned, the liquid solution is driven into the pole with a pressure dependent on the height of the tank above the racks. The proportions of the solution are 1 lb. of sulphate of copper to 5 gallons of water (Douglas says 1 part by weight of the salt to 100 parts of water). It is usual to inject 0.35 lb. of the sulphate into every cubic foot of timber. This gives a weight of 1½ to 2 lbs. of sulphate for the smallest pole (about 25 ft. long). After the lapse of a period varying from two to 24 hours, the liquid makes its appearance at the top of the pole and drops into gutters placed conveniently to catch it. Douglas says "three days is the average time required to inject a 25ft. pole." The process is complete when every portion of the top of the pole is found to be saturated with sulphate of copper. This is known to be the case when a brown stain is left on the timber by the application of a piece of potassium ferrocyanide. Another authority says: "Scotch fir having a very open grain is the best suited for preservation by injecting process. To boucherize successfully, the worst timber (that is, as regards its chance of lasting unprepared) is wanted. In some cases, where the poles were too old, a month elapsed before the copper liquid applied to the butt appeared at the top of the poles." Another authority of large experience says: "Scotch fir, grown on a peaty soil, coarse grain and open. The average time taken to successfully boucherize three 28ft. poles, during the months of April, May, June, July, August and September, was from five to seven days. In October, November, and December, January, February and March, about ten days. In June, the quantity of liquid that percolated through three 28ft. poles was 68 gallons; and soft larch took a few days longer than the Scotch, but answered very well. Some few red or hard larch were successfully done, but in others the solution only penetrated a very short way from the end. Spruce and silver pine answer well, but take much longer than do either larch or Scotch." Cully says: "It is quite possible to inject trees that have been cut down two or three weeks; but the labor will be much more costly than when injected in the forest on the day they are felled, because the pores contract in drying. It is extremely difficult to inject larch unless it is of very open grain and very free from resin. The liquids cannot penetrate the hard wood, and it has sometimes happened in boucherized poles that the heart wood, which is more durable than the sap wood under ordinary circumstances, has rotted, leaving the sap wood sound. Spruce and Scotch fir are very suitable, and these cheaper descriptions of fir are rendered more durable than the more expensive sorts when unprepared. The cost of the sulphate process calculated on 5,287 Scotch firpoles injected in the forest, and averaging 25 ft. in length, is about 2d., equal to 4 cents, per lineal foot of timber." Another authority says: "The process is most successful during the spring and summer months (Culley says the autumn), when the sap is in the wood. Cold hinders, and frost stops it." Timber, when chosen for strength and seasoned in the ordinary way, is, of course, felled in the winter months, when the sap has retreated from the wood. It is possible to treat the wood, with equal success, from the top or small end as from the butt end. The application of the sulphate of copper to the small end instantly starts the sap, which pours out from the large end. The boucherizing of the pole was completed, in a particular experiment, in the usual time. In the construction of a house it would be necessary, in the case of the upright timbers which stand in the moist ground, to provide for the escape of the fireproof salt by painting the lower end with a coating of some soluble or waterproof paint, which should also be fireproof. Such a paint we shall give further on. Without this precaution much of the salt is lost in the ground, and the surrounding ground in which the boucherized pole stands will be found tainted for some 12 in. or more with sulphate of copper." Mr. Boucherie says, however: "It is an error to suppose that the sulphate of copper is very soluble, or that when it is exposed to the rain its preservative power disappears after a certain time. The sulphate of copper fixes itself into the elements of the wood, and

could not be dissolved by washing. Cases of failure are to be attributed to a disease in the wood, the diseased tissues seeming to resist the sulphate." Professor Abel also objects to the theory of the salt being washed out when a telegraph pole is standing in moist ground. He says: "I believe a small quantity of the copper salt is converted into insoluble compound. The action of the metallic compounds is to combine with certain albuminous substances in the wood, by which they are converted into insoluble substances, and it is by the chemical alterations which these undergo that the preservative effect is produced." It is generally allowed, however, that the preservative effect of sulphate of copper is very variable. Professor Abel, judging from experiments on boucherizing and kyanizing for the preservation of wood from decay, says: "Generally the results were favorable to boucherizing." He believes, "Opinions are fairly divided between them—that is, the copper salts and mercury salts, which are undoubtedly both good preservative processes." Mr. Boucherie says: "Sulphate of copper combines so well with the celluline that washing with pure water will never expel it." Culley says: "The sulphate of copper must be free from iron as it is inert and possibly hurtful; besides this, the iron becomes oxidized by exposure to the air and forms a muddy deposit, which chokes the fine filter formed by the pores at the end of the pole and thus stops the process, as the sulphate always contains *some* iron (Japanese sulphate of copper obtained from the Mint contains a large quantity, but varies much in this respect). It is well to make a saturated solution, and allow it to remain exposed to the air as long as possible, so that the iron may deposit at the bottom of the vessel, after which it may be diluted for use. If the solution is too strong, it appears to contract the sap vessels and to crystallize at their ends. The water must be free from lime and perfectly clear. If it contains lime it is as well to add a little sulphuric acid to precipitate it, and either allow it to settle or filter the water through sand, for even the slightest cloudiness interferes with the injection.

"All timber, when treated by any of the preservative processes in general use, becomes short; that is, it breaks in two, crosswise, easily, and when the whole wood is impregnated its tensile strength becomes impaired. It no longer, when dry, retains the same amount of elasticity it possessed when in its natural state; when in a moist state it recovers a great deal of this. The shortness is easily tested by taking a piece of dry, preserved timber and trying to split it with an axe, it will be found that the axe will not follow the course of the grain of the wood." This, then, seems a reason in favor of impregnating the wood, and especially the smaller pieces, after they have been cut to fit into their several places. Again Sabine says: "It is necessary that no ungalvanized iron should come in contact with wood impregnated with sulphate of copper (and the same point must be considered when using other salts, otherwise the copper of the solution will be reduced by galvanic action)." Another authority says: "Boucherizing is injurious to the stay wires and bolts, as the sulphur so quickly attacks not only the galvanizing of these, but the iron itself, that the stay wires of poles are soon eaten away." This shows that when salts are used for impregnating wood, iron bolts and nails should be protected in some way—by paint, say. Kyanizing (chloride of mercury) is equally injurious to iron. Another process for preserving wood from decay is Beer's process, in which *borax* is used. It is supposed to neutralize the decaying vegetable matter in the wood, and is also a very good non-inflammable solution. We have entered thus fully into the methods of preparing wood with sulphate of zinc, chloride of mercury, and sulphate of copper in particular, because whatever may be the non-inflammable solution selected as most suitable, some such method of injection will be necessary if the impregnation is required to be other than merely superficial, in which case the same considerations would apply. Moreover, since all the three above-mentioned are salts, they will not only preserve the wood against rot, the influence of the weather, insects and worms, but will render it also to a certain degree non-inflammable, but which non-inflammable solution is best we shall probably be better able to judge further on. But we should, to begin

with, put chloride of mercury and sulphate of copper out of the question, as the first is too expensive, and when burnt is turned into vapor which has a fearfully suffocating effect on the nose. Sulphate of copper, though cheap and free from this disadvantage, would turn the wood a blue color when the surface is damp with the moisture in the air. Chloride of zinc and borax, the other two of the above-mentioned processes, both preserve from decay and flame, and might be used for injecting green timber. But, at present, let us go through the information that can be gathered on the subject of non-inflammable compounds and solutions. Much of this relates to rendering fabrics non-inflammable, and this is done by steeping them in almost any saline solution. Thus, cotton and linen stuffs prepared with a solution of borax, phosphate of soda, phosphate of ammonia, alum, or sal-ammoniac, do not suffer active combustion nor burst into flame. The salts act by forming a crust of incombustible matter on the surface of the fibers. They do not, however, prevent carbonization taking place when the temperature is sufficiently high. The cotton thread is reduced to a cinder when burnt, but from the action of the salt its fibers still retain sufficient tenacity to support a light weight. The addition of 1 oz. of alum or sal-ammoniac to the last water used to rinse a lady's dress, or a less quantity added to the starch to stiff en them, renders them non- inflammable, or, rather, they will not readily take fire, and, if kindled, are slowly consumed without flame. None of the above are used for fine, soft muslins, because they render the fabric harsh, and destroy all its beauty. The salt which is found to answer all conditions most completely is tungstate of soda; steeped in a solution of 20 per cent. of this salt, muslin is perfectly non-inflammable when dry, and the saline film left on the surface is of a smooth and fatty appearance like tallow, and does not interfere with the process of ironing. The non-fulfillment of this last condition completely prevents the use of many other salts, such as sulphate or phosphate of ammonia, which are otherwise efficacious in destroying their inflammability. The addition of a little phosphoric acid, or phosphate of soda, is recommended to the tungstate, for without this a portion of the tungstate is apt to undergo a chemical change, and become comparatively insoluble. For a solution of tungstate of soda of minimum strength, dilute a concentrated solution of neutral tungstate of soda to specific gravity, 1.14, and then add 3 per cent. of phosphate of soda. This solution is found to keep, and answer its purpose well. Again, in Ure's Dictionary, "Cotton and linen cloth may be best rendered incapable of burning with flame by being imbued with a solution of sal-ammoniac or of alum." Tomlinson says: "Experiments were tried with 40 salts, but out of these only four appear to be applicable to light fabrics—namely, phosphate of ammonia, chloride of ammonium (sal-ammoniac), sulphate of ammonia, and tungstate of soda. The sulphate of ammonia is the cheapest salts, but causes brown spots on the muslin when ironed, and dissolves in water, so that it requires to be renewed after every washing. Tungstate of soda is the only one that does not interfere with the iron, and consequently is that usually adopted. The oxides of tin withstand both the water and the soap, but they impart a yellow tinge, consequently their application has been restricted to canvas, sails, and other coarse materials (and would not affect their use with woods). This is also the case with borate and phosphate of protoxide of tin and arseniate of tin. These last are some of the attempts which have been made to fix some of the non-soluble compounds in textile fabrics. The method of rendering a sailcloth permanently non-inflammable is to so soak the canvass for two days in a protochloride of tin solution of the strength of two parts of the salt to one of water, and to leave it for a day in a concentrated solution of stannate of soda or carbonate of soda. The canvas is lastly dried, and is then ready for use." Also, we find in the "English Cyclopædia": "Cotton and linen fabrics may be partially protected from fire by a solution of alum or common salt; but alum weakens the fibers, and the salt makes them harsh and crisp; borax will exert a considerable preservative effect, but the material is weakened, as with alum. It was found that phosphate of ammonia exerts the preservative effect; but the salt becomes decomposed under the laundress's iron.

Sulphate of ammonia (only quarter the price of its predecessor) had most of its merits, but the same defect. Tungstate of soda has all the advantages, and is free from the disadvantages." So much for fabrics. Of course, what would be a disadvantage for linens need not be so for woods; but I give the above as an addition to our list of non-inflammable solutions, to guide us to the selections of the most suitable ones hereafter. The "English Cyclopædia" says, with regard to wood: "Many methods have been devised for making woods more or less fireproof. The substance which is attracting most notice now is silicate of soda. Mr. Abel, chemist to the War Department of England, and Mr. Hay, chemist to the English Admiralty, made experiments on this salt in 1857. A portion of a wooden but was painted three times, inside and out, with a solution of silicate of soda; but unfortunately for the fairness of the experiment, the building was constructed with a double boarding, so that it was only possible to coat or impregnate each plank on one side; but the value of the silicate was established beyond a doubt. A flame from a large heap of shavings was placed against this part of the building for some minutes, but only succeeded in catching the end of one plank, and even that did not blaze, but only smouldered for a short time. By the heat of the fire the salt was drawn to the surface of the wood, and formed a glaze upon it. Subsequently, when the main body of the hut was destroyed by fire, after several unsuccessful attempts to extinguish it by Phillip's Fire Annihilator (for testing the efficiency of which the experiment was made, the silicate of soda experiment being only a secondary one), although the fierceness of the flame was such that few materials could have withstood it, yet several planks remained of the exterior coated portion. Upon examining the planks, the unprotected surfaces were found to be completely charred, but this charring only extended to those parts which had not been touched by the silicate. Asbestos paint has been used with nearly similar results. So far as experiment has gone, silicate of soda appears the most convenient and effective known for the purpose." Again, respecting silicate of soda, "Spon's Workshop Receipts" says: "Deal boards become almost incombustible when painted over with a diluted solution of silicate of soda called also glass water. The glass water is generally sold as a thick fluid like honey. This may be thinned out with water six or seven times its own bulk; the water must be soft, or boiled water will do, and apply the solution warm. In about 24 hours apply a second coat, and, perhaps, a third. Use a new brush, and wash the brush in clean water after using, or it will get too soft. Avoid grease or fat on the boards before painting them." In the same book is another receipt, as follows: "Soak the wood in a strong solution of alum and sulphate of copper—about 1 lb. alum and 1 lb. sulphate of copper should be sufficient for 100 gallons of water. These substances are dissolved in a small quantity of hot water, then mixed with the water in the vessel in which the wood is to be steeped. The timber to be rendered fireproof can be kept under the liquor by stones, or any other method of sinking it. All that is required is a watertight vessel of sufficient dimensions to hold enough of the liquor to cover the timber, which should be allowed to steep for four or five days; after this it is taken out and allowed to dry thoroughly before being used." This would be a very good plan to adopt with the upright timbers after thoroughly injecting them by the system of boucherizing, as before explained, whilst the timber is green. As the Japanese use their wood plain, the blue sulphate of copper tinge would hardly do for the lighter woodwork. With regard to burnettizing, sulphate of zinc, which, as before mentioned, has been largely used as a preservative against decay, there is no reason why it should not be used for every part of the wood in a house, injecting it whilst the wood is green, as it acts chemically on the sap, and is white. Sir W. Burnett says of it that it renders the wood non-inflammable. The following is extracted from his pamphlet on the subject: "For preserving timber, canvas, cordage, and woolen things from dry rot, mildew, moth, and the destructive influences of the elements, salt water, so far from hastening decay or neutralizing its effects, has, on the contrary, the quality of increasing its efficacy; it is

perfectly innocuous, and cannot endanger health. All the timber and ceilings of a ship may be impregnated with the solution without the slightest prejudicial effect on the crowded inmates. It retards the oxidization of metals, as has been proved repeatedly upon copper and iron bolts, with the most satisfactory results, and articles prepared with this solution will resist combustion in proportion to the strength of the solution used." Again, another method: "In Maughan's process, dry wood is saturated with an aqueous solution of phosphate of soda and muriate or sulphate of ammonia. A decomposition ensues, followed by an evolution of ammoniacal vapor, and the formation of an incombustible coating on the surface of the wood." Jackson's patent consists in the application of a solution of salts of zinc and ammonia. Mr. Payne's wood-preserving process is well known. Wood is rendered fireproof by means of a solution of sulphuret of barium or calcium. The wood or other vegetable matter is put into an air-tight vessel, from which the air is driven out by means of steam. The steam is condensed by the injection of the solution of the sulphuret, and by the application of cold water to the outside of the vessel. A partial vacuum being thus obtained, the solution is allowed to flow into the vessel from the tank containing it through a pipe furnished with a stopcock. The stopcock is then closed, and an air-pump connected with the vessel is worked until as perfect a vacuum as possible is obtained within the vessel. The cock is then again opened to allow the solution to fill the vessel nearly. It is then shut, and by means of a force pump a further quantity of solution is introduced, until the pressure on the interior of the vessel amounts to 110 lbs. to 140 lbs. per square inch. This pressure is maintained for an hour, and the solution is then drawn off. The vegetable matter is then impregnated in a similar manner with an acid or a solution of some substance, such as sulphate of iron, which will unite with the barium or calcium, and set the sulphur free. When the vegetable matter is to be impregnated with a large quantity of solid matter it should be dried between the application of the two fluids. By this means an insoluble sulphate of lime or sulphate of barium is formed in the body of the wood, which is thus rendered nearly as hard as stone. Wood so prepared is now largely employed in English public works and railways. Of this same process Payne himself says: "By impregnating wood with a solution of metallic oxides, alkalies, and earths in various proportions, using exhaustion and pressure to do it, and then in order to prevent the disunion of such solutions by introducing another solution by a similar means, an insoluble substance is formed in the interstices of the woods. Wood subjected to this process is not only proof against wet and dry rot in every situation, but will not communicate flame, and will resist the attacks of insects. The most porous, the softest, and, of course, the cheapest woods are rendered equal in point of usefulness, durability and strength to the hardest and best description of timber. Wood thus prepared is susceptible of the finest polish. As a preventative to the spread of flame, especially in countries where the houses are for the most part composed entirely of wood, and in most cases covered with wood shingles—the use of this process will be of the utmost importance, and will greatly lessen, if not entirely prevent, the dreadful catastrophe so frequently occurring there from fire. For all outdoor work, to whatever inclemency of the weather it may be exposed, the advantages are great, and also for sleepers and other works on railways, as also for wood pavement. For canvas and cordage, by the proper application of this process, all the advantages named are communicated without injury to their elasticity. Sails, rigging of ships, canvas for tents, tarpaulings, &c., are by this means effectively protected and improved." Again, "Herbert's Encyclopædia" says: "Many ingenious experiments have been resorted to to render wood fireproof. Solutions of muriate of ammonia, muriate of soda, sal ammoniac, borax, alum, and several other salts and alkalies have this property to a certain extent. Professor Fuchs invented a solution for this purpose—10 parts potassa, or soda; 15 parts of pure silicious earth, and 1 part charcoal mixed with water. This composition, applicd to the surface of the wood, forms a vitreous coat, which

effectually resists the action of fire. Decisive experiments fully established the efficacy of this plan, and the Royal Theater of Munich was protected by the application of this composition. The surface covered was upwards of 400,000 square feet, and the expense, it is said did not exceed 5,000 francs—£200, or less than 4s. for 400 square feet. The following is an English composition for a like purpose: 1 part, by measure, of fine sand, 2 parts of wood ashes, and 3 parts slaked lime ground together with oil, and laid on with a painter's brush; the first coat thin, and the second thick. This forms a very strong and adhesive compound, which is both fireproof and waterproof. Again, Solomon's patent consists in a peculiar application of two solutions to the surface of the wood, the first consisting of sulphate of alumina, glue and water, and the second of chloride of calcium, glue and water." Also, in "Spon's Workshop Receipts": "A wash composed of lime, salt, and fine sand, or wood ashes, put on in the ordinary way of whitewash, renders a shingle roof fifty-fold more safe from fire from falling cinders in case of fire in the vicinity. It has also a preserving influence against the effect of the weather. The older and more weather-beaten the shingles the more benefit derived. Such shingles are generally more or less warped, rough and cracked. The application of the wash by washing the upper surface restores them to their original or firm form, thereby closing the space between the shingles and the lime and sand; filling up the cracks prevents it from warping. By the addition of a little lampblack, the wash may be made of the same color as old shingles, and thus remove the offensive glare of a whitewashed roof." Such is the information we are able to glean, so far; and before offering any suggestion of our own as to the best mode of proceeding, let us remember that to season timber in the ordinary way requires seldom less than three years', often six or eight years' exposure to the air freely; and though (owing to the suffocating fumes emitted when exposed to great heat) Kyan's (chloride of mercury) is out of the question for us, still the burnettizing (chloride of zinc), boucherizing (sulphate of copper), and beerizing (borax) systems the destructive principle (sap) is dried and rendered inert. They render larch, firs of all kinds, willow, birch, elm, beach, ash, poplar, &c., of considerable value for durable purposes. We would, therefore, suggest for houses already built: Apply several washings of silicate of soda to the fixtures of every description, and let the removable lighter work, roof shingle (when used), mats, &c., soak several days in the same solution. Where shingle roofs are used, let these be afterwards coated with lime, salt, fine sand, or wood ashes. When new houses are to be built, impregnate the main or thick timbers thoroughly with chloride of zinc by pressure, obtained as in boucherizing, whilst the timber is green; allow it to dry thoroughly before fixing, and paint the outside with silicate of soda three times when in position. The lighter woodwork, shingles, &c., can be cut from large balks thus impregnated, and afterwards washed superficially with silicate of soda, or this thin woodwork may be saturated by steeping in silicate of soda for several days. By the use of the above comparatively simple and inexpensive remedies, all complicated steam-pressure paraphernalia, vacuum pumps, &c., are avoided. The area of fires would be greatly reduced, whilst a scheme of fire assurance would become feasible where these precautions were taken by the inhabitants of any block of buildings, as any spread of a conflagration would be arrested and kept in check at such a small loss to the outside houses of the block, as would fall lightly on any company holding an assurance on the whole block. The inhabitants of any block of houses might assure against fire amongst themselves, on condition that the above precautions were observed by all, whilst any jealousy on account of vested interests of firemen might be met by paying a certain sum, to be approved by former statistical returns, to fire brigades, and deducting according to the number and extent of fires, not to pit any district fire brigade against another for a prize, as that would only lead to a prevalence of fires (accidental, of course) in opposing districts.

THE Columbo breakwater is finished, and the Ceylon *Observer* advocates the construction of a northern arm to the breakwater and docks.

ABSTRACT OF RESULTS OF EXPERIMENTS ON RIVETED JOINTS, WITH THEIR APPLICATION TO PRACTICAL WORK.*

By Prof. ALEXANDER B. W. KENNEDY.

From "The Engineer."

The experiments, of which the following is an abstract, consist of fourteen series, and cover in all 290 experiments, 64 on perforated—punched and drilled—plates, 97 on actual joints, 44 on the tenacity of the plates used in the joints, 33 on the tenacity and shearing resistance of the rivet steel used in the joints, and the rest on various other matters connected with them. All the joints in the whole series were supposed to be made of the same material, a soft steel or ingot iron, supplied from the Landore-Siemens Steel Works; and that all the rivets were made from rivet steel supplied from the same works.

Having summarized the experiments themselves, the author proceeds to summarize the conclusions which it appears safe to draw from them, to examine the proportions of joint which they appear to indicate as the best, and to mention the points on which further information is now being obtained. In accordance with the plan pursued in the rest of this abstract, this will be done in the briefest possible manner, without discussing the points at length as they occur. In most cases a much more detailed treatment of them will be found in the various reports of the riveting committee, of which this is only an abstract. It will be remembered, and may be again stated here once for all, to avoid repetition, that the conclusions given below all refer to joints made in soft steel plate with steel rivets, that the holes were all drilled, and that the plates were in their natural state—unannealed. Further, it should be said, that all dimensions, thicknesses of plate, &c., were measured by the most accurate means available; and in every case the rivet or shearing area has been assumed to be that of the holes, not the nominal—or real—area of the rivets themselves. Also in every case the strength of the metal in the joint has been compared with that of strips cut from the same plates, and not merely with, nominally similar material. It is thought that, if these points had always been attended to, many of the discrepancies in published riveted-joint experiments would never have appeared.

(1). The metal between the rivet holes has a considerably greater tensile resistance per square inch than the unperforated metal. This excess tenacity amounted to more than 20 per cent.—both in ⅜ inch and ¾ inch plates—when the pitch of the rivet was about 1.9 diameter. In other cases ⅜ inch plate gave an excess of 15 per cent. at fracture with a pitch of 2 diameters, of 10 per cent. with a pitch of 3.6 diameters, and of 6.6 per cent. with a pitch of 3.9 diameters; and ¾ inch plate gave 7.8 per cent. excess with a pitch of 2.8 diameters.

(2). The shearing resistance of the rivet steel is a matter upon which, as has been pointed out, further experiment is required. It may be taken as established that the resistance per square inch in double shear is as great as that in single shear, so that allowance need not be made for the two shearing planes not being equally stressed. In single-riveted joints, however, the bending of the plates will put considerable tensile stress in the rivets; and this may diminish their apparent shearing resistance. In single-riveted joints it may be taken that about 22 tons per square inch is the shearing resistance of rivet steel,* when the pressure on the rivets does not exceed about 40 tons per square inch. In double-riveted joints, with rivets of about ¾ inch diameter, most of the experiments gave about 24 tons per square inch as the shearing resistance, but the joints in Series XIII. went at 22 tons. In Series XIII. the larger rivets also went at a low load; but in the other double-riveted joints, with larger rivets, these latter remained unbroken at a stress of 22 tons per square inch.

* Abstract of paper read before the Institution of Mechanical Engineers.

* In one pair of single-riveted joints only a shearing resistance of over 24 tons per square inch was reached; in none of the other did it exceed 22.5 tons.

(3) The size of the rivet heads and ends plays a most important part in the strength of the joints—at any rate in the case of single-riveted joints. An increase of about one-third in the weight of the rivets—all this increase, of course, going to the heads and ends—was found to add about $8\frac{1}{2}$ per cent, to the resistance of the joint, the rivets remaining unbroken at twenty-two tons per square inch instead of shearing at a little over twenty tons.* The additional strength is, no doubt, due to the prevention of so great tensile stress in the rivets through distortion of the plates.

(4) The strength of a joint made across a plate is equal to that of one made in the usual direction. (Both this conclusion and the last preceding are stated as the result of a very limited number of experiments; but there seems no reason to doubt their general truth.)

(5) The intensity of bearing pressure on the rivets exercises, with joints proportioned in the ordinary way, a very important influence on their strength. So long as it does not much exceed 40 tons per square inch—measured on the projected area of the rivets—it does not seem to affect their strength; but pressures of 50 to 55 tons per square inch seem to cause the rivets to shear in most cases at stresses varying from 16 to 18 tons per square inch. This conclusion is based on the experiments of Series X., in which the margin was made equal to the diameter of the drilled hole. For ordinary joints, which are to be made equally strong in plate and in rivets, the bearing pressure should, therefore, probably not exceed 42 or 43 tons per square inch. For double-riveted butt joints, perhaps, as will be noted later, a larger pressure may be allowed, as the shearing stress may probably not exceed 16 or 18 tons per square inch when the plate tears. But in this case it would probably be wise to increase the margin.

(6) A margin—or net distance from outside of holes to edge of plate—equal to the diameter of the drilled hole, has been found sufficient in all cases hitherto tried.

(7) To attain the maximum strength of a joint, the breadth of lap must be such as to prevent it from breaking zigzag. Such a method of fracture must inevitably be accompanied by unequal stresses in the plate straight between the rivet holes, and by consequent diminution of strength. It has been found that the net metal measured zigzag should be from 30 to 35 per cent. in excess of that measured straight across, in order to insure a straight fracture. This corresponds to a diagonal pitch of $\frac{2}{3}p+\frac{d}{3}$, if p be the straight pitch and d the diameter of the rivet hole. To find the proper breadth of lap for a double-riveted joint, it is probably best to proceed by first setting this pitch off, and then finding from it the longitudinal pitch or distance between the centers of the lines of rivets.

(8) Visible slip or "give" occurs always in a riveted joint at a point very much below its breaking load, and by no means proportional to that load. A careful collation of all the results obtained in measuring the slip indicates pretty clearly that it depends upon the number and size of the rivets in the joint, rather than anything else, and that it is tolerably constant for a given size of rivet in a given type of joint. The loads per rivet at which a joint will commence to slip visibly are approximately as follows:

Rivet diameter.	Type of joint.	Riveting.	Slipping load per rivet.
$\frac{3}{4}$ inch	Single riveted	Hand	2.5 tons
"	Double riveted	"	3.0 to 3.5 tons
"	"	Machine	7 tons
1 inch	Single riveted	Hand	3.2 tons
"	Double riveted	"	4.8 tons
"	"	Machine	8 to 10 tons

To find the probable load at which a joint of any breadth will commence to slip, it is only necessary to multiply the number of rivets in the given breadth by the proper figure taken from the last column of the table above. It will be understood that the above figures are not given as exact; but they represent very well the results of the experiments in all series from VIII. to XIII., except Series X., in which the average—for 1 inch rivets—was much lower than that given above. In this series, however, the proportions of the joints were intentionally somewhat abnormal; and it is, perhaps,

* See "Proceedings," 1881, pp. 713, 714.

not to be expected that in this respect their results should agree with those of the other experiments. This result as to the slipping of a joint, although, perhaps, unexpected, is not contrary to what ought to have been expected. For experiments show that, long before stresses are reached which could visibly stretch the plates of a joint, there will be quite measurable shear of the rivet. The visible slip, therefore, will consist almost wholly of this shear, the magnitude of which will depend primarily on the number and size of the rivets in the joint. Anything that will hold the plates up better together, such as hydraulic pressure on the rivets, might be expected to diminish this shear or delay its commencement—exactly as seems to have happened. The following table gives the results of experiments on this matter which were made along with those given in the Committee's first Report, but which have not been previously published in the "Proceedings" of the Institution. The experiments are on 1 inch turned pins of rivet steel, tested in the single-shear apparatus already described. Of course, the shear would commence later, and be at first smaller in extent, when the pin was replaced by an actual rivet, and when the plates were thus forcibly held together, instead of being quite free to slide, except as far as held from motion by the resistance to shear.

(9) The value of machine-riveting, as compared with hand-riveting, in cases where sound hand-riveting is possible, lies mainly, if not entirely, in the fact that it doubles the load at which the slip of a joint commences. This conclusion is subject to modification by future experiments with the use of higher pressures in closing the rivet, which may probably still further raise the slipping load, so that the advantage of machine-riveting may quite possibly be even greater than it is here assumed to be; but there is no indication that it is likely to affect the ultimate strength of the joint. The question of *friction* in the joint, which has not been specially experimented on by the committee, no doubt comes in in the same way. The friction induced by the rivet will affect the point at which slip commences, but can hardly have much, if any, relation to the breaking load. It is thought that the load at which visible slip commences is probably proportional to the load at which leakage would begin in a boiler. Looked at this way, it will be seen that the great value of hydraulic riveting appears to lie rather in the increased security and stiffness it gives at ordinary working loads than in any actual raising of the breaking load. From a practical point of view the former is probably the more, and not the less, important function. Further experiments are now in

Shearing Stress in lbs. per square inch.	Test Numbers.					
	343	344	345	346	347	348
	Amount of Shear in Inches.					
0	0.0	0.0	0.0	0.0	0.0	0.0
6,365	.010	.013	.016	.022	.055	.021
12,730	.022	.028	.030	.034	.066	.032
19,100	.034	.040	.042	.048	.078	.043
25,460	.055	.060	.060	.071	.091	.062
28,320	.066	—	—	—	—	—
31,830	.080	.086	.082	.091	.118	.183
35,010	.093	—	—	—	—	—
38,190	.113	.113	.108	.114	.140	.108
41,380	.141	—	—	—	—	—
44,550	.168	.152	.142	.170	.171	.155
47,740	.200	—	—	—	—	—
50,910	.242	.200	.196	.248	.238	.222
54,110	—	—	—	—	—	—
Breaking load per sq. inch } lbs....	54,110	54,930	55,240	52,830	56,670	53,530
Breaking load per sq. inch } tons...	24.15	24.52	24.66	23.59	25.29	23.90

progress to test the effect of higher closing pressures on the rivet than were used in Series XII. and XIII., the results of which, it is hoped, will allow more definite conclusions to be arrived at in respect to the comparative merits of hydraulic and hand riveting.

(10) The experiments point to very simple rules for the proportioning of joints of maximum strength, which will be mentioned before any other joints are discussed. Assuming that a bearing pressure of 43 tons per square inch may be allowed on the rivet, and that the excess tenacity of the plate is 10 per cent. of its original strength,* the following short table gives the values of the ratios of diameter d of the hole to thickness t of plate $\left(\frac{d}{t}\right)$ and of pitch p to diameter of hole $\left(\frac{p}{d}\right)$, in joints of maximum strength in ⅜-inch plate:

Original Tenacity of Plate. Tons per Sq. Inch.	Shearing Resistance of Rivets. Tons per Sq. Inch.	Ratio $\frac{d}{t}$	Ratio $\frac{p}{d}$	Ratio $\frac{\text{Plate area}}{\text{Rivet area}}$
30	22	2.48	2.30	0.667
28	22	2.48	2.40	0.785
30	24	2.28	2.27	0.718
28	24	2.28	2.36	0.690

Summed up and rounded off, this means that the diameter of the hole—not the diameter of the rivet cold—should be two and a-third times the thickness of the plate, and the pitch of the rivets two and three-eighth times the diameter of the holes.† In mean also it makes the plate area 71 per cent. of the rivet area. If a smaller rivet be used than that here specified, the joint will not be of uniform and therefore not of maximum strength; but with any other size of rivet the best result will be got by use of the pitch obtained from the formula formerly cited,

$$p=a\frac{d^2}{t}+d$$

where, as before, d is the diameter of the hole. The value of the constant a in this equation is as follows:

For 30-ton plate and	22-ton rivets,	a=0.524
"	28	"	22	"	0.558
"	30	"	24	"	0.570
"	28	"	24	"	0.606

or in the mean, the pitch $p=0.56\frac{d^2}{t}+d$.

It should be noticed that with too small rivets this gives pitches often considerably smaller in proportion than 2⅜th times the diameter. For double-riveted lap joints a similar calculation to that given above, but with a somewhat smaller allowance for excess tenacity on account of the large distance between the rivet-holes, shows that for joints of maximum strength the ratio of diameter to thickness should remain precisely as in single-riveted joints; while the ratio of pitch to diameter of hole should be 3.64 for 30-ton plates, and 22 or 24-ton rivets, and 3.82 for 28-ton plates with the same rivets. Here, still more than in the former case, it is likely that the prescribed size of rivet may often be inconveniently large. In this case the diameter of rivet should be taken as large as possible; and the strongest point for a given thickness of plate and diameter of hole can then be obtained by using the pitch given by the equation

$$p=a\frac{d^2}{t}+d,$$

where the values of the constant a for different strengths of plate and rivet may be taken as follows: [See table next page.]

Practically we may say that, having assumed the rivet diameter as large as possible, we can fix the pitch as follows, for any thickness of plate from ⅜ in. to ¾ in.:—

For 30-ton plate and 24-ton rivets
" 28 " 22 "

$$p=1.16\frac{d^2}{t}+d$$

For 30-ton plate and 22-ton rivets

$$p=1.06\frac{d^2}{t}+d$$

For 28-ton plate and 24-ton rivets

$$p=1.24\frac{d^2}{t}+d$$

* The excess strength is taken lower than the average result of the experiments, because it is probable enough that the steel used had more than the average softness.

† The small difference here from the constants formerly given is due to the assumption, now quite justified, of a somewhat greater bearing pressure than was then allowed.

Table of Proportion of Double-riveted Lap Joints, in which

$$p = a\frac{d^2}{t} + d.$$

Thickness of Plate.	Original Tenacity of Plate. Tons per Sq. Inch.	Shearing Resistance of Rivets. Tons per Square Inch.	Value of constant a.
⅜-inch	30	24	1.15
"	28	24	1.22
"	30	22	1.05
"	28	22	1.12
¾-inch	30	24	1.17
"	28	24	1.25
"	30	22	1.07
"	28	22	1.14

In double-riveted butt joints it is impossible to develope the full shearing resistance of the joint without getting excessive bearing pressure, because the shearing area is doubled without increasing the area on which the pressure acts. In the writer's last report it was shown that, considering only the plate resistance and the bearing pressure, and taking this latter as 45 tons per square inch, the best pitch would be about four times the diameter of the hole. It appears justifiable, however, to apply here the results of Series X., and take corresponding constants. Thus we may probably say with some certainty that a pressure of from 45 to 50 tons per square inch on the rivets will cause shearing to take place at from 16 to 18 tons per square inch. Working out the equations as before, but allowing excess strength of only 5 per cent. on account of the large pitch, we find that the proportions of double-riveted butt joints of maximum strength under given conditions are those of the following table:

Original Tenacity of Plate. Tons per Sq. Inch.	Shearing Resistance of Rivets. Tons per Sq. Inch.	Bearing Pressure. Tons per Sq. Inch.	Ratio $\frac{d}{t}$	Ratio $\frac{p}{d}$
30	16	45	1.80	3.85
28	16	45	1.80	4.06
30	18	48	1.70	4.03
28	18	48	1.70	4.27
30	16	50	2.00	4 20
28	16	50	2.00	4 42

Practically, therefore, it may be said that we get a double-riveted butt joint of maximum strength by making the diameter of hole about 1.8 times the thickness of the plate, and making the pitch 4.1 times the diameter of the hole. These are very nearly the proportions which were used for the ⅜ inch joints in Series XI. to XIII.; for the ¾ inch joints the diameter of the rivet was, as with the lap joint, less than that indicated by theory. In thick plates, where it is thought impossible or inconvenient to make the rivet-holes so large as 1.8 times the thickness, the best pitch for any assumed diameter of rivet cannot be found by the method formerly used; for here we have not a given maximum shearing stress to work to, but rather the shearing stress which in a given joint causes a given maximum pressure on the rivets. The best ratio of pitch to diameter of hole in double-riveted butt joints of maximum strength for any assumed diameter of hole, d, is, therefore, the same as that given in the last table, or in mean, 4.1.

(11). All the experiments hitherto made have necessarily connected themselves with the question of strength, and the proportions just given belong to joints of maximum strength. But in a boiler the one part of the joint, the plate, is much more affected by time than the other part, the rivets. It is therefore not unreasonable to estimate the percentage by which the plates might be weakened by corrosion, &c., before the boiler would be unfit for use at its proper steam pressure, and to add correspondingly to the plate area Probably the best thing to do in this case is to proportion the joint not for the actual thickness of plate, but for a nominal thickness less than the actual by the assumed percentage. In this case the joint will be approximately one of uniform strength by the time it has reached its final workable condition; up to which time the joint as a whole will not really have been weakened, the corrosion only gradually bringing the strength of the plates down to that of the rivets. Thus, suppose a single-riveted lap joint in ⅝ inch plate is in question, and it is considered that corrosion will make this equal to only ½-inch plate before the boiler pressure has to be lowered The rivet should then be proportioned as if the plate had a thickness of 0.5 inch, which

would give for 30-ton plate and 22-ton rivets (see Table, preceding page) a diameter of hole of 1.24 inch. Assume this as too large to be convenient, and take the diameter of hole as 1 inch. Then from the Table, preceding page, the pitch will be

$$p=\frac{0.524}{0.5}+1=2.05 \text{ inches.}$$

The ratio of plate to rivet area to start with will be 0.835, which means, of course, that the plate is in excess; but the ratio will diminish until it reaches 0.667, when the strength of the plate has become equivalent to that of one only $\frac{1}{2}$ inch thick, as was required. The efficiency of the joint would be 45 per cent., whereas the best efficiency of a joint in $\frac{5}{8}$ inch plate with 1 inch holes (p=1.84 inch) would be 50 per cent., and the best possible efficiency of a single-riveted lap joint in $\frac{5}{8}$ inch plate under the given condition of strength would be about 62 per cent. It is hardly necessary to point out how strongly these figures indicate the necessity of using as large rivets as possible, and of taking every possible means to reduce the allowance necessary for corrosion. For a boiler, such as has just been discussed, is absolutely no stronger than one of $\frac{1}{2}$ inch plate throughout, if only the thickness of the latter could be kept unreduced at the joints.

(12). There are now in hand for the Riveting Committee further experiments on double-riveted joints of the general types already tested, in $\frac{3}{8}$ inch, $\frac{3}{4}$ inch and 1 inch plate, designed specially to throw light upon the questions of hydraulic and hand-riveting, high and low-pressure hydraulic-riveting, and the practical value of exceptionally large rivets. They will also give further information as to the slip of joints, and other points already discussed; and may further, it is hoped, be made use of to throw some light on rather more obscure problems—such as those raised recently by Mr. Milton at the Institution of Naval Architects—connected with the stress in the metal of the plate in the neighborhood of the joint.

Although this paper is an unofficial abstract of the result of experiments only, and not an official summary of the whole work of the Riveting Committee, the author may be allowed to call attention, in conclusion, to other memoirs, not reports on experiments, which have been prepared in connection with the work of the committee. Of these the earliest, and by far the most important, is the admirable summary of the published results obtained up to the time when the committee commenced work, by Professor W. Cawthorne Unwin, and published in the "Proceedings" for 1881, pp. 301-368. This paper, along with many valuable suggestions made by its author at the time, formed really the foundation of the whole work of the committee. The table compiled by Mr. Ralph H. Tweddell, showing rules of practice used by manufacturers for riveted joints in iron, published in the "Proceedings" for 1881, pp. 293-299, has proved very instructive. In addition to this, Mr. Tweddell has contributed some remarks on hydraulic riveting to the last report of the committee just issued to members; and Mr. W. Silver Hall has added in the same place a collation of Mr. C. H. Moberly's experiments and a few others, with those of the committee.

Disintegration of Building Stone.—The sandstone commercially known as freestone, which is extensively used for building purposes in American cities, is subject to disintegration from the action of the sulphurous acid produced by the consumption of coal and from frost. There is much difference in the ability of various quarries to withstand these destructive influences. The outer surfaces of some buildings in New York and Philadelphia have been, by the advices of an eminent chemist, treated with a mixture of paraffin and carbolic acid with apparently good results. The flat surfaces are warmed by means of a stove like a plumber's stove, but with a flat side, and the paraffin when applied in a melted condition penetrates the stone readily, it is said that in some instances to the depth of $1\frac{1}{2}$ in. Mouldings and carved work are heated by means of a blast flame from india-rubber bags of illuminating gas. Another process has been suggested, but the preliminary results do not appear to be of a satisfactory nature on account of its tendency to crack. In this process the mixture used is an artificial stone, and consists of three parts glass sand, three parts broken marble, two parts anhydrous clay, and two parts freshly slaked lime still warm. After a coat of the above has been applied wash it with water on the following day. The central portion and wings of the Capitol building at Washington were originally built of freestone, which disintegrated so rapidly as to threaten the permanence of the structure, and the whole was protected by several coats of white paint. The wings afterwards added to the above and now used for their House of Representatives and Senate Chamber, are built of white marble, which conforms in color to the central portion of the building, so that the whole building appears to be made of marble.

THE UTILIZATION OF A NATURAL CHALYBEATE WATER FOR THE PURIFICATION OF SEWAGE.

By JOHN C. THRESH, D.Sc.

From the "Journal of the Society of Arts."

The removal of organic matter from water-carried sewage is a subject of such extreme importance, that a description of any process actually being employed, whether completely or in part successful, cannot but be acceptable to all who are interested in sanitary matters. On this account, I have undertaken to lay before you this evening particulars of a process of a somewhat novel character, now being worked at one of our inland health resorts, Buxton, Derbyshire.

The novelty of the process lies in the fact that the waters of a chalybeate spring, flowing from an old coal mine in the district, are utilized for effecting the precipitation and purification. Until after the public opening of the works in question, I was unaware of any similar system of sewage treatment having been attempted. Since then, it has been brought to my notice that at Prestwich, near Manchester, an iron water from a neighboring colliery is caused to mix with the sewage from some forty cottages, and after passing through a number of tanks, the effluent runs into the brook. Whether anything is added besides the iron water I have not been able to ascertain, neither do I know whether the condition of the sewage and effluent has ever been reported upon.

The Buxton sewage is entirely "domestic" in character, there being no factories or works of any kind turning their waste products into the drains. The amount probably averages over 400,000 gallons daily, but varies very considerably, sometimes sinking as low as 300,000 gallons, and at other times rising to 1,000,000 gallons. Considering that the population of the place is only 7,000, these amounts appear enormous, but it must be remembered that during the season (May to November) there are constantly from 1,000 to 5,000 visitors also in the town. Besides this, most of the water from the springs, for which Buxton is famous, finds its way into the sewers, certainly not less than 100,000 gallons from this source being added daily to the sewage, and a considerable amount of storm water also passes into the drains and assists in diluting the sewage. Were it not that during storms the flooding of the sewers brings down an immense amount of offensive matters, the sewage at such times would be excessively dilute. At the present time no change in the drainage is contemplated, but should it appear desirable for any reason to diminish the volume of sewage to be treated, the whole of the bath water could be easily diverted into the river.

Several schemes for purifying—or at least clarifying—the sewage have been tried and abandoned, and for some years the whole has been allowed to run directly into the river without being subject to any treatment whatever. This river, the Derbyshire Wye, rises only a little distance beyond the town through the middle of which it flows; and as in dry seasons the volume of water in the stream is fully doubled by the addition of sewage, its condition in the hot weather is better imagined than described. The adoption of the process of treatment with iron-water and lime, an account of which I am now about to give, has resulted in considerably improving the character of the stream, and there is little doubt that this river will henceforth cease to create a nuisance in the lovely valley through which it flows.

Briefly stated, the sewage is treated by mixing with about one-third of its volume of a natural chalybeate water, to which a certain amount of milk of lime has been added, and allowing the mixture to flow into a series of tanks, in which the precipitated matter collects, whilst the clear effluent flows over a weir at the end into the river.

It will probably conduce to lucidity if the whole process, &c., is described systematically, and in the following order:

1. Construction of the works.
2. The chalybeate water.
3. Action of this water on sewage.

4. The sewage before and after treatment.
5. The matter precipitated (sludge).
6. Cost of construction and maintenance of works.

1. *Construction of the Works.*—The following brief account of the works is taken from a report of Mr. Hague, Assoc. Inst. C.E., the town surveyor, from whose plans and under whose supervision the whole has been constructed:

The iron-water employed is conveyed by gravitation in especially made earthenware tubes, from a disused colliery at the foot of the Axe Edge hills. At a short distance from the "heading" entrance to the colliery, the iron-water enters a brick receiving tank, constructed on the edge of the brick course nearest the colliery, and is conveyed across the brook in 9-inch metal pipes supported on stone piers. Thence it takes a northeasterly direction to the site of the works, a length of 2 miles 163 yards, and enters a second tank at the rear of the liming rooms adjoining the works, which are situated between the River Wye and the Midland Railway, in Ashwood Dale.

The liming and mixing rooms are erected over the River Wye, supported by a stone semi-circular arch, the liming room floor being on a level with the adjoining highway, and connected with the Board's sidings on the Midland Railway.

The lime required for precipitation purposes is conveyed from the sidings alluded to to the hopper of a patent liming machine. A cistern of 800 gallons capacity receives the pulped lime from the machine, and is supplied with an agitating apparatus, to keep the lime required during the night of a uniform and suitable consistency. Both the machine and agitating apparatus, &c., are driven by an "overshot" water-wheel, 16 ft. in diameter and 3 ft. wide. The water for driving purposes is taken from the Wye, about 500 yards higher up the stream, and conveyed in large sanitary tubes.

Immediately outside the liming and machinery rooms are constructed duplicate brick tanks, into which the main outlet sewer discharges. The tanks are furnished with wrought-iron screening wagons, for the purpose of abstracting the solid and floating matter. After passing through the screening wagons the sewage runs through a brick conduit into a circular metal chamber, furnished with horizontal paddles, where the iron, lime, and sewage are thoroughly mixed. From here the sewage flows a distance of 50 yards, through an earthenware conduit, to the settling tanks, consisting of two sets so arranged as to work either singly or together. Those tanks are constructed of brick walls set in cement, with concrete bottoms, the walls being coped with dressed local grit stone. The length of the tank is 266 ft., and width 73 ft., and they are capable of holding 400,000 gallons. The formation of the tank bottom is of original design, being 3 ft. 6 in. deeper at the entrance than the outlet, an arrangement which has fully met the object for which it was introduced, that is to retain the sludge at the inlet end of the tanks. The first of those tanks is formed with a brick division wall six feet from the inlet, supported on arches of a similar material, under which the sewage flows into a second tank, and thence through the entire series of tanks, with a barely perceptible motion, to the effluent weir sill.

In the center of the main division wall, at the inlet end of the tanks, a triangular well is constructed for cleansing purposes, and is supplied with duplicate iron run-off cloughs, so arranged as to remove what water remains, owing to the extra depth of the tanks at that end, after the suspended matter has subsided. Outside the entrance tank is a sludge well, fitted witth a strong chain pump, driven by water power, and with cloughs so arranged as to remove the sludge. It should be stated that the bottom of the tanks is formed with a longitudinal and transverse inclination, with the view of expediting the cleansing process, and minimizing manual labor.

2. *The Chalybeate Water.*—The mineral water mentioned as being utilized in this process of sewage purification is derived from a spring which arises in the so-called "old level" mine at Burbage, about two miles beyond Buxton, a mine driven in one of the beds of shale between the mountain limestone and the Yoredale rocks. Until recently diverted, the water flowed into the Wye, very near its source, covering the bed of the river for some distance with a yellow deposit of ochre. The flow of water varies somewhat considerably, but even in the driest seasons

there is always an abundant supply, probably never less than 100,000 gallons in the 24 hours. The amount of salts in solution is almost as variable as the flow, undoubtedly due to the fact that the stream in its course from its origin in the mine, to the point where it issues from the hill side, is diluted with water which percolates through the strata above, the amount of such dilution varying of course with the rainfall.

The water has always a faintly opalescent appearance, and if exposed to the air an ochery deposit very rapidly subsides. The sample which I submitted to careful analysis, and the results of which are now given, was collected during a somewhat wet summer, but at a time when no rain had fallen for several days previously. As besides sulphates the water contains only a small proportion of carbonates and a trace of chlorides, there is little room for theorizing as to the nature of the salts in solution.

Each gallon was found to contain—

	Grains.
Silica	1.44
Ferric oxide	.28
Ferrous sulphate	8.30
Aluminic "	1.26
Magnesium "	7.44
Calcium "	9.01
Sodium "	1.99
Ferrous carbonate	8.22
Sodium Chloride	.76
	28.70

[The total iron corresponds to 14.8 grains of the crystalline sulphate, and the alumina to 3.6 grains of potash alum.]

From the estimations of the iron made at various times, I find that, as a rule, the water is not nearly so strongly chalybeate as this sample, but the analysis gives us an insight as to the nature of the constituents and their relative proportions. It may not be without interest to mention that on the other side of Axe Edge, the hill from which this spring arises, there is another chalybeate spring very much more powerful, containing in each gallon 299 grains of solid matter, of which 174 grains are ferric sulphate, and 73 grains aluminic sulphate. The flow, however, is not nearly so considerable, and to convey it to Buxton would have been a task of some magnitude.

3. *Action of the Chalybeate Water on Sewage.*—When the iron-water is mixed with from two to four volumes of (Buxton) sewage and allowed to stand, a deposit slowly forms, but even after many hours the mixture does not become perfectly clear, in fact it remains cloudy for days. An examination of the supernatant fluid shows that a considerable amount of organic matter remains suspended and in solution, but that some has been removed.

If, however, lime be mixed in proper proportions with the iron-water and sewage, then a more or less flocculent precipitate at once forms, the rapidity with which this settles depending upon the order in which the ingredients are mixed, and the amount of agitation received. The best results in the laboratory were uniformly obtained by adding lime in the proportion of 15 grains to one gallon of the ultimate mixture to the iron water, and then pouring this into the sewage, and stirring gently for half-a-minute to a minute. Under these circumstances, the flocculæ first formed aggregate together, and fall to the bottom of the receptacle with the utmost rapidity. If the stirring be neglected, the flocculæ are small and subside very slowly; if the agitation be too violent, these large flakes are either broken up or prevented from forming, and clarification is again retarded. Analysis of the effluent, when clear, invariably proves the greater part of the impurities to have been removed in the precipitate. As showing the marked improvement affected by addition of the lime, the result of a typical experiment may be quoted.

For fifteen hours the sewage and iron-water, unmixed with lime, were allowed to flow through the tanks, and at the expiration of that time, a sample of the effluent (1) was taken. The liming machine was now set in motion, and after twenty-four hours, another sample of effluent (2) collected. The results of the examination of the sewage and the two effluents are here compared.

	Organic Ammonia.	Free Ammonia.
	In parts per Million.	
Sewage (mean)	5.	13.
Effluent (1) turbid	1.7	5.
" (2) clear	.7	3.9

Working at the tanks, it is found that less lime is required to effect clarification than had been calculated to be necessary from laboratory experiments. As nearly

as can be ascertained, twelve grains of slacked lime to a gallon of mixed iron-water and sewage secures efficiency, and is the proportion now being added. An excess of lime is distinctly prejudicial; a flocculent precipitate forms, and begins to subside, then the whole volume of fluid gradually becomes opalescent (apparently from formation of calcium carbonate), and then only clears after a considerable lapse of time.

4. *The Sewage before and after Treatment.*—To illustrate the character of the sewage and of the effluent produced, the results of the daily examination of both for one week have been tabulated. The samples were collected alternately in the morning and afternoon:—

RESULTS EXPRESSED IN GRAINS PER GALLON.

	Total solids.	Alb. NH_3	Free NH_3		Total solids.	Alb. NH_3.	Free NH_3.
Sewage.	..	.31	.83	Effluent.	23.	.05	.19
"	37.	.33	1.05	"	28.	.05	.51
"	37.	.22	.65	"	25.	.04	.18
"	51.	.36	1.16	"	28.	.07	.24
"	39.	.23	.76	"	27.	.06	.27
"	56.	.32	1.01	"	26.	.04	.38
"	37.	.26	1.12	"	27.	.04	.17
Mean...	43.2	.29	.94	Mean ...	26.3	.05	.28

A more complete analysis of a typical sample of sewage and of the corresponding effluent, yielded the following results:

	Sewage. Grains per gallon.	Effluent. Grains per gallon.
Total solids (solid at 212°F.)	36.1	26.5
Loss on ignition	18.3	5.5
Chlorine	3.	2.2
Free ammonia	.60	.24
Alb. "	.22	.04

	Sewage. Grains per gallon.	Effluent. Grains per gallon.
Hardness, temporary	10.	6.
" permanent	4.	11.
Total	14.	17.

Nitrates and nitrites present in only small quantities in both.

Upon comparison, it will be noticed that in the sewage the total solids varied from 37 to 56 grains per gallon, with an average of 43 grains; in the effluent from 23 to 28, with an average of 26. In the sewage the albumenoid ammonia varied from .22 to .36 grains, with an average of .29; in the effluent, from .04 to .07, with an average of .05. The free ammonia in the sewage varied from .65 to 1.2 grains, average .94; in the effluent, from .17 to .51, the average being .28.

With regard to the alkalinity of the effluent, I have refrained from expressing this numerically, for fear of fostering a misconception. If by alkalinity we mean presence of uncombined alkali (ammonia and lime), then I have never observed the sewage to be alkaline. On no occasion has an alkaline reaction been indicated on adding a little phenol-phthalein as indicator; on the contrary, it has invariably been necessary to add more or less alkali to produce the pink tint. Moreover, the presence of calcium carbonate is always demonstrable by comparing the permanent with the total hardness.

When an indicator, such as methyl-orange, is employed, no acid reaction is exhibited; indeed, a certain amount of acid must always be added before coloration is produced. This, however, does not prove the presence of free alkalies, inasmuch as with such indicators no reaction is obtained until all the carbonates have been decomposed.

This distinction between alkalinity due to presence of free alkali and carbonates, is one of some importance in treating of sewage effluents, as to whether they are in a fit condition to cast into a stream; and I have more particularly made reference to it here, because I have seen it stated that the effluent from the Buxton sewage was "distinctly alkaline," and commented upon as though this alkalinity was due to the presence of lime.

The amount of lime salts in solution, both in the sewage and effluent, may be inferred from the results of the examinations for hardness. As a rule, the sewage varied within the following limits:—

Temporary hardness	8—12
Permanent hardness	4— 6
Total	12—18

The effluent similarly gives—

Temporary hardness	6— 0
Permanent hardness	10—20
Total	16—20

In treating sewage with lime alone, if sufficient be added to effect anything like clarification, the effluent is almost inva-

riably and truly alkaline, but in the scheme under consideration, a considerable portion of the lime is used up in decomposing the salts of iron, aluminum, &c., in the chalybeate water, a little doubtless is carried down with the precipitate thus produced, without entering into any combination or effecting any decomposition, the remainder attacks the soaps, fatty acids, and free carbonic acid in the sewage, throwing out of solution also an equivalent of calcium carbonate by the withdrawal of the acid gas which had held it in solution. Thus, while the permanent hardness is increased, the temporary hardness is reduced, the total hardness being but slightly affected.

Returning to the organic matter in the sewage and effluent, as measured by the free and organic ammonia, in order to ascertain what improvement is due merely to dilution, and what to the action of precipitating agents, we have only to know the relative proportion of iron-water used and make a simple calculation. During the week for which the results of the daily examination of the sewage have been recorded, as near as I can ascertain, the volume of iron-water would be about one-third that of the sewage. Correcting for this dilution, we find the organic ammonia reduced from .29 to .07, and the free ammonia from .94 to .37 grains per gallon, *i.e.*, the organic ammonia is reduced to less than one-fourth, and the free ammonia to one-third, results which, considering the character of the sewage, must be regarded as satisfactory. With a sewage exceptionally strong (for Buxton), the decrease is very much more marked.

Inevitably from the relative volumes of the sewage and water, the condition of the stream into which the precipitated sewage flows must be largely influenced by that of the effluent. The improvement in the river became apparent almost the moment the works were put into operation. Immediately before the sewage was turned into the tanks, a sample (No. 1) of the river water was taken about 200 paces below, and afterwards a sample (No. 2) when the sewage was running into the tanks and the effluent into the river. Upon analysis these gave the following results:

Grains per Gallon.

	Free ammonia.	Organic ammonia.
No. 1	.29	.15
No. 2	.09	.02

Showing that the improvement was most marked.

Compared with the action of solutions made to imitate in composition this natural water, it is invariably found that the latter, under the same conditions as to agitation and quantity of lime added, give a more flocculent precipitate, subsiding with greater rapidity, and yielding a brighter effluent than is the case with the artificial water. As previously stated, the best results are obtained by adding the lime to the chalybeate water, and immediately pouring the mixture into the sewage, with due agitation. Frequently the sewage can be quickly cleared in this way, when it absolutely refuses to clear if the lime be added to the iron-water and sewage previously mixed. The probable explanation of this somewhat singular fact appears to be, that the action which takes place during purification is not only chemical but mechanical. Upon adding the lime to the undiluted iron-water, a copious flocculent precipitate at once forms, and when poured into the sewage, these hydrates (and carbonates?) combine with certain of the organic constituents of the sewage, aggregate into large flocculæ, taking up at the same time the matters which are suspended, and carrying the whole to the bottom. When the lime is added to the iron-water, diluted with 2—4 volumes of sewage, the precipitate which forms is only slightly flocculent, and rarely aggregates by agitation; hence it does not free sewage of the matters suspended in it.

Apparently, also, by adding the lime to the iron-water first, a smaller quantity of the alkali is required to affect the purification. This is probably due to much of the lime entering at once into combination with the free carbonic acid of the sewage, and being thus rendered incapable of acting upon such a dilute solution of iron, aluminum, and magnesium salts.

5. *The Sludge.*—Notwithstanding that not a particle of insoluble matter is used in this process, the amount of sludge produced is somewhat considerable, and unfortunately, as yet, I have been unable to persuade the local authorities to take

any steps to make it more disposable. At the present time it is simply pumped from the well (by the chain-pump worked by a water wheel), after draining from the tanks as much water as possible, and carted away to the board's farm. The members of the board are sanguine that its effect as a manure will be such, that the local farmers will be glad to fetch it away when the tanks are being cleansed. If such is the case, and they do not care to try and make it into a salable commodity of any kind, then, so far as they are concerned, the sewage problem is solved. It is suggested that the "destructor" they are about to erect to burn the town's refuse should be so adapted as to dry, and, if necessary, burn the sludge, as the ash, which consists of lime and ferric oxide chiefly, can probably be utilized. As the sludge is apparently not nearly so slimy as that produced by lime alone, it would not be so difficult to convert it into a salable manure.

As nearly as can be ascertained, the perfectly dry residue from 1,000,000 gallons of sewage and iron-water will be 25 cwts., but as the sludge, as lifted by the pump, contains at least 75 per cent. of moisture, five tons of such sludge would be deposited. Taking the amount of liquid flowing through the tanks to average 600,000 gallons per day, 21 tons of sludge would be formed weekly. This, by drying, would be reduced to six tons, and by incineration to about three tons.

The sludge varies somewhat in composition, but when sufficiently dry to be pulverulent, its average composition is as under:

Moisture	15.
Organic matter	38.
Oxide of iron and alumina	14.
Calcium and magnesium carbonates	24.
Phosphoric acid	1.
Other mineral matter	8.
	100.

The nitrogen present corresponds to 1.5 per cent. of ammonia.

6. *Cost of Construction and Maintenance of Works.*—The original estimate for the construction of the tanks, erection of three workmen's cottages, conveying the iron-water, laying of tramway, and for machinery, was £3,000. This, however, is likely to be slightly exceeded, but if the cost of the cottages is deducted, £3,000 will more than cover the remainder.

Very little labor is required to efficiently carry on the works; one man being able to attend to the lime, machinery, and the emptying of the cages in which the larger solids are collected. When the sludge is being removed from the tanks, a little additional help is required. The total cost per annum for labor may be put down at £75. The only additional expense is the lime, the cost of which will be about £80 yearly. The total expense, therefore, including interest on capital, is £275 per annum, made up as follows:

Interest on £3,000, at 4 per cent.	£120
Labor	75
Lime	80
	£275

Taking the population of the town at 7,000, this involves an annual charge per head of 9½d., or as it is put in the official statement by the Board, "the cost of the system, including the erection and maintenance of the works, will be covered by a rate of 1¾d. in the pound." Put in other words, each million gallons of sewage treated cost the ratepayers £1 5s.

Having given all the actual details of the process as now being worked, I would again, in conclusion, draw some attention to the fact that the action of this chalybeate water differs from that of a mere solution of sulphate of iron and alumina of corresponding strength in the important particulars, that, when mixed with lime (in due proportion) and added to sewage, it precipitates the suspended and dissolved organic matter (1) more rapidly and (2) more completely, removing the whole of the former and considerable portions of the latter. Moreover, in using alumina and iron salts for sewage purification, the best results are always said to be obtained if the sewage is dosed with lime before these salts are added; whereas with the natural water such is not the case, the maximum efficiency being apparently secured by adding the lime to the iron-water, and then mixing with the sewage. If after adding the lime the mixture be allowed to stand a short time before pouring into the sewage, its efficacy is impaired, or even altogether destroyed.

In the natural water, it must be remembered, we have not only ferrous sulphate, but also ferrous carbonate, and there is

considerable probability that the compound plays a more important part in the purification of water by iron and its salts than has been hitherto suspected. It is well known that the mere immersion of metallic iron in an organically polluted stream tends to purify it, and possibly this may be explained by the action of carbonic acid on the metal, and of the resulting compound on the organic matter.

A series of experiments bearing on this and other points connected with the subject are now being conducted, the results of which cannot be without interest, and may probably be of considerable importance.

DISCUSSION.

Mr. Baldwin Latham said there was nothing particularly new in this process, but he thought it was a mistake to mix the lime-water with the iron-water before the mixture went into the sewage. At the town of Horsham the water supply contained a large quantity of iron, and he found that the whole of that iron could be precipitated by the addition of lime. It appeared, therefore, to him that mixing iron with the lime before passing it into the sewage would produce an inert compound; and all recent experience showed that wherever lime was used, it should be added before other chemical compounds, especially where such compounds were of an acid description. Very great improvements had taken place in many sewage works by simply putting in the lime first before the sulphate of alumina, much less chemical being required, and more satisfactory results being obtained. Although the quantity of chemicals used in this Buxton experiment was large, the expense would be considerable if the same process were applied to a large town, and they had to purchase chemicals. One volume of this material was added to two volumes of sewage. This dilute water, containing no sewage, and from which all the salts were precipitated, would give about twenty-eight grains in the effluent without any chemical, as against twenty-six produced by the process; so that the degree of purification due to absolute admixture was very great by using this large volume of water from which the material had been precipitated under the action of lime. It was a well-known fact that if you could always command sufficient water to mix with sewage, then no chemical process was required, because one volume of sewage to four of pure water would give an effluent purer on the average than the best results shown by irrigation, which was acknowledged to be the best system. The tank space employed was very large for the volume of sewage stated, and he could not understand how this sewage was so strong as it appeared to be, unless there was some large amount of solid matter in the water supply. If both these tanks were in use, they would contain the sewage for twenty-for hours, which gave a long time for sedimentation to take place. Iron in various forms had been used repeatedly, and from investigations made some years ago for the Commission on Metropolitan Sewage, it was shown by Dr. Frankland that of all the salts of iron perchloride was the best, but that utterly failed when it came to be tried over a number of years. At Northampton that process had been adopted in conjunction with lime, but the river became so foul that it had to be abandoned, and the sewage applied to land. In fact, the iron had been almost universally abandoned, one reason being that sludge containing a large amount of iron was supposed not to be so valuable for agricultural purposes. The paper was interesting as recording an experiment; but he feared when the parties in this particular neighborhood wanted an effluent of a high standard of purity, they would find some other method would have to be adopted. He should like to know what means were taken for regulating the chemicals which passed into these tanks, as he understood they were left to work by themselves at night, when probably the sewage would be less in quantity and more diluted. But as the strength of sewage varied, so should the amount of chemicals vary, and in all the works with which he was connected where chemicals were used there was an automatic arrangement worked by the sewage itself, so that the right volume should be added. Under ordinary circumstances, the flow of sewage at one period of the day would be three times as much as the average flow, so that the adjustment of the amount of chemicals was very important. He thought the large number of divisions in the tank was probably a disadvantage, as it would be impossible to rectify a mistake. If too

little chemicals went into one compartment it might be nearly filled with raw sewage, which would ultimately pass out in the same state. But if there were no impediments in the tank, it would be possible by large admixture to greatly modify the ill-effects due to bad judgment.

Prof. Bischof remarked that when Dr. Frankland made his experiments, and found that perchloride of iron was the most efficacious in purifying impure water, he did not extend his experiments to the ferrous carbonate and hydrate, and he believed that these salts were the most efficient in removing organic matter from water. If, however, these were used, he quite agreed with Mr. Latham that you should not attempt to neutralize the action of the iron by first adding lime. He had tried this by adding soda, and always found the purifying action was considerably decreased. He had had a rather extensive experience in connection with purifying water with iron. Some time ago he noticed at the Antwerp waterworks that after the water had passed through the spongy iron filter, when it entered the second sand filter—because with potable water you must have a second filtration—peculiar flakes appeared in it, some of which he was able to collect for analysis; they were about the size of half-a-crown, and very much resembled dried leaves. After drying them at a temperature of 120° Centigrade, he found that the dried residue contained 49 per cent. of peroxide of iron, and after that was dissolved with dilute hydrochloric acid, what remained was incinerated, and scarcely left any residue whatever. It appeared to be something of a silicious nature, but it was impossible to test it qualitatively. This was an instance in which an insoluble compound, formed by certain organic acids, such as uric acid and hippuric acid, which all formed insoluble compounds, with ferrous or ferric salts was separated almost with the same definiteness with which you could separate a crystal. Returning to the paper, it seemed to him there was an over-abundance of iron present, because in very impure water he had found two milligrammes per liter of ferrous hydrate, or carbonate, calculated as ferric peroxide, were sufficient to effect purification. He should like to have seen a determination of the ammonia in the ferruginous water, which was very essential, to show the purification effected by the iron and lime. He would also ask whether it had been found that the sewage, after having been thus treated, could be kept for a length of time without undergoing decomposition. This was a most important question, the destructive action of iron on microphytes. That this action took place was now beyond doubt. This had been shown by him in various papers, and the late Dr. Voelcker stated in one of his papers that the presence of a proto-salt of iron in the soil was a sure sign of barrenness. These organisms in sewage were mostly, at any rate, plants, and probably were acted upon in the same way as the higher plants. This view had been confirmed by Dr. Griffiths, who found that by manuring wheat with sulphate of iron it was freed from mildew, but he also found that when he added too much sulphate of iron he destroyed the wheat itself.

Dr. Percy Frankland wished to remark, in reference to what had been said by the last speaker, that the perchloride of iron, used by his father in the experiments made for the Rivers Pollution Commission, was only nominally perchloride, and that on analysis it was found to consist principally of proto-chloride. Therefore, on the addition of lime, there would necessarily be principally ferrous hydrate present, and he presumed purification would take place by means of that, and only to a less extent by the ferric hydrate. With regard to this water at Buxton, it was obvious, from the interesting paper which they had heard, that unless there were a very large excess of this purifying material present, it would be exceedingly unsuited for the purification of sewage, because it appeared that not only was the volume very variable, but the compositione qually so, so that by no automatic device could it be arranged that the reagent should be duly proportioned to the sewage. This mode of purification by means of iron was a very old one. In fact, the composition of this chalybeate water appeared to be the inverse of that of the reagents which were used in some other sewage works, for instance, at Leyton, where the alumina added corresponded to the quantity of iron present in this water, and the quantity of alumina here corresponded to that of iron there. The proportions used at

Leyton were about 12 grains of lime, 2½ proto-sulphate of iron, and 10 grains alum per gallon of sewage. Contrary to the practice at Buxton, it was usual there to add the lime in the first instance, and then the other re-agents. It was exceedingly remarkable that a better result was not obtained when the mixture between the iron and the lime took place after admixture with the sewage. It was obvious that this Buxton sewage, however, was about one-tenth the ordinary strength. With regard to the analysis of the sewage before and after purification, he should like to ask Dr. Thresh whether he had taken care that the samples of the effluent should correspond with the samples of of raw sewage examined, because the albumenoid ammonia given in the effluent appeared to be very constant, and not to vary with the time of collection; that collected in the afternoon containing the same quantity as that collected in the morning. It was a pity the analyses were not more complete. There were no determinations of organic carbon and nitrogen, which would have rendered the testing of the process far more conclusive. Again, it would have been desirable that the amount of chlorine in the raw sewage and the effluent should be determined, because it was only by taking into account some mineral ingredient, which could not be removed by any reagent, that one could be at all sure of the relative dilution of the sewage before and after treatment. Dr. Thresh appeared to lay a great deal of stress on the disappearance of ammonia in this process, but he should like to ask him what, in his opinion, became of it. He did not see how it could be precipitated, and if not, it was not of much consequence what became of it. Probably it passed away into the air; in fact, in nearly all these precipitation methods, one found a considerable reduction in the amount of free ammonia; whilst if the process was tested in the laboratory, where the mixture was made in a stoppered bottle, the ammonia remained practically constant. For instance, in one case he himself had found sewage that contained 5.5 grains of free ammonia per 100,000 parts, when the actual effluent contained only 1.75. But when the same process was tried on a small scale, the ammonia was only reduced to 4.75, or scarcely at all. At any rate it did not appear to pass into the sludge, as the amount of nitrogen found there was much less than the ammonia which had disappeared would yield. It was very desirable that this sludge should be filter-pressed. The great objection to all these precipitation works was the accumulation of sludge, and the nuisance occasioned when it was air-dried, but this was almost entirely abolished if it were filter-pressed. He noticed that the local board expected to obtain a sale for the ashes of this sludge, which consisted principally of oxide of iron and lime, but he did not think these materials would find much sale near Buxton, which was already so abundantly supplied with them.

Mr. Ekin said Dr. Thresh had probably two main objects in view in this paper; first, to point out to local boards who were fortunate enough to have a chalybeate spring in their neighborhood what could be done with it; and, secondly, to show that there were indications here of new methods of water purification which were very promising. With regard to the use of chalybeate springs from mines for mixing with sewage, he had experience some years at Worseley, in Staffordshire, where an action was brought by the riparian owners against the Local Board on account of an enormous amount of sewage being poured into a sluggish stream; but there was no difficulty in proving that a small brook, which was fed from a mine near, and was very rich in iron, completely destroyed the sewage in the course of a very short run. He had followed Dr. Thresh's experiments, and really thought there was some hopeful indication in this particular mixture of a solution of the sewage problem. Certainly, as far as they knew, nothing had ever given such an absolutely clear effluent. The effluent at Buxton was exactly like sparkling spring water, a result which he had not seen anywhere else. Mr. Baldwin Latham had rather objected to this as simply an iron process, and he was so well read in all matters relating to sewage that he (Mr. Ekin) was rather surprised to find that he was not quite up to date in objecting to iron for making the sludge useless; for some remarkable results had been lately given before the Chemical Society, which showed that in certain proportions iron was exceedingly valuable as manure. But

this was not an iron process alone; there were aluminum, magnesium, calcium, &c., and some experiments of his own, on a small scale, with a similar mixture of salts, seemed promising. Iron salts alone certainly would not give anything like the same results. Dr. Thresh had been very candid about the whole matter, having given in other places ample details of his experiments. He had worked very industriously at the composition of this spring, and had not been able to produce by any mixture of salts what this chalybeate water would undoubtedly do, but he hoped he would still persevere until he found out the secret of this particular combination, whatever it might be. With regard to the cost of the ingredients, he did not know that they need be at all prohibitive. If the Metropolitan Board of Works could afford to deodorize London sewage with potassium permanganate, he thought he could promise that a mixture of this kind would be considerably less expensive. Of course the old difficulty of the sludge cropped up, and he thought twenty-one tons per week for such a small town was rather large. Filter-pressing might be very useful, but that certainly seemed the difficulty with all sewage operations.

Mr. Baldwin Latham said that at Leyton the iron and aluminum process had been abandoned, and lime and black-ash waste was now used, which had certainly the remarkable property of preventing the sewage effluent undergoing a change.

Mr. Maxwell Lyte said, as regards the cost of the ingredients, he understood that alumina was one of the ingredients in these chalybeate springs. Hitherto, in the A B C process and others, alumina had been the chief precipitating agent, but his father had taken out a patent for the use of aluminate of soda in conjunction with sulphate of alumina, and by that means was able to effect a saving in the precipitation of alumina, or hydroxide of alumina, of about 33 to 50 per cent.

The chairman said anyone who had had much to do with the treatment of sewage would know that there were a great many points on which a very substantial agreement existed, whilst there were other points on which opinions were widely opposed. There could be no doubt of the accuracy of the statement made by Mr. Latham, that sewage irrigation when satisfactorily carried out, did produce a purer effluent than the process of precipitation, also supposed to be well carried out. The only question was, whether the process of precipitation, in certain cases, effected a sufficient amount of purification for the purpose; and here, again, although there were diverse opinions, the preponderence of opinion was that the process did, in many cases, effect sufficient purification. With regard to the particular precipitant to be employed, he did not gather that Dr. Thresh recommended that every town should carry out expensive engineering works to bring chalybeate water from a distance, when they could get their chemicals in a much more concentrated form, but he did think that great praise was due to Dr. Thresh for having taken advantage of the special opportunity afforded him by the proximity of this natural water. Of course there were advantages and disadvantages attending the use of a chalybeate spring. One advantage was that it could be obtained practically for nothing; but on the other side was the disadvantage that you had to deal with a very bulky material, necessitating increased size of tanks, and you were also dealing with a material somewhat uncertain in quantity and composition. On which side the ultimate advantage lay, could only be decided by experience. It would seem, on the face of it, that this water containing iron in the two forms of sulphate and carbonate, also appreciable quantities of alumina, and large quantities of magnesia, was pointedly fitted by nature for this particular purpose. As regards the order in which the chemicals were mixed, and the observation of Dr. Thresh with regard to the effect of an insufficient or extreme amount of agitation, those were entirely practical questions. So far as his experience had gone, it accorded with Mr. Latham's, that the best results were obtained by the addition of lime first, and the subsequent addition of alumina, or alumina mixed with iron; but how far it would necessarily follow that a similar course would be best where the iron was already dissolved in such a large excess of water, was an altogether different matter. He should have preferred a little more information on one or two points with regard to the nature of the sewage, and the chemical character of the ef-

fluent; and he would refer more particularly to the removal of insoluble organic matter by the process. Substantially the whole of the insoluble matter in sewage was capable of being removed by precipitation, together with a certain proportion of the soluble. Authorities differed whether the removal of the soluble or insoluble matter was most important. For himself, he entertained the opinion, which was rather opposed to that of Dr. Frankland, and the Rivers Commission, that the removal of the insoluble matter was the most important of the two, but this was an open question. Still, in dealing with precipitation schemes, it was to be borne in mind that you could certainly effect the removal of substantially all the insoluble organic matter, together with a certain variable proportion of the soluble. There was a great deal yet to be learnt with regard to the best mode of dealing with sewage; and they would all recognize that Dr. Thresh had made an important, ingenious, and, in this particular case, an exceedingly successful addition to their practical knowledge on the matter.

Dr. Thresh, in reply, said Mr. Latham seemed to have misunderstood the motive he had in making this communication. He claimed no originality, but the fact was, that in consequence of the opening of the sewage works being published, he had received innumerable letters asking for particulars of the process and in some cases for minute details. As it was impossible for him to reply to all these letters, some of his friends suggested that the best thing he could do was to read a paper before some society, and the secretary of this section had kindly fallen in with that idea. At Buxton, certain schemes had been tried, and having been requested to report upon them, and having condemned some of them, he was asked if he could suggest something better. It was acknowledged that this iron spring was a nuisance, as it covered the bed of the river with ochre, and rendered it unsightly. It then appeared to him that if they could make one nuisance remedy another, it would be a benefit to the town. Consequently, he undertook experiments, and found that by the addition of a little lime they could cause one nuisance to remove the other. Irrigation, of course, was out of the question at Buxton, as there was no land there with a depth of soil exceeding six inches. It was suggested that they should pump the sewage a little distance out of the town, and let it down a water swallow; but no one could say that would be a satisfactory way of getting rid of it. With regard to the effluent keeping without decomposition, some small fish had been kept in a tank, in a mixture of effluent with two volumes of river water, and after lapse of some weeks, they appeared quite healthy, and there was no perceptible smell from the water. It might be a mistake to mix iron-water with the lime first in the case of artificial mixtures of iron and alumina; but in this case, there could be no doubt of the result of his experiments. He did not know that he had hit on the right reason for it. but there was no question that when the lime was added first it did precipitate more rapidly. If you let mixtures prepared in both these ways stand for a few hours, and then examine the effluents, there is no difference with regard to purity, but when the lime was added first, the precipitation took place more rapidly. If you made a solution of sulphate of iron, and made the same experiments, you did not get the same result. With regard to chlorine, he had made a number of determinations, but did not think them sufficiently interesting to record. There was here a natural water which acted in some way which could not be quite explained, but that did not alter the fact that it did so act, and he thought it quite possible that the mode of mixing he suggested was really better than adding lime first to the sewage.

Dr. Bischof asked if the effluent was always free from iron.

Dr. Thresh said not invariably. The process was allowed to go on day and night, and sometimes the lime did not drop through the hopper, and then next day you might find a little iron in the effluent, but it did not exceed a very minute trace, and no notice was taken of it. A small trace like that would not materially affect the purity of the river.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers—Annual Convention of 1885.—The annual convention of this Society for the year 1885 will be held at Deer Park, Md., on the

line of the Baltimore & Ohio Railroad, June 24th, 25th, 26th and 27th, 1885. Sessions for professional discussion, and one for the transaction of business, will be held.

All members who can do so are invited to arrive at Baltimore on Monday, June 22d, and take part in the excursion named below.

The sessions will be held during Wednesday, Thursday, Friday and Saturday, except that on one of those days, to be specially announced hereafter, an excursion will be made by invitation of the B. & O. R. R. to the Cheat River Grade, Kingwood Tunnel, Tray Run Viaduct, and other interesting points on the Mountain Division of the Road.

ENGINEERS' CLUB OF PHILADELPHIA—REGULAR MEETING, May 16th.—Mr. Kenneth Allan exhibited and described an improved protractor, designed by Mr. John R. Freeman, Member A. S. C. E., Lawrence, Mass., and made by Messrs. Darling, Brown & Sharpe, Providence, R. I., with special regard to accuracy and finish. It consisted of a plate 6″×12″, bearing an 11″ semicircle graduated to 20 min. The vernier, reading to minutes, is carried on an arm, through the center, to which may be accurately attached different arms, 20″ long, graduated to mm., $\frac{1}{1000}$ ft., $\frac{1}{30}$ in., $\frac{1}{40}$ in., $\frac{1}{10\times5}$ in., and $\frac{1}{5\times10}$ in.

The error from eccentricity of center is determined within $\frac{1}{4}$ min. The total error at any part of arc is believed to be less than $\frac{1}{2}$ min. In use, 100 angles are easily plotted in an hour, and in a year's work its price—$130—is estimated to have been saved. The whole is of nickel-plated steel.

Mr. Dana C. Barber, Assistant Engineer of the Philadelphia Water Department, in charge of Sanitary Surveys and Investigation of River Pollution, presented an account of the pollution of the Upper Schuylkill, being a brief *resume* of some of the more notable and peculiar features, viz., the acid pollution from the coal mines, the cesspool drainage of Reading, the Pottstown water supply, the winter disturbances in the quality at Phœnixville, the drainage of State Insane Asylum at Norristown, etc.

The sulphuric acid from the mines about Pottsville has been decreasing since 1868, on account of the transfer of mining operations to the other side of the mountain, draining into the Susquehanna, and now amounts to but one-third of its former quantity. The most now comes from the region about Tamaqua. But few fish live in the river above Reading, and the water is unfit for use in boilers. At Reading the acid is neutralized by lime-water from Tulpohocken Creek, sulphate of lime being formed and deposited. The water just below Reading would be very pure but for the foul drainage from that city—the greatest source of pollution in the valley. The most peculiar feature there was the underground strata of cavernous limestone, into which nearly half the cesspools and privy wells are dug, and thus drained of liquid waste by natural subterranean channels to the river.

At Pottstown, eighteen miles below the enormous pollution from Reading, the water supply was found to be drawn from the river, and that, too, at the lower end of the town, below the discharge of much domestic sewage from the town itself. The local physicians have protested against this practice in vain.

The water supply of Phœnixville, drawn from the Schuylkill above the town, was ordinarily very good, but at times in winter, when the river was frozen over, became very bad. The author had discovered the cause of this to be due to the peculiar course of the channel and formation of the river bed just above the pumping station, which caused a deep ice jam that arrested the coarser organic impurities and sent them into the reservoir.

The pollution from Norristown was very serious, especially that from the State Insane Asylum near the town. No attempt to purify any kind of liquid waste before discharging it into Stony Creek had been made, except the water-closet sewage (60,000 gallons per day, from 1,200 people), which had, for two years, been treated as follows: The sewage was first settled in receiving basins, and afterwards filtered—first through blocks made of copperas, plaster of Paris and purifying lime of gas works, and then through finely-pulverized blast furnace slag. By an ingenious arrangement the filters were cleaned by perforated water pipes passing through the bottom, flushing the accumulations back into the settling tanks, from which the sludge was pumped by a steam ejector, and run by gravity to a distant part of the farm. This arrangement worked well, but on account of the nuisance from the unpurified sewage, it had been decided to dispose of the whole by sub-surface irrigation.

President de Kinder read, from a clipping of the *Public Ledger*, of recent date, the following expression used at a meeting lately held by the Homœopathic Society of this city: "In quality, we have a fluid from which a dumb brute may well turn in disgust." Mr. de Kinder said that he was bitterly opposed to the pollution of our drinking water, that he was against the admission of sewage and other deleterious matter into the river,; that the intercepting sewer should be finished without delay, and that the great need for improvement of the distribution system and of large storage basins should be attended to at any cost. He also said that the idea of any sewage matter being allowed to mix with our water supply was very disagreeable, and he would be opposed to it from a point of cleanliness, even if it had no effect upon the wholesomeness of the water.

But, while not wishing to defend, in any sense, either the pollution of the river or any delay in making the needed improvements, he must protest against such extravagant language as had been used by this Medical Society. There was abundant proof to show that impure water was not, of necessity, unwholesome, and eminent authorities were quoted to prove this.

He further stated that chemical analysis of water was not infallible, and that, at any rate, the chemist could only state the results of his analysis. The Sanitary Engineer was the proper authority to draw the conclusions and to sit in judgment upon the question of the

wholesomeness or unwholesomeness of the water supply, for he could judge as to what the effects were in other places where water of a similar quality was in use.

Although heartily in favor of an improved supply, he must yet contend that the death rate of our own city showed that things were not so fearfully black as these gentlemen of the Homœopathic Society would have it. They had a perfect right to think as they pleased, but they should exercise caution before unnecessarily alarming the community. If the water were as vile as they would have it, why had he or his family never been warned by any medical authority against the use of it, notwithstanding several members thereof had been under medical treatment almost continuously within the past three or four years?

Few doubted that running water would purify itself. Exactly what effect the run between Manayuck or the Falls and his own residence had upon the quality of the water, he could not say, but, at any rate, he himself and thousands of others drank it, if not with an absolute relish, at least without feeling any evil effects therefrom.

Proceedings of the Engineers' Club of St. Louis—May 13.—The committee to consider the status of Civil engineers in the employ of the United States, made the following report:

Engineers' Club of St. Louis:

Gentlemen—Your committee to consider the status of Civil Engineers in the employ of the United States respectfully reports:

That we find the question of the relation of Civil Engineers to the public works undertaken by the general Government was one of considerable prominence in the debates in Congress, during the late session of that body, and is now in the public press. Also, that there is a general impression that the time has come for an increase in the numbers of the corps of engineers sufficient to enable them to conduct the works under the present organization without employing civilians, or for a reorganization of that branch of the public service, by which it shall be made a distinctively civil service.

Your committee is decidedly of the opinion that the best interests of the military service, as well as those of the civil engineering profession and of the country as interested in both these, demand that the latter alternative be the one pursued.

Your committee regrets to see the discussion of the subject in the public press turning aside from the broad question of creating an organization for the conduct of public works, which shall be equitable and just in distribution of rewards for merit, in promotion to higher rank and pay; which shall recognize the change in the condition of the engineering profession, both military and civil, since the days when both had to be imported from Europe, and which shall allow its engineers to come by judicious selection the same as judges are selected, from the ranks of a profession, and not from the roll of a single school.

To this question, personal matters, past present or future, the value of different schools and mode of training, or the honesty and truthfulness inculcated through certain associations, are alike foreign.

There seems, therefore to be need for a conservative influence, lest the utterances of individuals be taken as expressing the views and wishes of the engineering profession, and lest a discussion of a pure question of public policy degenerate into a controversy about matters of no consequence.

Other clubs than our own have taken up the consideration of the matter, and have appointed committees. It has been suggested that these committees act together, if possible, through correspondence and conference by representations, if practicable, with a view to a joint memorial to express to Congress our view of the matter, and a draft of the legislation required to carry that view into practical effect.

Your committee, therefore, asks to be continued, and for authority to confer with the representatives of other engineer societies, clubs or associations, with a view to concert of action, but without power to pledge this club to anything.—Respectfully,

Robert E. McMath,
J. B. Johnson,
H. S. Pritchett,
J. A. Ockerson,
Committee.

By vote of the club the committee was continued, and the authority asked was granted.

Mr. Frank H. Pond read a paper on "Pumping Machinery and Waterworks." He reviewed the methods of procuring water, from the days of running streams and wells, which developed into the grand aqueducts of Rome to the use of water-pipes for introducing water into dwellings, first employed in London in 1582. Cast iron came into use for making water-pipes in 1810. The oldest waterworks in the country are supposed to be those built at Bethlehem. Pa., in 1754. Now, in 1885, there are 1,040 water works in the country, of which 355 are supplied by gravity, 649 by pumping, and 36 by unknown methods. Then followed an extensive treatise, illustrated by tables, in which the conditions, works and requirements of the modern pump were thoroughly explained. The main points treated of were the pump employing crank motion and heavy fly-wheel, and the duplex pumping engine.

The paper was discussed by Messrs. Moore, Johnson, McMath and Seddon.

ENGINEERING NOTES.

Honingmann's Fireless Locomotive.—We have already published several notices of Mr. Honigmann's invention, and we may therefore assume that our readers are acquainted with the chief features of the "soda engine." It is generally supposed that Honigmann was the first to make practical use of the "regenerative" properties of a solution of caustic soda, potash, or common salt. This, however, is erroneous. Similar trials were made many years ago in England by Mr. Loftus Perkins, while the essential features of Honigmann's engine were laid before the public by Mr. Spence in 1874. If our readers will refer to Vol. xvii. of

Engineering, they will find on page 124 a paragraph entitled "The Utilization of Waste Steam," where the essential features of Honigmann's engine are described. At the time referred to (January, 1874), Mr. Spence, Jun., proposed to pass the exhaust steam of a high-pressure engine into a solution of caustic soda, which, according to his calculations, would thus be heated to 375 deg. This heated solution was then to be circulated through pipes in an ordinary boiler, where it would serve to raise fresh steam without the use of fuel. As the solution of soda became diluted by the steam condensed in it, it would lose its heat-restoring capacity, and must be regenerated by concentrating it in another boiler of ordinary construction. This paragraph elicited a letter from Mr. Loftus Perkins which we published on page 141 of the same volume (xvii.). Mr. Perkins says there: "I then (in 1864 or 1865) constructed a four horse-power engine, worked by a boiler immersed in a bath of chloride of calcium, the exhaust steam being condensed in the salt and all the waste heat from the engine being entirely absorbed, thus making a perfect heat engine. Succeeding so far the difficulty arose how to get rid of the water absorbed by the salt; this could only be done by distillation, and even if quintuple distillation is used, the coal consumed could not be less than 1¼ lb. per horse-power per hour, whilst a well-constructed, high-pressure, compound condensing engine used less than 1 lb." It is therefore evident, from what has been said, that Mr. Spence, eleven years ago, proposed an engine substantially the same as Honigmann's, and that even prior to that, viz., twenty years ago, Mr. Perkins had actually constructed a similar engine. These engines were, however, supposed to be uneconomical, and thus the matter was dropped, to be taken up again by Mr. Honigmann, who, it is true, has not succeeded yet in using less fuel with his fireless locomotive than with an ordinary engine, but who has shown that the advantages derived from the total absence of smoke and exhaust steam need not be accompanied by any considerable sacrifice in economy of fuel. Thus, though Mr. Honigmann is by no means the first inventor of the soda engine, he is undoubtedly the first who, by his perseverance, has succeeded in introducing it into actual practice.

Aeronautical Exhibition at the Alexandra Palace.—The executive of the International Exhibition, which was opened on March 31 at the Alexandra Palace, and which, we trust, many of our readers have made a point of visiting, have been able to secure the support of the Aeronautical Society of Great Britain in holding, in connection with the International Exhibition, an Aeronautical Exhibition. The organization of this exhibition has been entrusted to Mr. Fred. W Breary, the honorary secretary of the society for nearly twenty years, and who has been appointed commissioner for that department. It is to be opened on June 1, and the exhibits that are to be admitted will consist of the following objects: (1) Models of designs for the accomplishment of aerial navigation by mechanical means only. (2) Models of designs for the accomplishment of aerial navigation partly by buoyancy and partly by mechanical means. (3) Models constructed to elucidate either of the two last objects which are capable of flight and carrying their own motive power. (4) Machines constructed upon a scale calculated to carry a weight equal to that of a man, upon the principles advocated by the inventors. (The practicability may be demonstrated by the flight of a model of similar character, and of weight-carrying capacity sufficient to enable a judgment to be formed as to the probable efficiency of the large machine when actuated by the power necessary for its support and propulsion, whether by manual or mechanical methods.) (5) Light motors. (It may be observed that light motors are in request for other purposes than aerial navigation. But for the latter object it is essential that extreme lightness shall be a condition. Therefore, only a motor possessing that qualification in proportion to its power with the smallest consumption of fuel in the case of steam or other adjuncts, and capable of working up to one horse-power, at the least, for twenty minutes, will be deemed deserving of the prize.) (6) Balloons, navigable or otherwise. (7) Balloon material and appliances for propulson or otherwise. (8) Kites or other aerial appliances of that character, for saving life at sea, for traction or otherwise. (9) Objects of interest connected with aeronautics.

The amount of the prizes to be given to successful exhibitors is to depend upon the success which attends the appeal to the members and friends of the Aeronautical Society. Further particulars will be furnished to intending exhibitors (who shall have applied for space) at a later date. An interesting feature of the exhibition will be, that advantage is to be taken of the large outdoor space available to invite the competition of amateur and professional aeronauts, so that the disputed question of aerial locomotion by the aid of buoyancy may be conclusively tested. Any form and any appliance is admissible. It is hoped that a contest of balloons for the nearest approach to a given locality may be arranged upon the day or days appointed, to start from the Alexandra grounds. The goal is to be determined shortly before the time of ascent. It is believed that upon such an occasion there would be unusual opportunities for the conveyance of paying passengers. As it is scarcely expected that an aeronaut will claim to drive his balloon against a wind of greater velocity than that by which his balloon can be propelled, but may be able to deflect it considerably from the wind's course, the place of ultimate destination will be determined by the set of the wind at the hour appointed. But if the more ambitious attempt should be desired, then accordingly the goal will be arranged. By preference, a cathedral town will be named. In order to induce aeronauts of any nationality to prepare for these contests, and, as showing that Great Britain is as alive to the advantages of a useful system of aerial locomotion as any other country, it is desired to give such prizes in money as will command competition. In order to make the exhibition a suc-

cess, the Aeronautical Society appeal for assistance in furtherance of a prize fund. Contributions may be forwarded to Mr. Breary, who will act as honorary treasurer, and checks should be crossed "London and County Bank." On this occasion it should be mentioned that the honorary secretary of the society has issued a short pamphlet, in which the relations of the Aeronautical Society with respect to aeronautics are explained.

IRON AND STEEL NOTES.

Slag Cement.—We have on various occasions referred to the ingenious and very successful processes of Mr. Frederick Ransome, for the manufacture of cement from blast furnace slag and lime. The discovery, made about twelve years since by Mr. Charles Wood, that slag run from the blast furnace in a melted condition falls into a fine granulated state, removed one of the main objections to its utilization—the great cost attending its reduction to powder by mechanical means. One of the materials composing the Ransome cement is thus obtained ready for use, and being practically a waste product, its cost is nominal, the expense attending its application being limited to handling. By his earlier method the other material employed—chalk or lime—was ground and mixed with the slag, the combination being then calcined, and again ground; from this resulted a cement possessing very high qualities both as regards quickness in setting, and strength. Very recently, however, Mr. Ransome, following the same line of investigation, has improved greatly on his former simple process, and he has found that the spent lime from gas works may be employed with results as good as those obtained with lime prepared specially for the purpose. In order, however, to get rid of the sulphur with which the lime is saturated when it leaves the gas purifier, Mr. Ransome resorts to a very simple and efficacious device. He mixes a certain proportion of powdered coal or coke with the slag and lime, and when this is exposed to the heat of the calcining furnace, the action of the coal or coke converts the sulphate into a sulphide of lime, that is subsequently entirely got rid of by the introduction of a jet of steam which drives off the whole of the sulphur impurities as sulphuretted hydrogen, leaving the lime quite pure. This, however, is only one of the recent improvements to which we have referred. A highly important modification is the use of a revolving retort for the calcination of the slag and lime. It is found that after the materials have been thoroughly burnt in this manner, they remain in the same fine state of subdivision as when they were placed in the retort, and on being discharged will pass through a sieve of 80 meshes to the inch. The costly process of grinding, which is unavoidable in the ordinary method of manufacture, is thus avoided, while the cement is said to lose none of its useful characteristics in this novel process. Of course this system is equally applicable where fresh lime is employed, instead of the waste material from gas works, only in such case it is unnecessary to add the powdered coke, or to apply the steam jet. We have referred in general terms to the strength of the cement produced by this method; the following Table of comparative tests made with samples of Portland and Ransome cement shows clearly the remarkable qualities possessed by the latter; the samples in each case were 1½ in. square, giving a sectional area of 2¼ square inches.

Age of sample.	Portland Cement Breaking Load.	Ransome Cement Breaking Load.
	lbs.	lbs.
2 days.	510	740
3 "	698	870
7 "	818	1170
12 "	—	1300
15 "	—	1330
21 "	—	1440
28 "	936	
7 years.	1327	

The foregoing figures speak for themselves, and indicate clearly that the Ransome cement possesses striking advantages over Portland, especially as it reaches a strength within a few days, which is higher than the Portland after seven years. Very important advantages are also found in the simplicity of manufacture, and the suppression of the final process of the cement manufacturer, that of grinding. The plant used is therefore simpler and involves much less expense in maintenance and labor for the production of a given quantity of cement than is required in the ordinary mode of manufacture. When in addition to this, it is remembered that waste materials are employed, it will be easily understood why the slag cement can be made for half the cost of Portland, and the commercial importance of Mr. Ransome's process will be readily appreciated.

RAILWAY NOTES.

Utilizing Old Steel Rails.—Some interesting experiments have been made recently by Mr. Robert E. Masters, Columbus, Georgia, in melting old steel rails in an ordinary cupola furnace and casting therefrom various useful articles, as brake shoes, truck wheels, &c. Mr. Masters has been experimenting with steel scrap, principally agricultural steel, for some time, having melted some 60,000 lbs. The melting requires a longer time than cast iron, but, when once started, the steel is stated to come down very hot and fluid. although it appears to cool more quickly in the ladle and gum up readily. There is ample time, however, to make large castings. The charge was 1 lb. of coke to 6¾ lbs. of steel. By itself the steel runs porous, but by adding one-sixth of cast iron, a very solid, close-grained casting is produced, which, Mr. Masters states, is stronger than malleable iron for small castings, and is superior to cast iron for many purpose. An experiment has also been made in melting steel rails at the rate of 5 lbs. of metal to 1 lb. of fuel, at a temperature of 3,000°. Some very fine castings were produced that way. Mr.

Masters appears to have undertaken a most important experiment, and one which promises to be of great value when its success is fully demonstrated, and methods have been fully worked out.

The Railway Administration at Frankfort-on-the-Main have, according to the *Journal of the Society of Arts*, recently repeated some experiments on the lighting of trains by electricity, which are said to have been attended by most satisfactory results. The experimental train was composed of a first, second and third class carriage, and a luggage van, which contained a special compartment for the dynamo and accumulators. The dynamo was of the Moehring type, and was driven from the axle of the wheels of the van, and at a velocity of 700 revolutions per minute, when the train was running at a speed of eighteen to forty-two miles an hour. When the train is running at full speed, the lamps remain in circuit whilst the accumulators are being charged, but when the speed is less than eighteen miles per hour the current is applied direct from the accumulators, a specially constructed automatic commutator regulating its intensity. During the day the lamps are thrown out of circuit, and the twenty-six accumulators are charged by the dynamo when the train is in motion. This installation weighs about 12 cwts., and costs £125. The train was lighted by twelve incandescent lamps, of which two were in the luggage van, two in the third-class carriage, four in the first, and the remaining four in the second-class carriage. The cost of fitting each carriage varies from £3 4s. to £4. These experiments are said to demonstrate the practicability of lighting trains by electricity, the light being perfectly steady during the journey, and at variable speed, and even during stoppages at stations; only at starting a slight oscillation was perceptible. As all is regulated automatically, no attendant is required, it is said, except at starting. The experiments were continued for six weeks, at the end of which time everything was found in perfect order. The cost of lighting is estimated at ten centimes per lamp per hour.

ORDNANCE AND NAVAL.

The Brennan Torpedo.—The naval authorities continue to make experiments with the new locomotive torpedo which has been introduced by Mr. Brennan, a gentleman from Australia. This favored inventor has had placed at his disposal for experimental purposes a casemate on the upper tier of Garrison Point Fort at Sheerness, and a factory has been erected outside the port with a tramway running down to the beach. With these resources at his command Mr. Brennan has so far perfected his idea that a trial of the apparatus has been made which has, it is reported, proved so convincing to the official mind that a sum of £10,000 "on account" has been paid to the fortunate antipodean; this being, it is said, but a tenth part of the reward the inventor will receive for his ingenuity. The torpedo is ejected from the fort by means of a steam engine, at a velocity estimated at 50 miles an hour. There are within the machine two coils of wire wound on spindles each connected with the shafting of a screw propeller. The ends of these wires are made fast to drums on the steam engine within the fort, and as the wires are unwound from the reels in the torpedo on to those on the engine, the screws are set revolving and the weapon propelled forward. The steering is effected by hauling harder on one side or other of the wires so as to make the respective screw revolve faster. Lights screened from the front are placed to show to those on the fort the position of the torpedo. If it be true that the authorities have been led into such surprising liberality as report credits them with, it is to be hoped they are attempting to beguile other nations equally interested in torpedoes with the story of this duplex top-spinning method of propulsion, and are concealing the real mechanism of the Brennan torpedo, the merits of which ought to be very large, if the department values them at £100,000.

The New Brazilian Armor-Clad.—The Aquidabau, which has recently been completed by Messrs. Samuda Brothers, ran a very successful series of trials off the Maplin last week. This vessel is of the same general description as the Riachuelo, full particulars of which were given in a paper read by the late Mr. Samuda before the Institution of Naval Architects last year. The new vessel is 280 ft. long and 52 ft. wide, the displacement being 5,000 tons. The mean draught on trial was 18 ft., the vessel having been designed not to draw much water as she is required for service in the South American rivers. The hull is built of Siemens steel and sheathed with wood. The ram is a solid gun-metal casting and the stern frame is of the same material. The machinery is protected by a water-line belt of steel-faced armor 11 inches maximum thickness and 7 feet wide. There is an armored deck 2 inches thick, carried fore and aft, and arranged to protect the steering gear aft and also to strengthen the ram. The armament consists of four 9-inch 20-ton, breech-loading guns, placed in turrets protected by 10-inch armor. On the upper deck there are two 5¾-inch breech-loading guns at the bow and two similar weapons at the stern. There are fifteen Nordenfelt guns and five ports for the discharge of torpedoes. The engines are by Messrs. Humphreys, Tennant & Co., and are of the three-cylinder, compound type. The Aquidabau was tried in sea-going trim recently. With natural draft the indicated horse-power was 5270 and the speed 15.257 knots. With closed stokeholes and fan draught the power was raised to 6201 indicated horse-power and the speed 15.818 knots. In the forced draught trial only six of the total number of eight boilers were used. Two runs on the mile were made with only one screw working, the speed being at the rate of 11.447 knots, 15° of helm being required to keep the ship straight. A half-circle was turned against the screw in 8¼ minutes. A six-hours' coal trial was made. The official report states that the consumption was at the rate of 45 tons a day when the ship was steaming at her contract speed of 14 knots. As the coal bunkers carry 800 tons, the Aquida-

bau could steam over seventeen days on her bunker coal and cover a distance of about 5,700 knots.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

SIXTH Annual Report of the Ohio Society of Surveyors and Civil Engineers. Pamphlet, 156 pp. Columbus, Ohio.

Circulars of Information of the Bureau of Education, No. 1, City Schools of United States. By John D. Philbrick, LL.D. Pamplet, 207 pp. Washington: Government Printing Office.

Planting Trees in School Grounds and the Celebration of Arbor Day. Pamplet, 64 pp. Washington: Government Printing Office.

Proceedings of the Institution of Civil Engineers. Address of the President, Sir Frederick Joseph Bramwell, F.R.S. Physiography by John Evans, D.C.L., LL.D. The Tekapo Bridge, by Frederick William Marshant, A.M.I.C.E. Steam Tramways, by Hon. Richard Clere Parsons, B.A. The Sydney Tramways, by Walter Shellshear, Assoc. M. Inst. C.E.

Thunder Storms of May, 1884; Signal Service Notes, No. 20, by Prof. H. A. Hazen. Pamphlet, 8 pp. and 2 charts. Washington: Signal Office.

FORESTRY IN POLAND, LITHUANIA AND THE UKRAINE. Compiled by JOHN CROUMBIE BROWN, LL.D. London: Simpkin & Marshall.

In preparing this book the writer has simply followed the plan begun some years since of collecting, arranging and publishing the best obtainable information relating to Forestry of different countries. His books, therefore, as they appear in succession relate to regions not described in the previous reports.

The subject is a broad one and comprehends not only the propagation and preservation of the forest trees, but the history of the laws governing such preservation, and also much concerning the physical geography of the region.

The present work contains much that is interesting about a section of Europe not much traveled by the tourist.

HEALTHY FOUNDATIONS FOR HOUSES. By GLENN BROWN, Architect. Science Series, No. 80. New York: D. Van Nostrand.

Even under the present conditions of widely diffused sanitary knowledge few people realize that the health of a household depends rather more upon the earth directly beneath them than upon the surface conditions surrounding them. A bad smell from a neighboring factory may be vexatious and worthy of legal restraint, but still quite harmless when compared with a damp cellar-bottom, which dispenses disease germs that offend directly none of the senses.

The work is divided into four parts: I. Natural Foundations; II. Drainage; III. Foundation Walls; IV. Miscellaneous.

The illustrations are numerous and excellent, and the instructions regarding building or draining are to an eminent degree practical. Designers or occupants of suburban residences should be familiar with the principles here inculcated.

THE FIGURE OF THE EARTH. By FRANK C. ROBERTS, C.E. Science Series, No. 79. New York: D. Van Nostrand.

This little essay, reprinted from *Van Nostrand's Engineering Magazine*, where it appeared as an original contribution, is chiefly of interest to students of physical and mathematical science.

The object of the author has been to arrange in compact form the important mathematical principles for the deduction of the Figure of the Earth upon the spheroidal hypothesis. This is an important service rendered to the student, as most of the literature upon this subject is burdened with voluminous mathematical discussions of collateral subjects.

The subject is presented in two distinct parts: I., Historical. II., The Oblate Spheroidal Hypothesis.

The first part or section may be read with profit by any student of Physical Geography. The second section supposes a moderate amount of mathematical knowledge in the reader, but still much less than is generally called for by previous writers.

A TEXT-BOOK ON THE MECHANICS OF MATERIALS, AND OF BEAMS, COLUMNS AND SHAFTS. By MANSFIELD MERRIMAN. New York: John Wiley & Sons.

This work differs from other good works of its class in presenting just what is comprehended in the above title and in omitting the tables of results, which, although valuable to the working engineer, are an encumbrance in a text-book and present a forbidding look to the learner.

This book presents the following topics in order, in chapters:

I. The Resistance and Elasticity of Material.
II. Pipes, Cylinders and Riveted Joints.
III. Cantilevers and Simple Beams.
IV. Restrained Beams and Continuous Beams.
V. The Compression of Columns.
VI. Torsion and Shafts for Transmitting Power.
VII. Combined Stresses.
VIII. Appendix and Tables.

To fully fit the treatise for instruction purposes, Prof. Merriman has added problems to each section.

The illustrations are exceedingly well designed to illustrate each separate topic.

The well-earned reputation of the author, both as writer and instructor, renders any comment on the quality of the work superfluous. The investigations presented here belong to a field of labor in which the author is entirely at home, and in which he may be regarded as an authority.

GAS ENGINES. By WILLIAM MACGREGOR. New York: D. Van Nostrand.

The use of the gas engine is rapidly extending. With the introduction of electric lighting it received a new impetus.

Wherever in our large cities a motor is required for a temporary service at more or less irregular intervals, and where convenience is valued more than economic efficiency, there the gas engine finds favor. The question regarding the principles of action of this or that popular

gas motor are much more frequently asked than answered.

The book before us is designed to answer queries that are daily asked about engines which are constantly being set up in connection with dynamic electrical circuits.

The treatise deals first with the historical phase of the subject and describes some of the forms from which it may be concluded the present forms have been directly evolved.

Part I. deals with engines working without compression, and includes descriptions of the Lenoir, Hugon, Bischopp, Andrews and Sombart engines, illustrated with an abundance of diagrams.

Part II. explains the construction and action of gas engines working with compression, and is divided into two chapters; one devoted to that class of engine in which the whole of the four periods of the cycle are performed in the motor cylinder; and the other to that class in which part of the cycle is performed in a pump or separate cylinder. The Clerk engine being the representative of the best latter class.

Part III. presents in six chapters the theories of the different kinds, and will be to the student of thermo-dynamics the most valuable portion of the book. The book is sure of an extensive demand, because it answers in so direct a manner the questions that are daily asked among mechanical engineers.

How to Drain a House: Practical Information for Householders. By Geo. E. Waring, Jr., M. Inst. C. E., Consulting Engineer for Sanitary Drainage. New York: Henry Holt & Co, $1.25.

The author, a well-known authority and writer on sanitation, deals with the best methods and appliances for the plumbing and draining of houses, and writes on this subject of ever-growing importance to the householder in his usual vigorous and elegant style. The larger part of the contents have recently appeared in articles contributed to the *Century Magazine*. To these, which had special reference to the details of pipes, traps and fixtures inside of the house, the author has added important chapters on the sewage disposal for isolated houses, particularly by the method known as "subsurface irrigation." It is to be regretted that the illustration of the book should not have been rendered in a more uniform and better style. The remarks made in Chapter XXIV., on the relations existing between owners, architects, sanitary engineers, and plumbers deserve earnest and thoughtful attention.

While the book is principally intended for householders, it contains much that is of interest and value to the rapidly increasing number of engineers, who make healthful house construction a special study.

An Elementary Treatise on Hydromechanics. With numerous examples. By E. A. Bowser, Prof. of Mathematics and Engineering, Rutgers College, New Brunswick, N. J. D. Van Nostrand, Publisher.

This work on the equilibrium and motions of fluids possesses in a marked degree the essential features of a good text-book. The definitions and axioms of the subject are clearly stated and their relative importance made apparent to the eye in the text. Theorems based upon them are demonstrated as concisely as is consistent with completeness of treatment, and the principles are immediately applied to questions of a practical kind and fully illustrated by numerous examples. The elements of this science are presented in a way to encourage habits of thought and reflection, and at the same time, to fit for practical application, by giving knowledge of an extremely useful kind.

The book is suggestive, containing many references to the original papers on the many branches of this subject and indicating the line of study to those who wish to investigate further.

The first division is hydrostatics. The opening chapter deals with the equilibrium and pressure of liquids and applies the principles to the stability of embankments.

The second chapter is devoted to floating bodies and specific gravity, describing the instruments used in determining it and illustrating by many very interesting and useful examples.

The next chapter takes up the topic of gases; and in it Boyle and Mariotte's law is very clearly presented, and used in explaining the relations between the pressure, temperature and density of a mass of gas. Special consideration is given to the atmosphere, so far as it falls in line with the subject; and the theories of the barometer and thermometer, together with their applications are fully treated.

The second division is given to hydrostatics. It is practical in character, treating of the motions of fluids in pipes and taking into account the various elements, that determine the velocity of flow and amount of discharge.

A point of special interest is the attention given to the coefficients of resistance, due to friction, enlargement of section of pipes and so on. Many practical formulæ are theoretically derived and their application in practice fully set forth. The book concludes with a description of various machines whose action depends on the properties of air and water. The author has not been content to give the bare elements of the subject, but has furnished much valuable information and suggestion in such a way as to stimulate interest and study and so to contribute to the value of this book for training and instruction.

MISCELLANEOUS.

In the preparation of alumina, according to the *Chemiker Zeitung*, ferro-silicium is mixed with fluoride of aluminum in equal proportions, and the mixture is exposed to a fusing heat. The materials decompose each other, and volatile fluosilicium with iron and aluminum, are produced, the latter two bodies being alloyed together. In order to extract the valuable aluminum, a copper alloy is formed by melting the iron alloy with metallic copper; by reason of the greater affinity of the copper for aluminum this is secured, leaving with the iron only a slight residue of aluminum. When the fused mass is cold, copper-bronze and iron have so settled that both bodies can be easily separated.

VAN NOSTRAND'S

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ENGINEERING AS A PROFESSION.

AN ADDRESS TO THE ALUMNI ASSOCIATION OF THE STEVENS INSTITUTE OF TECHNOLOGY—JUNE 18, 1885.

By WILLIAM KENT, M. E., Stevens '76, President of the Association.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

A RECENT writer on Political Economy says: "The tournament of the world has changed its fields and its weapons. Men no longer strive with lance for a lady's favors. They struggle with matter to change its forms and add to its value. He who can render industry more varied, or more efficient, who can turn any element or gift of nature to novel use, is the winner of the prize." The same writer says further: "Society will not dispense with professional men, although they do not stand so far above the level as in generations ago. We will call upon lawyers when we get into strife over property and they are necessary to the social structure which protects the person. While sickness comes, doctors will maintain their calling. So long as there is a soul that longs for immortality, clergymen will be welcome in home and pulpit. But with expanding industries, with developing science, new professions have gained favor. Commerce has its spheres in which high training and strong intellects are needed and are well rewarded. So has the varied mechanism of this age. In the professions hundreds are starving in this country in the foolish pride of a decayed caste. In the walks of production, wealth invites every man who will bring brains and industry, which will win skill."

It is of one of these new professions, engineering, that I would speak, and I claim for it a rank as a learned profession equal to that of law or medicine, and second only to that of divinity.

Such a claim, for engineering to rank as a learned profession, worthy of all the honors that have been paid to the older professions, may seem to you, who are students or practitioners in that profession, as an unnecessary statement of a truism—one which needs no proof—you already accept it as almost an axiom. But it is not thus accepted by the world at large. In a recent conversation with a lawyer on this subject, he held that it was impossible that engineering could be of equal rank with the three learned professions of the olden time, since all its work tended only to the material advancement of the race; it benefited civilization only by the increase of wealth which it brought; it was of the earth earthy; in fact, a servant of Mammon; while the other professions were on a higher plane, preserving the life and the morals, and dealing with the intellect and with the immortal part of man. So Charles Dudley Warner, in the *North American Re-*

view for Sept., '84, writes of the "Demand of the Industrial Spirit" (of which spirit we must admit that the engineering profession is the hand-maiden) as denying the higher wants of the soul, as "demanding a radical revision of the college curriculum and that the ancient stamp of scholarship shall be put upon industrial and commercial pursuits." He says, "The last demand of the industrial spirit is that all education shall be lowered to its material aims; for lowered it will be, if all distinction is removed in academic honor between an education for the sake of the mind itself and an education dependent on and limited to material and practical aims. The danger in this is no less to science than to literature and philosophy. It is the greatest to all to the tone of modern life."

Such criticisms as these of the modern tendency of educational methods to fit men for the practical duties of life, make it necessary for us to be able to give a reason for our belief that such a tendency is not a degrading one, and that one of the results of such a tendency, that of placing engineering on the high plane of a learned profession, is not a danger to the tone of modern life, but one of its best safeguards, and is a real and important step in the advancement of civilization.

Let us first consider the requirements of the three professions which have hitherto appropriated the distinction "learned," and compare them with the requirements of the profession of engineering. But first we notice that the requirements of the three older professions are not now the same as they formerly were, but are becoming broader and more severe as the general public becomes better educated. In olden times, it might be sufficient for a lawyer to own a few books, to have a knowledge of the forms of law, and to have the ability to browbeat a witness and befog the judge; the doctor needed to be expert with the lance and with the leech, to have a wise expression of countenance like that of the owl, and be faithful in adherence to tradition regardless of the consequence; the minister should be a man of lordly mien, to be able to exercise proper authority and command the respect of his parishioners, and to have the grace of charity and general kindliness of manner, so as to make him always a welcome guest in their homes. In modern times, however, the requirements have greatly expanded. It is necessary for a lawyer, in counsel, to have such intelligence and such honesty, as will enable him to advise a client when to avoid as well as when to seek litigation; in advocacy, to have all the powers given by a thorough knowledge of logic and rhetoric, the quickness of perception, the eloquence and the profound knowledge of the law, which are needed in combat against similar powers arrayed on the other side. In medicine, a doctor must know when to withhold as well as when to give medicine, how to save a leg as well as how to take one off, and he must keep familiar with all the most recent discoveries of medical science, and know how to make proper application of them. In divinity the minister must keep abreast of his flock in intelligence; must be well versed in history, literature and science, as well as theology, to enable him to meet every new argument against his own beliefs which may be drawn from any branch of human knowledge. These three professions now all ask for the most liberal general culture, including not only a classical education, but a knowledge of the universe of learning, of all that is known or to be known of nature and humanity.

Let us compare these requirements with those of an engineer who should rank as a member of a learned profession. He should be a man of broad general culture. No branch of education should be looked on by him with contempt, and his culture should be a broader one than that given by the old college curriculum. The "demand of the industrial spirit" is a noble one. It is for a higher and broader education than that of Oxford and Cambridge. All the culture that the Greek and Latin tongues may give, all that history, literature, music and the fine arts may give must not be slighted. Do the classics give a man stronger reasoning powers? Does literature give him the graces of speech and the power of the pen to mold human thought? Do the fine arts give him the sense of the beautiful? All these are of benefit to the engineer; but to these he must add as more important to his professional success the knowledge of human nature and of finance, gained only in the school of business experience; and

of the higher mathematics, which he must use as easily as a mechanic does his two-foot rule; of the sciences which reveal to us the secrets of nature, geology, mineralogy, physics, chemistry, and their allies; and to all these he must add a sound body with a sound mind, a familiarity with the powers and the limitations of the mechanical trades, and a certain amount of personal manual dexterity.

So vast, indeed, is the field of knowledge which the profession of engineering requires as its foundation, that no one man can be expected to encompass the whole of it. As the jack-of-all-trades is generally master of none, so the engineer who attempts to become educated in all branches of even the ground work of an engineering education, not to speak of the branches of the profession itself, is apt to prove a failure. Hence the necessity not only of specializing the profession of engineering into the branches of military, naval, civil, mechanical, mining, electrical, sanitary, and the like, but also of making a discrimination as to the branches of general education which should be acquired as preliminary to an entrance into the general study of engineering, and of its special branches. Hence the specializing of schools of engineering, Rensselaer devoting itself chiefly to civil engineering, Columbia to mining, Stevens to mechanical engineering.

I may here mention the place which such professional schools should occupy in our general American educational system. Vice-Chancellor McCracken, of the University of the city of New York, said the other day: "The college is a school that teaches something about everything; the university is a collection of schools each of which teaches everything about something." He argues in favor of postponing university work in arts and science until after graduation from college, and says that the only foundation in America that has made its principal business to set up a university faculty in arts and science, the Johns Hopkins University enrolls this year 174 graduate students in arts and science, representing 97 colleges. He does not mention technical schools, such as Stevens, in his address, but I think that if he considered the matter he would advise as I would, that the technical school of mechanical engineering be ranked as one of the schools of the university; a school whose place should be to teach everything about something, and that something mechanical engineering, and not to teach something about everything, which is the function of a college. So also a collegiate education should, if possible, be obtained before entrance into the school of engineering, as the best possible preliminary preparation. And here I would agree with Vice-Chancellor McCracken in his argument against inviting sophomores to become specialists. He says: "It is injustice to a boy of seventeen, and the Harvard sophomore is but seventeen. It encourages him to a premature marriage of himself to some 'ology' or 'ism.' . . . How can the youth of seventeen, until he has looked over the whole field of learning, decide as to his turn of mind, whether it is not poetic or philosophic, or a mind for invention or for acquisition, for leading or for following, for the study of the world of things or the world of men."

So much for the requirements of the engineering profession as far as education is concerned. Let us now consider its requirements in actual work.

The work of the engineer has been defined as the overcoming of the resistances of nature, and the best engineer is he who effectually overcomes these resistances with the least expenditure of time, labor and money. The successful engineer must love his work for its own sake, and not for its emoluments. He must have the same professional pride that a good lawyer or doctor has, and be ready to sacrifice his money, fame, or even life itself, if duty should demand it. The responsibility thrown upon an engineer is sometimes one whose extent cannot be measured by a money standard. His mistakes may be more serious than those which hurt only the pockets of the lawyer's client, or those which the doctor buries six feet underground. Think of the mistake of the Ashtabula bridge, the engineer of which committed suicide; of the Tay bridge, the disaster to which is said to have broken the heart of its builder. And as to financial responsibility, how many millions of dollars have been lost by engineering mistakes. See the abandoned mines and mills in our

gold and silver districts, the silent blast furnaces and rolling mills built in the wrong locations, the waste of money and of life in the Hudson River tunnel and in the Panama Canal.

No higher trusts are assumed by any other profession than by that of engineering. It behooves that profession, therefore, as much as any other to be sensitive of its honor. Shall a judge be corrupt, or a lawyer defraud his client? No more should an engineer either give or take a bribe, or do aught to bring dishonor on himself, or to demoralize his associates. In manners he should be beyond reproach, but in integrity beyond suspicion.

In its rewards the profession of an engineer is not behind any other. If statistics could be brought to bear I have no doubt that professional engineers could be found, on an average, to be reaping greater financial rewards than the average of doctors, lawyers and ministers of the same number of years in practice. Mr. Roberts, in his "Government Revenue," estimates that of physicians only one-third earn over $2,000 per year, one-fifth earn between $1,000 and $1,500, the next one-fifth will strive for $1,000, and one-fourth will get only $600, $500 or less. Of lawyers he says the annual earnings of less than one-fourth are $2,000 per year, one-tenth in addition receive $1,000 a year. "No calculation can bring the number getting $1,000 a year from their profession to one-half of those on the rolls as in active practice. One-fourth do not earn $500 annually from legal business." I have no doubt that the engineering profession would show a much better record than this if statistics could be obtained.

In the reward of public fame and honor, no profession stands higher than that of the engineer. If a list of the benefactors of mankind since the time of Archimedes should be made, the engineers of the world would be conspicuous in it both in the number of their names and in the grandeur of their achievements.

There is one grand distinction between the professions of law, medicine and divinity, and that of engineering. The former are the professions of conservatism, the latter is the one of progress. The object of the profession of medicine is the conservation of life; that of law, the conservation of morals and the rights of property; that of divinity, the conservation of belief. Engineering, however, is essentially progress. Its history is one of continual advancement. It is like science itself, so far in fact that many of the advancements in civilization greatly credited to science pure and simple, are really the achievements of engineering, an applied science, of which pure science is but the handmaid. In this connection I may quote from Prof. Thurston's paper on the "Mission of Science," and you will note that the word "engineering" might be used wherever he uses the word "science." "A century ago, with the birth of the steam engine, later with the introduction of the product of the printing press into the daily life of the world, with the operation of the electric telegraph and the introduction of the railroad, began the real progress of science, and we are now seeing but the beginning of her awe-inspiring career. She has taught us to drive ten thousand tons across the seas by the might of over 12,000 horse-power engines. She has taught us to send printed messages across the continent; she has shown us how to drive railroad trains faster than bird can fly, yet the mission of science has made but the veriest beginning. It still remains to her to perfect and systematize a thousand new industries, to invent as yet unimagined new arts, to bring the laborer worthy of his hire all that he needs and all that he can desire for his own comfort, and for the care and comfort of his family, to adjust the power of production to that of consumption, and both to the working capacity of the world, so that the now seeming natural conflict between labor and capital shall no longer have even an appearance of existence."

Probably similar thoughts were in my own mind three years ago when writing from the Electrical Exhibition in London, I mentioned the possibilities of future achievements of one branch of engineering, the electrical, I said "These currents of electricity shall furnish power to drive our railway cars, our road vehicles, and our steam boats; shall furnish energy to run our sewing machines, to raise our water, to light and warm our houses and cook our food. They shall separate the ore from the dross, shall

reduce and fuse the ore into metal, and shall gild and refine not only our metals but our whole civilization. And when this is done—when man has subdued unto himself all the forces of nature and forced them to do his work, will he work any fewer hours or less hard? Will he take any more rest, or any more pleasure, or will he be the same over-worked, nervous, ambitious and dyspeptic creature that he is now? Will electricity solve the labor problem? Ah! these are questions apparently beyond the reach of our present philosophy, but they are questions which the future is bringing to us with terrible rapidity. It is wise to look them in the face."

I have thus given you briefly some of my views on the requirements of the engineering profession, of the work it is called upon to do, and of some of its future possibilities. I hope you see as I do, that the profession is not altogether of the earth earthy, that it is not altogether a profession whose end is simply the increase of wealth of a favored few, but that it is a profession charged with as weighty responsibilities and duties to the human race as any other; that it is the profession to which the world must look for nearly all future advances in civilization, whether these come through the enginery of war, civilizing barbarians by the means of modern artillery, through sanitary engineering, at the same time preserving the health and benefiting the morals of mankind, or through inventions which shall so increase the wealth of the human race at large that the primal curse of labor may be to a great extent removed, and the race have more time than it now has for the cultivation of its intellectual, moral and spiritual nature.

WATER METERS (COMPTEURS D'EAU).*

By CH. ANDRE.

Translated for VAN NOSTRAND'S MAGAZINE.

THE FRAZER METER.

There are two forms of this meter: the first introduced in 1872 and the second in 1878.

In the earlier pattern the general form was cylindrical; the smaller sizes being of cast iron and the larger of wrought iron plate. The water coming within the outer casing exerted pressure upon the pistons, of which there were four joined two and two, and single acting. There are two three-port slide valves, working horizontally. The valve for each pair of piston rods regulates the flow for the cylinders of the opposite pair. The registering wheel work is moved by the pistons acting on a ratchet wheel.

In 1878 Mr. Frazer patented his later form of meter, which differs from the first in several particulars. The outlet pipe is supplied with a rubber ball-valve, which serves to mitigate the water hammer shocks on the valves. The pistons turn upon their axes while making the stroke, so as to insure a regular wear on the packing.

There are two double-action pistons in place of the four single-action, thus reducing the length, weight, and price of the machine.

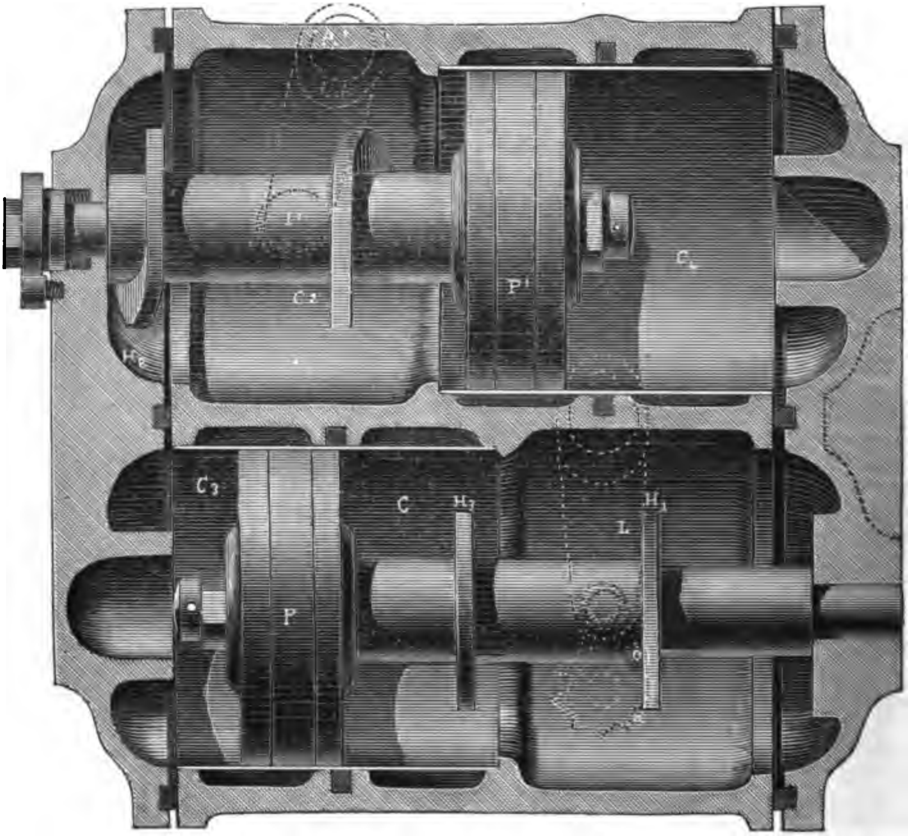
The water enters from above, passes through a strainer which arrests solid particles and also serves to keep the valves in place during transportation.

The valves rotate instead of working back and forward, and are driven by a key and collar on the piston-rod. The adjustment of the parts is such as to admit of examination of the meter at any time.

Furnished with a safety valve, this meter has been applied to the measure of feed-water for boilers.

The durability of the Frazer meter is very satisfactory. The packing of one at the Eastern Railway Station in Paris was recently renewed. It had registered 200,000 cubic meters, and the packing was still water-tight.

* The greater part of M. Andre's paper is devoted to descriptions already given in Mr. Browne's paper. Only the more important of the remaining ones are here given.



THE FRAZER METER (1872 Model).

THE CROWN METER.

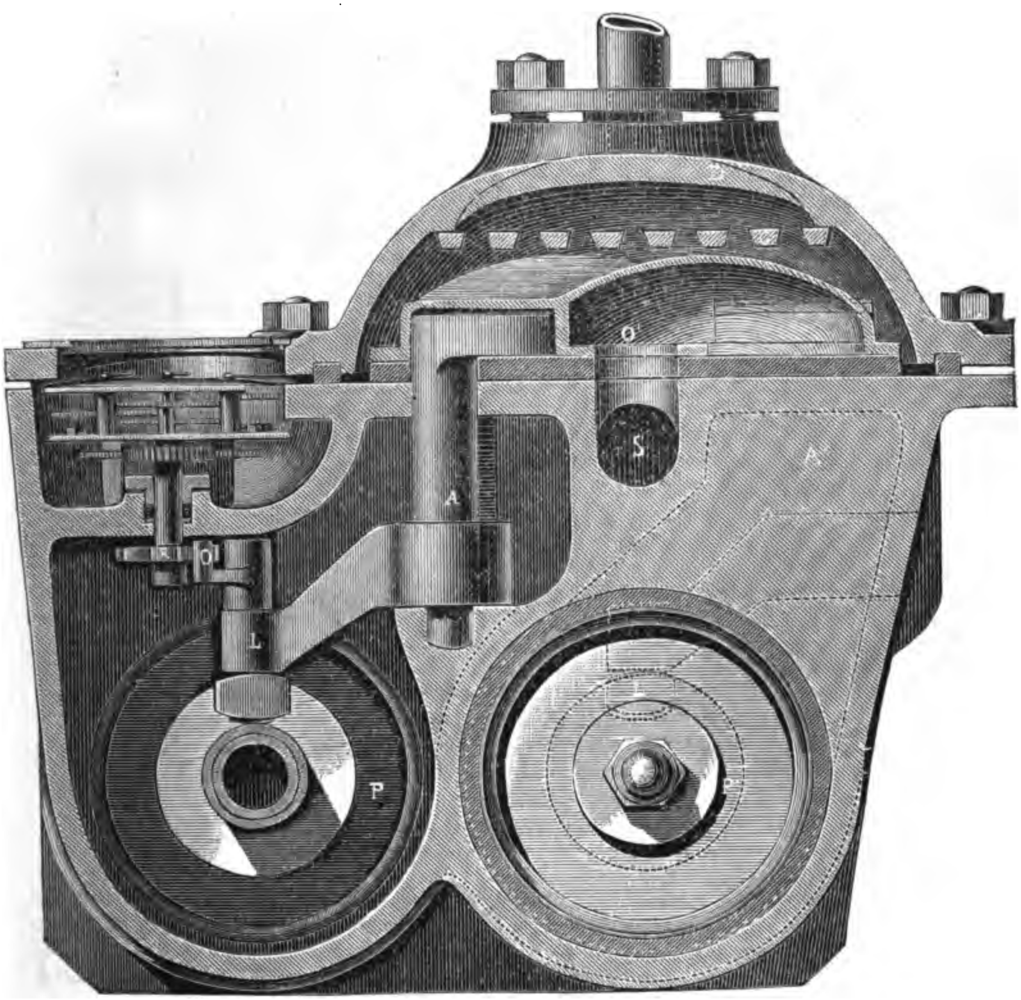
This meter has a rotating piston and is known in France as the Nasch Meter (Compteur Nasch).

The apparatus is enclosed in an iron box. The water enters below and passes through a copper strainer of large surface.

It is composed of four strong pieces, only one of which is movable. One of these pieces is the so called crown. It is substantially a heavy ring with indentations on the concave edge. A pinion rolling on the inside of this crown constitutes the piston. It separates the crown into two water ways. The piston has one tooth less than the crown. The piston has at the center of each face a cavity and a little further out a deep groove. The lower central cavity communicates with the upper groove by oblique conduits through the metal. In like manner the upper central cavity communicates with the lower groove. The piston and the crown are of the same height, and are situated between two fixed discs which perform the office of valves. These discs are perforated with curved conduits opening at their extremities on the side towards the piston. One of these extremities; the one nearest the point where the piston and crown are in contact, communicates with a groove, while the other extremity opens at 90° from this point into the open space between the teeth.

The water enters by a central hole in the lower disc, traverses the piston, is conducted into the upper groove, thence by the curved conduits into the space between the crown and piston and causes the piston to roll.

The water then goes out through the conduits of the lower plate; thence into the lower groove of the piston, passes



THE FRAZER METER (1878 Model).

through the piston and out through its upper central cavity.

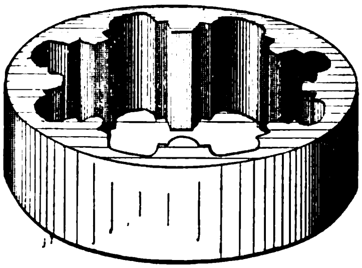
The piston carries on its upper surface a bronze rod, which describes a circle and drives the registering wheels.

The crown meter is especially adapted for measurement of large quantities of water under light pressure. It is also more exact when the delivery is small than are the velocity meters, but is less so than the ordinary piston meters. This naturally follows from the fact that the Crown meter is not furnished with packing and a water-tight piston. Its efficiency depends chiefly upon precision of adjustment and lightness of its piston, which is made of vulcanite. It is put in action by a water pressure of two or three centimeters. Six liters of water per hour will keep one at work.

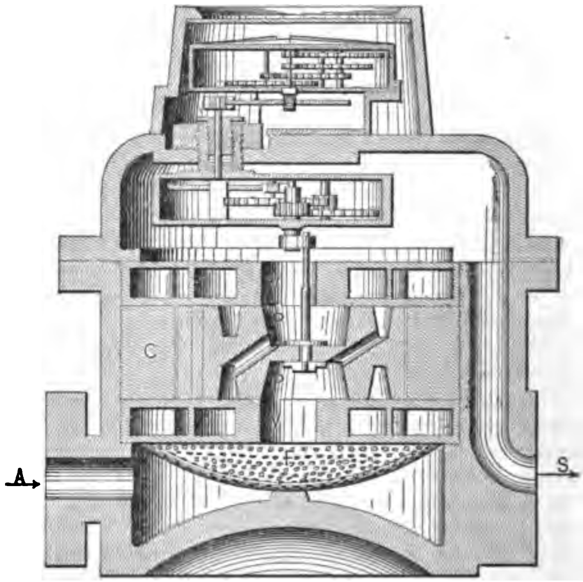
From the experience in French cities, it is concluded that where exactness of measurement is regarded as the first importance, only the piston meters of ordi-

The Crown Meter.

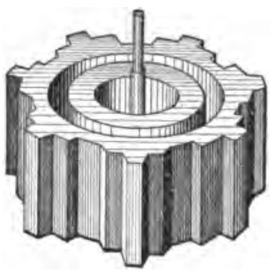
Crown, C.



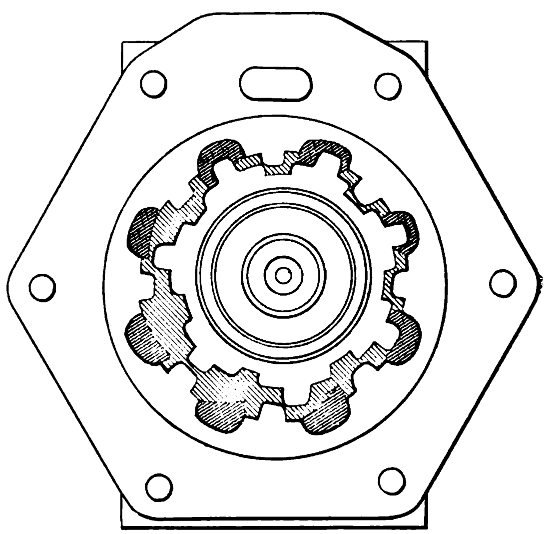
Vertical Section.



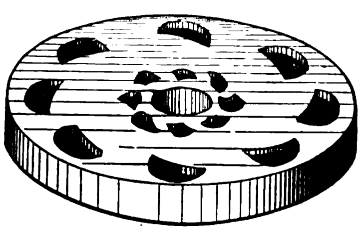
Piston, P.



Plan View.



Valves, V.



nary type are in use. In the provinces the English Siemens meter is most widely employed. Other kinds of meters may be profitably employed in measuring velocities. Piston meters are large and costly, but accurate and durable. The other varieties are small, easy to work, but less exact and more delicate.

The Crown meter occupies a sort of intermediate position between the two classes. Its capacity, its price, and its precision, entitle it to favorable consideration; but where solidity and durability are regarded as essential merits, the piston meters are preferred.

CABLE TRAMWAYS.

By W. N. COLAM.

Paper read before the Society of Engineers.

As is now pretty well known it was in America that continuous wire cables were first adopted for the purposes of street locomotion. The author, however, in addressing English engineers on the subject of cable tramways, thinks he is best considering the wishes of those present by dwelling but cursorily on the birth, history, and construction of the cable system in America; and in devoting a considerable portion of his paper to a description of the Highgate Tramway, which is not only the most recently constructed cable tramway, but the first ever made in Europe. In the case of all inventions which have eventually proved to be of great value, the honors of inventorship have usually been contested. Suffice it to say that the cable system of tramways is of comparatively recent origin, and that the author proposes to leave the question as to who originated it for others to settle. It is doubtful whether the invention in this case would, even at the present time, have been practically worked out had it not been for the fortunate circumstances that a few wealthy men living on the heights of the city of San Francisco had no means of easy excess to their dwellings.

The idea of employing a wire cable for hauling vehicles is, of course, very old. and has been utilized in a variety of ways, but the novelty of cable tramways consists in the secretion of the rope beneath the surface of the road in such a manner as not to interfere with the ordinary traffic, and also in the gripping arrangement, which allows cars of the ordinary type to be used. No two lines on the cable system have as yet been constructed alike, but the principle of working has in all cases been the same, and may be concisely described as follows:

On the cars and under the control of the driver a gripping arrangement is fitted which is extended down through a narrow continuous slot between the rails and into a tube under the surface of the road. Here it is so manipulated by the driver as to close firmly or partially upon, and, when required, to release itself from, an endless wire cable which is kept constantly moving by a stationary engine placed at any convenient spot along or near the line. The cable is supported on small vertical pulleys at suitable intervals on straight portions of the road, but at curves inclined or horizontal, pulleys are substituted according to the nature of the curve. Where very sudden and acute rises occur in the surface of the road (such as at crossings on the San Francisco hills) small inverted pulleys are used to depress the cable and keep it from touching the top of the tube. Such cases, however, are seldom met with in practice.

In the month of August, 1873, a few energetic and skillful men succeeded in overcoming the many objections and apparently almost insurmountable obstacles which invariably attend the introduction of new schemes, and the result of these labors was the construction of the pioneer cable line up Clay Street Hill in San Francisco. This hill is very steep, the severest gradient being as much as 1 in 6; and, like most hills around San Francisco, is leveled where intersecting roads cross, giving the longitudinal section of the road, a step-like appearance. The street length of this line is 2,800 feet, and the altitude risen to in that distance is 307 ft. The first cable used was 3 inches in circumference, and was worked at a speed of 528 ft. per minute for 17½ hours per day, and it lasted over two years. Owing to an agreement made with the local authorities, this line had to be constructed in about two months, consequently it was not built quite so substantially as others since. The line soon proved itself a great financial and mechanical success, and in 1877 extensions were made, but not in the same manner. The total length of cable at present in use on this line is 11,000 ft.

The Sutter Street Tramway Co. closely watched the results of the Clay Street trial, and three years and a-half after the opening of that line they had converted their line to the cable system, as it had (owing to the grades) hitherto for many years been worked unprofitably by horses. This company has now over three miles of double stock, crossing two of the most important thoroughfares in the city. There are four cables employed, all working from one engine-house, three of which work the main line and one a branch line at right angles. The total length of cable in use is 37,736 ft., running at 780 ft. per minute. Near the end of the branch line the cars have to pass round a sharp curve, and no difficulty is found in doing so. One year after the completion of this line the passenger traffic had increased 962,370, and the shares of the company previously offered at £5 sold freely at £12 10s. The construction of this line demonstrated the ease with which ordinary lines can be converted to the cable without seriously interfering with the traffic.

In 1877 the California Street Co. was organized, and opened their line in April, 1878. As the route for this line was very steep, at places being as much as 1 in 5.5, it was decided to build it in a more substantial manner than the previous two, which had wood in their formation. The tube here was solely and wholly a combination of iron and concrete, forming a strong homogeneous mass. The cables adopted were also heavier than before, being as much as four inches in circumference, The total length of double track is 12,650 ft. The engine-house in this case is situated in a valley, and contains two engines, hauling two cables of 8,840 and 17,055 ft. long, at a speed of 537 ft. per minute. Geary, a most central and popular street, was the next route chosen for a cable tramway, which was completed in March, 1880. The grades on this line are comparatively easy, the worst being 1 in 10.3. Here the engine-house was placed about half-way along a double track of 13,200 ft. This line has been very successful and free from accidents, notwithstanding it is crossed at right angles by three horse tramways, one of which also runs over its metals for a considerable distance.

In 1881 the Presidio and Ferry line of 13,000 ft. of double track was constructed. Some of the grades on this line are exceedingly heavy, the worst being 1 in 5.1. There is a curve at the intersection of two streets, the streets from both directions descending towards the curve, around which it was originally arranged for the cars to gravitate; but improved methods have since been introduced, so that either the main cable or an auxiliary one, running at a reduced speed, can be picked up by the cars.

In 1880 the wonderful mechanical and financial success of the San Francisco cable lines excited the attention of tramway companies in other parts of America, and even in the distant English colony of New Zealand. In both places the authorities, having convinced themselves of the value of the system, lost no time in organizing lines, and in August, 1880, the citizens of Dunedin in New Zealand saw the first cable line (out of San Francisco) opened. Roslyn tramway is practically a single line of 3 ft. 6 in. gauge, with turnouts. It is 3,500 ft. long, and the total rise is 500 ft. The steepest gradient is 1 in 7. An interesting feature of this line is an S curve of 215 ft. radius. The success of the Roslyn line induced the Dunedin people to further invest their money in the Mornington Tramway, which was opened in March, 1882. This line is one mile long, and the altitude attained is 430 ft., the worst gradient being 1 in 6.25.

The next city to recognize the advantages of the cable system was Chicago, and on the results obtained there, under so vastly different circumstances and conditions of climate, depended the spread of the system further east, or the limitation of it to milder climates. It may be taken as fortunate that immediately after the completion of the line, in January, 1882, Chicago was visited with heavy falls of snow and severe frost. The cable was at once well tested, and proved to be easily operated, never losing a trip, when all other vehicle traffic was suspended. Again, this winter, the cable system has been uninterrupted, notwithstanding the exceptionally severe weather Chicago has had. The Chicago company had at various times tried all the well-known tramway motors, and they finally reverted to the cable, although the city

is practically a dead level, and the company was doing well, as their $100 shares were quoted at $250. The cables on this line are worked from one engine-house, and are so arranged and actuated that three different maximum speeds are imparted to the cable, suitable for the respective districts through which they pass; thus in one place the cable is always moving at 315 ft. per minute, at another 716, and at another 804. At the end of the main cable line instead of switching the cars to the up line, they are run (at half speed) completely around a square block of buildings which entails four considerable right angle curves. The Chicago company has now a line capable of carrying over 10,000 passengers per hour, and on more than one occasion 100,000 people have ridden on the cars in one day. The annual passenger return before converting was 24 millions, and the first year after 27 millions, with the working expenses considerably reduced.

Market Street, the last line in San Francisco that was converted into the cable system, is by far the most important in that city. For a long time it was thought it would be impracticable to have a cable line in this street, owing to its already very crowded state at certain times of the day. Experience has, however, shown, that out of the eight lines of tramways passing along this street, those worked by the cable are the general favorites, for numerous reasons, amongst others the more uniform speed at which they travel. From 1860 to 1867 this line was worked by steam engines, and by horses from that date to 1883, when the cable was inaugurated. This road has about 10 miles of double track, and several branches worked by 46 heavy and large cars. Five cables of 4 inches circumference traveling at 750 ft. per minute are operated from two distinct engine-houses. The total length of these cables is 90,810 ft., and they weigh over 100 tons.

A noticeable feature in this line is a curve of 80 ft. radius which passes in front of the engine depot, and around which the cars are hauled by an auxiliary cable worked at half the speed of the main line cable.

Lines have been made in Philadelphia, and also one across the Brooklyn Bridge in New York; both, however, have proved failures from an engineering point of view, because in each case the authorities attempted to ignore the potents which cover the construction of good economical cable roads. In Australia the city council of Melbourne has been so satisfied with the report of their engineer, who specially visited and examined the cable lines of America, that they have sanctioned the construction of 27 miles of line in that city, which is now well in hand. The city of Sydney has also granted a company permission to construct a test line of a mile long, with the understanding that considerable extensions will be permitted upon its proving a success.

A cable company has been formed in New York, and the Mayor of that city has granted a concession for the construction of a system 72 miles long, part of which is to be street tramways, and part to run above the street as the present elevated railway does. The decision of the council and mayor has not only been influenced by overwhelming evidence of the behavior of cable tramways, but they have carefully considered the rapid increase of population which is annually being enormously augmented, and they have satisfied themselves that the cable is the only system which will be able to cope with the traffic of a few years hence.

The author has abstained from giving descriptions of the various gripping appliances used on the lines he has alluded to, his object being not to weary the members with described explanations of complicated machinery. He has, however, furnished models which show the principles upon which they have all been made. Having given some information as to what has been done in cable tramways abroad, the author will next proceed to describe the Highgate Hill cable tramway.

It is surprising that notwithstanding the rapid growth and great success of the cable system in America and our colonies during the last eleven years, it was not until May, 1884, that the first cable tramway in Europe was opened by the Lord Mayor of London, on Highgate Hill. To those who had worked hard in introducing the system into this country, it was reassuring to find how soon after

the opening of this line the objections of the local authorities disappeared, and the inhabitants living along the route prepared to vote for further extensions. The power to construct a tramway up the Highgate Hill was obtained by the Steep Grade Tramway and Works Co. during the session of 1882. In the summer of 1883 that company requested the Patent Cable Tramway Corporation (who own all the important patents necessary for constructing such lines) to furnish the designs for and construct their lines on the cable system. On the introduction of anything new into this country the idea introduced has usually to pay heavily for the privilege, and this occasion proved no exception, for the surveyors of the parishes through which this line passed were very zealous in watching the interests of their respective districts, and on so small a line as this their several requirements proved serious and most expensive obstacles to the completion of it.

Highgate Hill is a well-known steep incline rising from the junction of Archway Road, Junction Road, and Holloway Road, in the parish of St. Mary's, Islington; about half way up the ascent it is intersected by two other parishes, viz., Hornsey and St. Pancras, finishing opposite the old historical "Gate House." The summit of this hill and its surroundings is a great holiday resort for Londoners, and on many occasions this line has been severely tested by excessive and sudden rushes of traffic, which would have proved too much for any other system of tramway to have met.

The principle of the system working at Highgate is the same as on all the other cable lines in operation, but the details of construction differ in many respects.

The gauge of this line is 3 ft. 6 in., and it commences at the foot of the hill just where the rails of the North Metropolitan line terminate. The total length is 3,800 ft., of which 3,300 is double track and the rest single. The total height ascended is 239 ft., and the steepest gradient is 1 in 11. The tube is made of good strong concrete, and is connected with the concrete required by the local authorities for supporting the set stones and rail chairs. Tube frames of cast iron weighing 120 lbs. are embedded in the tube, at intervals of 3 ft. 6 in. The object of these frames is to support the Z-shaped steel rolled beams which form the slot in the road for the gripper shank; these beams, which weigh 36 lbs. per yard, are shown bolted to the cast iron frames. The rail adopted weighs 43 lbs. per yard, and is that known as Dugdale's patent. It is supported on cast iron chairs placed opposite each tube frame, to which they are connected by the bolts, firmly securing the gauge true to the slot. The slot is $\frac{5}{8}$ of an inch wide. From the surface of the road to the bottom of the tube is 17 inches, and the width of the tube is $8\frac{1}{2}$ inches.

At intervals of 40 ft. recesses are made in the concrete for receiving the 12 inch cast iron pulleys which support the cable in the tube. The pulleys run loose on spindles screwed into castings, which are held in their places by bolts built into the concrete. The pulleys are kept on the spindles by check nuts, which can be easily taken off whilst the cable is in motion, and the pulley removed and replaced through the hatch in the road. The hatch covers are light cast iron boxes filled with hard wood, and are dropped into cast iron hatches which rest partially on one wall of the concrete tube, and on the bottom flange of the Z beam. The hatch is built in by the paving, and cannot be detected easily in the road. The pulleys are lubricated by Stauffer's patent lubricators screwed into the end of holes bored up the centers of the spindles. At the single portions of the track, where the cable runs in opposite directions through the same tube, the spindles are made longer, and two pulleys placed on them instead of one. These pulleys are inserted where the road curves, and can be removed in the way alluded to before. All the recesses are drained into the sewers.

There are three portions of single track, and at the junction of the double and single tracks two tubes have to verge into one. The slots in following the tubes also converge, and in doing so leave a portion of the road between the slots unsupported. The designing of these points in cable tramways will always require considerable experience only to be obtained by careful observation during the working of such tram-

ways. The triangle formed by the junction of the two slots is a cast steel trough which is rigidly bolted to the Z beams, and supported by a cast-iron frame from beneath. The apex of the triangle is a strong steel spring screwed to the steel trough. This spring is sprung under the top bend, and against the side of the Z beam, where it is supported on a plate bolted to the side of the beam. Thus the slot is always open for the grip shank of the ascending cars. The inside of the steel trough is filled with set stones, thus reducing the metal on the surface of the road to a minimum.

The author will next proceed to describe the three brick pits under the roadway, and the machinery in them for diverting the cable at each end of the line, and at the point where it leaves and return to the tube in passing to and from the driving gear in the engine-room.

1st. The terminal pit at the bottom of the hill. This pit is rectangular in shape, 18 ft. long by 10 ft. wide, and 7 ft. deep from the surface of the road. It is strongly roofed over by rolled iron joists and concrete. It is lighted by gas, and access is obtained to it through a manhole in the road. The machinery inside this pit consists of a narrow cast iron pulley 8 ft. in diameter with V-shaped jaw; it is free to revolve horizontally on a pin vertically let into a cast iron carriage mounted on four wheels. The wheels rest on and traverse the lower inside flanges of two rolled iron joists set at an inclination, between and along which the carriage is free to move backward and forward. A chain is fastened to the lower end of the carriage, and is led over suitable gear to a heavy dead weight, the object of which is to keep the necessary strain upon the cable.

2d. The terminal pit on the top of the hill. This is also a brick pit 18 ft. long and 4 ft. 10 in. wide, and 10 ft. deep from the surface of the road. From the drawing it will be seen that the parts of the cable approaching and returning to and from this pit are quite close together; the cable has, therefore, to be returned to the tube in a different way to that adopted in the lower pit. As the slot passes over a portion of this pit, the roofing is arranged to carry the castings for supporting the slot beams. In this pit are two cast iron pulleys of 8 ft. diameter, one placed immediately in front of the other, one revolves in a vertical plane, whilst the other is canted sufficiently to throw its top out of plumb the same distance as from center to center of cable when passing through the single track.

3d. The pit in front of the engine-house is also of brick, and is so constructed as to be approached from the engine-room. It contains four 8-feet pulleys. The tube slot runs the length of this pit. The slot beams here are bolted to special castings, which are mounted on short cross iron joists resting on longitudinal joists. This arrangement leaves the slot open to the pit beneath. The engine-room is in the basement of the depot, the ground floor of which is used as the car shed.

In selecting the engine for working this line two important points had to be taken into consideration, first, that they should have a most sensitive automatic cut-off valve gear; and second, that they should be powerful enough to do the work of an extension two miles long. The nature of the work on cable tramways varies so much and so quickly that within an incredibly short space of time the engines may be seen both working hard and hauled around by the load. The engines chosen were a pair of high-pressure horizontals, with cylinders 14 by 28 inches, built by Messrs. Grafton & Co., of Cannon Street. They are fitted with Collmann's patent valve gear, which the author considers a very good one for the purpose, and he is thoroughly satisfied with the way these engines have always done their work, the valve gear being so effective that the brake arranged to act on the fly wheel is never called into play. The engines can be disconnected and worked separately when required.

On the engine shaft is fixed a cast iron helical toothed pinion which gears into a larger cast-iron wheel keyed on a countershaft, which also carries the grip pulley. It is this pulley which does all the work of hauling in the cable from the road. The jaws are of a long V shape, and can be adjusted by thinning down or packing up the wood bolted between the segment castings which form the jaws. Although the author consid-

ers this class of pulley gives the cable a rather severe pinch, he is satisfied it releases the cable freely, which is a great point in its favor, especially when used as at Highgate. These pulleys were made by Grant, Ritchie & Co., of Kilmarnock. Immediately in front of the grip pulley, and in line between it and the pulleys, is the arrangement for taking up the slack of the cable, which is something considerable when new or freshly spiced. Changes in the atmosphere will also affect the lengths of the cable. This taking-up arrangement consists of two long rolled joists laid parallel to each other, 18 inches apart and a little above the floor level. On the top flanges of these joists are mounted two 8 feet pulleys, the shaft of the one nearest to the grip pulley turning in journals forming part of a horseshoe shaped casting, which is arranged to be moved either way along the tops of the joists by suitable screw gearing. The front pulley is fixed on a shaft which turns in journals bolted firmly to the flanges of the joists. This latter pulley can be moved forward from time to time and bolted down as before, the operation being repeated as often as the stretching of the cable has exceeded the capability of the sliding pulley to take it up.

The boilers for supplying the engines are those known as the Babcock & Wilcox sectional type, with water tubes, and are worked up to 100 lbs. pressure. This class of boiler is being used very much in the States on cable tramways. For feeding the boilers, a small vertical donkey pump and an exhaust injector have been provided, the latter delivering the water into the boilers at a temperature of about 180° Fahr.

The author having thus far briefly described all the places and machinery through and over which the cable has to work, he now proposes to make one round trip with it, and point out its travel. The speed of the cable is six miles per hour. Starting from the top of the grip pulley, the cable makes one half turn, and passes to the farthest and fixed pulley on the taking-up gear; it there makes one half turn, and comes to the top, and passes over the pulley in the horseshoe casting, where it is again sent down and straight off to the bottom of the left-hand pulley, which directs it up to the large pulley set in line with the tube in the road, whence it is sent on its way down the hill supported on pulleys. On nearing the bottom of the hill the tube is not led straight into the lower pit, but is carried round between the tracks of the first turn-out. The cable leaves the tube at this point, and passes through 10-inch pipes into the lower pit, and on to the horizontal pulley, which directs it into the up-hill tube, in passing through which it is supported in the same manner as in the down-hill tube. Upon reaching the upper terminal pit, the cable passes the first pulley on to the top and over the second, around which it makes a three-quarter turn, and is sent up to the top of the first pulley, which is slightly canted to return it into the tube, close to the up-hill portion of the cable; from it the cable passes down the hill until it reaches the pulleys, where it is deflected downwards and into the engine-room, where it rises gently until it again reaches the grip pulley from which it started. The long length of cable which passes, in sight, through the engine-room, enables any fracture of a wire to be quickly detected, which can be remedied at once or at night as the importance of it demands. The cable used is 8,200 ft. long, and is 3 inches in circumference. It is made up of 114 crucible steel wires of No. 16 wire gauge, and formed into six strands wrapped around a hempen core. The guaranteed tensile strain of this cable was 80 tons to the sectional square inch. The gripper is made of cast steel, and consists of two principal parts, one of which is in the same piece with the lower and movable jaw, and the other with the upper and fixed jaw. The wedge shown, in being forced into or withdrawn from a shoe, raises or lowers the casting holding the lower jaw. The cable is thus seized firmly or slightly or allowed to run through the jaws, as it may be required that the cars should travel full speed, slow, or stand still; and by opening the jaws wide the cable automatically leaves the gripper. The jaws are lined with soft pieces of cast iron, which can be easily and quickly removed and replaced. Two grippers are fitted to each car, one on each end.

The grippers have hold of the cable whilst descending as well as in ascend-

ing the hill, and only release it at the termini and in passing the pulleys in front of the engine-room. At the latter place, as the cars slowly near the pulley, the drivers open their grippers wide, and the cable automatically leaves the jaws. The cars then descend by gravitation along the few feet of deviated track, the object of which is to take the grippers past and clear of the pulleys. On the cars again reaching the straight track, the cable automatically slides into the jaws of the grippers left open to receive it, and the drivers have only to screw up their grippers, when the cars are again carried on at the speed of the cable.

This being the first cable line in England, for obvious reasons it was considered advisable to try more than one form of car, and at Highgate there are now four distinct types of cars working.

1st. An ordinary short wheel-base tramcar with inside and outside seats, but not arranged to carry a gripper.

2d. A dummy or open car which has a gripper each end. This car and the first are worked coupled together, and demonstrates the ease with which the rolling stock of other lines of any system can be attached to and carried on by the cable cars.

3d. A bogie car of an ordinary design with gripping arrangements at each end.

4th. A long bogie car to carry 60 passengers fitted with gripping arrangement each end; this has proved a very remunerative type of car.

All the cars are fitted with two classes of brakes, one of common design which acts on the tires of the wheels; the other a slipper brake which acts vertically on the rails. The latter brake consists of an iron frame firmly secured to the bottom of the car; inside this frame slides a shoe fitted with a long block of hard wood, which the driver, through an arrangement of powerful levers, can quickly force down on the rails, thus utilizing a part or the whole of the dead weight of the cars and passengers. This brake was severely tested by Major-General Hutchinson of the Board of Trade, who tried it on a car which was allowed to gravitate until it acquired a velocity of about 20 miles per hour on a grade of 1 in 11, when the car was brought to a standstill within 30 ft.

The Highgate tramway has now been working most successfully for the past eleven months, during which time it has carried 563,408 passengers, with but two mishaps, which the author is glad to be able to say had nothing whatever to do with the system. Time will not allow now of a full explanation of these unfortunate occurrences, but the author suggests that all interested in the matter should refer to the Board of Trade inspector's report and be guided by it alone.

Engineers will doubtless see at once that the cable system admits of a large increase of traffic, with but small proportionate increase in working expenses. They may, however, be astonished to hear that the Highgate line has carried 100 per cent. more passengers, with an addition to the consumption of coal which was practically nil. It must be interesting to know that every cable tramway constructed and worked up to the present time has paid a dividend after the first year's working. The author cannot now give details of the comparative construction and working costs of cable tramways, but he gives it from his experience that a saving of about 50 per cent. can be generally obtained in a cable tramway of three miles, the cable traveling at 6 miles per hour, and a five minutes' service of cars, allowing also interest at 6 per cent. on cost of construction and equipment as against a horse tramway with an average speed of 4½ miles per hour.

There is little doubt that the time has arrived in England when horse traction on tramways must be superseded by mechanical motors. Therefore, in comparing the relative merits of the known systems able at present to compete, it will be important to remember that there is a large mileage of tramway rails already laid in various parts of the United Kingdom which would be too light to bear the heavier traffic of locomotives depending on adhesion, thus at the out set giving the cable system, even on level roads, an advantage over steam, compressed air, and electricity, an advantage which increases very rapidly with the severity of the grades, until a point is reached when the cable system only can

ascend, and even then work almost as easily as on the level. The latter statement is perhaps at first hard to believe, but its correctness will be apparent when it is remembered that the cable cars in descending hills still retain a hold of the cable, thus utilizing their weight due to gravity in hauling up the ascending cars, a power which on all other systems is thrown away by the application of brakes in descending grades.

The author will conclude by tabulating some of the advantages of cable tram ways, as follows:

1. The steepest grades are as easy to work as levels, the weight of descending cars being utilized to pull up ascending cars.

2. The cars when taking up or setting down passengers are absolutely stationary, the hauling power being practically detached for the time, thus in cases of necessity to avoid collision or danger to life the cars can be brought up instantly.

3. The method of working is noiseless. The stopping and starting of cars is performed so gradually as to be almost imperceptible.

4. Fewer cars are required on the road to do the same work, because the more uniform speed gives a greater mileage per car, and consequently a larger carrying capacity.

5. Sudden increase of traffic can be accommodated to a practically unlimited extent by merely bringing more cars on to the line, the motive power being already provided, and in action.

6. Considerable reduction in wear and tear of the surface of the road as compared with horse haulage, and of the rails as compared with lines on which engines are working; the latter often requiring about 100 per cent. more deadweight to secure adhesion.

7. The traffic is not affected by snow or frost, as evidenced by the following extract from a letter by the Superintendent of the Chicago City Tramway Company: "Though we had at one time frost (snow) two and a-half feet deep, we did not discover any bad effects in our construction or operation."

8. It is the only mechanical motor which does not introduce more weight into the cars than is required for horse tramways, thus saving a renewal of rails when laid lightly for horse traction.

9. It is the only system which prevents cars running away in descending a hill, when all brakes have failed.

10. Its very low cost of working, as compared with other systems, ensures its financial success.

11. It is the only system which is not seriously affected by the proverbial dirty tram rail.

12. Perfect cleanliness of carriage way, a very important sanitary advantage.

13. The extinction, wherever cable traction is introduced, of the barbarous horse system—a cruelty unworthy of humanity and civilization.

Tempering of Metals by Cold Flow.—The tempering of metals during the operations of drawing and spinning are well known to all conversant with the details of such work. Cartridge tubes must be annealed between the several operations. Ward's process of making bolts by the cold flow of iron, also requires that such bolts be annealed before the threads are cut; even in flat plates, it is known that the roll temper of thin flat plates of brass or steel is very uniform. It is stated that when Mr. A. H. Emery, of New York, was engaged in making his testing machine for the United States Government, the result of exhaustive experiments upon the subject showed that the temper of steel plates as they are rolled is more uniform than is possible by any later tempering. Some recent experiments on the substitution of low steel for copper and brass in the manufacture of cold-drawn tubing, showed that in the process of drawing the steel became very hard and of increased tensile strength. This new feature is extending the uses to which low steel can be applied. A cannon has been made from such metal by drawing a 6-inch tube with walls ½ inch thick; a 7-inch tube was drawn cold, and when warmed was forced over the first tube; a third tube was fashioned in like manner 8 inches in diameter, and when forced over the other two, completed a 6-inch gun, with walls 1½ inch thick, and cylindrical in outline. It is claimed that such a gun of this dimension, which has been submitted to the United States Ordnance Board for examination, has sustained a water pressure of 75,000 lbs. per square inch; this corresponds to a tensile stress of 100,000 lbs. per square inch of metal. Be it so; admitting that the gun did withstand such a pressure once, it is no measure of the resistance which can be relied on as a safe working pressure of the impact of explosion of powder. Some enthusiastic traveler, on learning that the tensile strength of certain tropical spiders' webs is greater than that of steel wire, has jumped to the conclusion that the ideal cannon would be made by reeling such webs around a thin steel tube.

TEMPERED GLASS.*

By FREDERICK SIEMENS.

From "Iron."

The invention, by M. De la Bastie, of so-called toughened glass, which caused a great sensation at one time, induced the author of this paper to give close attention to the subject, which he proposes to bring before the society on the present occasion. Being a glass manufacturer, there was every reason why he should interest himself in an invention which, entering the lists with great pretensions, claimed not only to revolutionize the glass trade as it then existed, but to supply a new material which should take the place both of glass and metals. The author soon discovered that the De la Bastie process could lay no claim to the advantages to which it pretended, being indeed not a real manufacturing process at all, but rather a somewhat impracticable addition to known methods of glass-making. The wholly finished articles to be toughened had generally to be annealed in the first instance by one or other of the usual means, and thereafter to be heated to such a degree as to render them soft; they were then immersed in a bath of heated oil, or other fluid, capable of being maintained at a temperature of from 350° to 400° C. without evaporation. The toughening of finished articles of glass in this way is not only a very costly addition to the original process of manufacture, but the articles themselves are very liable to have their shapes spoilt and their surfaces injured. But besides these objections, there is another important point to be considered, which is, the liability of toughened glass to burst suddenly into small fragments, either spontaneously or by a sudden shock, like the well-known Prince Rupert's drops, formed by dropping fluid glass into water, whose peculiarity of breaking up into powder has been generally supposed to be due to the sudden cooling of the soft or fluid glass. This theory is, however, only conditionally correct, inasmuch as the cooling influence which acts from the surface inwards is not in proporton to the bulk of the glass, but to its surface, and must always act more quickly on those parts where the surface is large in comparison with the volume. Even the simplest form—a sheet, for instance—cools more quickly at the edges than in the middle, owing to the large surface for cooling which the edges offer. If, however, the cooling is regulated so that at every instant of time the temperature of the article is uniform throughout, no internal tension or strain can arise, and there will consequently be no tendency to crack or break in the way described.

The author, having satisfied himself by a series of experiments of the true cause of the spontaneous fracture of glass, has invented processes of manufacture by means of which glass may be thoroughly toughened, or as he prefers to call it, hardened. The principle upon which the processes depend consists in cooling the glass, not in proportion to its surface, but to its volume or capacity for heat. The method employed will be readily understood by considering a sheet of uniform thickness, which, after having been heated uniformly to a sufficient degree, must be cooled on the surfaces of its two parallel sides only, leaving the edges uncooled. This is done by placing the heated sheet of glass between two cold slabs of suitable material, prepared in a peculiar manner. Uniform cooling of the whole sheet is thus secured, no matter what its shape, because the edges are not subject to the cooling influence caused by the surfaces between which the glass is placed. The plan adopted for various articles varies with their shapes; but it is on the principle of uniform heating and cooling that the author's processes of manufacturing hard glass are based. Of these the two principal are known as press-hardening and casting; but besides these there is a third, theoretically less perfect than the others, viz., semi-hardening or hard-tempering. This, though less important, may be advantageously employed where presses would be unsuitable, and casting

* Paper read before the Society of Arts.

impossible or difficult, as in the case of bottles, lamp chimneys, &c. Press-hardened glass has now been made, with constantly increasing success, for six years, at the author's Dresden glassworks. The output has steadily increased more than 50 per cent. annually, from £600 value in the first year, until last year it amounted to over £7,000, or more than ten times as much. As there is no indication of a diminution in the rate of increase, the author anticipates that the manufacture will assume large proportions. The articles are mainly of plate and sheet glass, either flat or bent into a variety of shapes. Besides plain work, decorated sheets, such as sign-boards with enameled inscriptions, figures, and other ornaments, form an important part of the goods produced; the process, as already stated, is, therefore, one of manufacture (the goods receiving through it their definite shape and decoration), and not simply one of hardening or toughening. The glass is so hard that the diamond will not touch it, and it cannot, therefore, be cut or bent after manufacture; it may, however, be polished, etched, and slightly ground; its strength is at least eight times that of ordinary glass. As only absolutely homogeneous glass of the best quality is suitable for hardening, care must be taken in choosing sheet or plate glass for this purpose, so that it may not be in any way faulty, or contain stones, bubbles, or other imperfections.

The process of manufacture is as follows: The glass is first cut in the ordinary way to the requisite shape and dimensions, and is then exposed to the radiant heat of a peculiarly constructed furnace until quite soft; as soon as it has attained the necessary temperature, it is placed between cold metal plates, to be cooled down with a rapidity which varies with the thickness of the glass, but is in any case very great. The heating and cooling of sheet glass of ordinary thickness last altogether a minute and a-half, a minute being the length of the heating, and half a minute that of the cooling operation. It is a remarkable circumstance that glass may be thus heated and cooled in so short a space of time without either cracking or breaking. This is altogether due in the case of the operation of heating, to the uniform temperature of the furnace, and to the heat being produced entirely by radiation; should these conditions not be fulfilled, the glass would break to a certainty. As regards the success of the cooling operation, this depends upon the uniform temperature of the glass before it is cooled, and upon that of the metal plates between which it is placed whilst being cooled. This uniformity of temperature and the total absence of draught, which would cause irregular cooling, are the conditions under which the whole operation can be carried on with assured success. It is most essential, as regards the good quality of the hardened glass, that the operations of both heating and cooling should be rapidly performed; it is also of paramount importance that the glass should be heated up to as high a degree as is compatible with its being removed from the furnace and placed between the presses, and one of the main difficulties in connection with the process was the arrangement of a proper mode of handling the heated glass, considering that it is almost in the molten state, and as pliable as a piece of cloth. The temperature to which the glass has to be heated is, therefore, far in excess of that of an ordinary annealing kiln, and it is owing to the high temperature employed that the glass can be bent and shaped, as also decorated and enameled, during the process of hardening. In the ordinary process of enameling, the glass can be exposed to a comparatively low temperature only, on account of the tendency to get out of shape. Retorts or muffles are generally used, and the temperature not exceeding that of an annealing kiln, the process of heating up is exceedingly slow, and the enamel to be fixed on the glass has to be of a very soft, easily fusible character; borax enamels are generally used, and even they cannot be properly melted so as to be thoroughly incorporated in the glass. The case is entirely different when glass is enameled by the hardening process; the temperature employed being so much higher, and the heat acting so much more quickly, a more refractory enamel, such as that used for porcelain, becomes available. While in the first case the enamel can be scratched off the glass, and does not resist acids, or even the ac-

tion of the atmosphere, the enamel on hardened glass is as indestructible as the glass itself. From this it will be evident, that the hardening is at the same time the most perfect enameling process, and by far the cheapest, no extra heating operation being required.

It will now be readily understood that press-hardening is essentially a manufacturing process, the same operation which hardens the glass regulating the shape of the article, and fixing upon its surface a highly refractory and consequently superior enamel, admitting of variations of color and design, practically unlimited. It would lead the author too far were he to attempt to enter into all the details of the manufacture of press-hardened glass, which are very numerous indeed, on account of the variety of articles made; these are still on the increase, and there is no saying how long this may continue to be the case. The surface of the metal plates, or moulds used for the presses may be so prepared as to produce more or less cooling effect on the glass as required. If the glass is to be hardened to a very high degree, the metallic surfaces must be of very high heat-conducting power, such as copper, and must be left quite bare; the glass must also be raised to a very high temperature, as it would otherwise crack during cooling. If it is proposed to harden the glass to a lower degree, surfaces of iron are used, this metal not being so good a conductor of heat as copper, whilst the temperature of the glass is also kept lower. By covering the surfaces of the iron presses with wire gauze, their cooling effect may be reduced to any required extent, so that a certain amount of hardening may be produced without rendering it necessary to heat the glass to such a temperature as to make it difficult to handle, or to cause it to stick to the furnace bed. If a still lower degree of hardening is proposed, the faces of the presses may be covered with asbestos paper, or even clay slabs may be employed. It is very essential to the success of the hardening operation, that the heating should be done quickly and by radiation only, otherwise the surface of the goods and their general appearance will be impaired. The bed of the heating furnace must be made very smooth, either by the use of clay, or of sandstone tiles, dusted over with talc powder, and should always be kept in perfectly good order; whenever it becomes uneven, or is otherwise damaged, new tiles are placed on the old bed. Semi-hardened glass is made in the same large radiation furnaces as press-hardened, by means of the hard-tempering process, of which the following is a description: Finished articles, which are of shape to which presses cannot be easily applied, such as bottles, are heated up to such a temperature as will permit of their retaining their form; each one is then placed in a casting of sheet iron, which is so arranged that the heated article shall not touch the inner side of the casing. In order to effect this, the casing is provided with internal projecting ribs, which retain the glass article in position, touching it only at very few points. The casing with the heated article of glass within it is allowed to cool in the open air. Whenever it is a difficult matter to handle the heated glass, instead of placing it hot into the casing, the casing with the glass inside it is inserted in the heating furnace for the requisite time, and then allowed to cool as before described. The hard-tempering process is only applicable to articles of nearly uniform thickness throughout; bottles with thick bottoms, for instance, are not fit to undergo the treatment, as they would be apt to crack both during heating and cooling. The strength of semi-hardened is about three times that of ordinary glass, and it is not affected to the same degree as the latter by change of temperature. The process finds much favor, as the constantly increasing orders sufficiently prove. To secure success, a properly constructed heating furnace is of the utmost importance as regards both processes. As already explained, it is necessary that there should be no draught within the furnace, that the heat should be uniform, and that the flame should not act directly upon the sheets or other articles of glass, which would be thus tarnished, and liable to break whilst being heated, or on cooling, if not heated uniformly. The furnace employed is the regenerative gas furnace, heated by radiation, which the author has lately introduced, with great advantage, for many industrial purposes, and fully described

in a paper he read before the Iron and Steel Institute, in September last.

The third and last process to be described, which the author considers the most valuable of the three, is a peculiar mode of casting hard glass. This has not yet been introduced on a manufacturing scale, but the experimental castings produced have turned out to be quite satisfactory in every way. They consist of floor plates, grindstones, pulleys, tramway sleepers, and various ornamental work. The author thinks that castings might be produced with advantage for many other purposes, especially in connection with the building trades; but this can only be ascertained after works are established, which are now in course of construction, for the regular supply of goods manufactured by this process, as already the case with the previously described processes. Glass may be cast in this way into a variety of forms, which it would be impossible to produce with ordinary glass, owing to the liability of the latter to crack whilst cooling; it has, moreover, at least four times the strength of common glass, and can be made much more cheaply. It is manufactured in the following manner: Glass, melted in a tank furnace, such as described at the meeting of the Iron and Steel Institute, already referred to, is tapped into molds, as with iron castings. The process thus far resembles that carried on in an iron-foundry, but differs from it, inasmuch as a special material is used in place of sand, and that the mold and the glass inside it are heated and cooled together. The material or mixture to be used in place of sand must be selected so as to have, as nearly as possible, the same conductivity and capacity for heat as glass. In such a case, the glass and mold forming, as it were, one homogeneous body, the glass will cool without crackling, even if the cooling process is comparatively quick, which is quite necessary if hard glass is to be produced. Glass cast in this way may have almost any variety of form and inequality of thickness, in the last respect this process differing entirely from those previously described, in which only glass of uniform thickness can be dealt with. If care be taken that the surface of the glass does not approach the outer casing of the mold, it does not much matter how the cooling is effected. The great point is that the mold and glass should be brought to a uniformly high temperature, which should be rather above that at which press-hardened glass is made. When fully heated, the mold is taken from the furnace and allowed to cool in the open air, which generally acts quickly enough to produce a good hardening effect upon the glass within. When cold, the mold is opened and the glass removed.

It will be readily understood, from the description given, that the three processes differ so materially from one another that hardly any resemblance remains to show that they are merely different ways of treating differently-shaped articles, in carrying out the principle of keeping the whole body of the glass at a uniform temperature during the operations of heating and cooling. The De la Bastie process, as well as the ordinary tempering processes employed, fail in not being founded on the principle set forth; glass toughened by the De la Bastie process being cooled in a fluid bath, and ordinary glass in kilns, the cooling action is most active on the portions offering the largest surfaces to the cooling influence, and hence in the one case there is a strong tension or strain in the molecules, which causes them to break up spontaneously; and, in the other case, to counteract that tendency, it is necessary that the glass should be cooled very slowly. In all cooling operations the principle developed in the paper ought to be the ideal aimed at, and the author is convinced that ultimately every kind of glass will be more or less hardened in the cooling process. There is no reason why this should be done quickly; it may be done slowly, so as to allow of the glass being cut and ground while still possessing increased resisting power, and having less tendency to break under the influence of change of temperature. In the future, hardened glass will bear the same relation to ordinary glass that steel now bears to iron. It will, of course, be a long time before this is brought about, just as it has taken a long time to develop the use of steel to such an extent as almost to have replaced iron in the market. As a proof of the extent to which the production of hardened articles of glass has been already developed, the author has placed

some samples of hardened glass on the table. The members of the Society of Arts will thus be in a position to judge for themselves as to the comparative value of this glass, as well as of its strength and immunity from temperature influences. In the collection is included samples of military water bottles, of which more than 10,000 have already been supplied, mostly to volunteer regiments in this country, and glass similar to that used for fitting up the chart-room on board the Inflexible, which was ordered after a report of trials made on board of the Glatton, where the tempered glass withstood the concussion of the firing of heavy guns. From the steady progress in the past there is every reason to believe that in future the hardening processes described in this paper will be applied to all manufactures of glass of an important character.

Several experiments were made at the conclusion of the paper, to show the strength of the tempered glass, pieces of ordinary sheet glass and of the tempered glass being placed on four corks, and a cricket ball dropped upon them from various heights. The ordinary glass broke with a fall of about 2 feet, whilst in some cases the tempered glass did not break except with a blow from a height of 5 feet 4 inches.

Discussion.

Mr. E. A. Cowper said great thanks were due to Mr. Siemens for bringing forward this new manufacture in a practical form; but it was not a new thing, nor a small matter, seeing that the business had risen to £7,000 a year in six years. Several of the drinking bottles he had seen for some years past, and they were very much liked, because they were clean: tea could be put in them one day, and beer another. The question of cutting with a diamond was a very curious one. You could scratch the surface of some of these sheets, but you could not cut it; it would not split through as common glass did. Probably that was due to common glass being in a state of tension, which, when relieved by a scratch, caused the glass to fly right through; so that even plate glass, half an inch thick, would succumb to the slightest scratch of a diamond one-hundredth of an inch deep. With this glass you could not do that. The castings would be an important new manufacture. The tempering was not so thoroughly carried out in this case as in the hardening of glass in other forms, but it was sufficiently hard to serve as sleepers for tramways and railway chairs for electric railways. He thought, also, architects would welcome this as a means of obtaining articles of various tints, whereas they at present had to search for different stones. By this process they could obtain them, to any extent, in various tints, and in any form, by casting. Something of the same sort was attempted some years ago by Mr. Attwood, who used basalt, which was cast at Chance's factory in Birmingham. He made some heavy castings for mantelpieces, and so on, but it was a complete failure, as there was no means of hardening or probably annealing them. Some flew to pieces. One of the defects of the De la Bastie process was that the articles sometimes suddenly burst into little bits, the reason of which no doubt was that, being dipped into a liquid, one part necessarily got cool before another. If you wish to set up various strains by producing various temperatures at the same instant of time, there you had the process in perfection, for it was impossible to dip a glass article even into oil at a high temperature without chilling it in such a way as to produce varying strains. The cooling surfaces Mr. Siemens used were, first, a large plate of iron, on which the glass was quickly laid, and then the top plate came suddenly down upon it, and it was squeezed perfectly flat, so that every part was in contact with the iron. If it were merely laid on a sheet of iron the glass might cockle a little, and the proper effect would not be produced. Various experiments would be necessary to give the exact comparative strength, and he should have liked particularly to see experiments on the actual tension by pieces being put into an hydraulic press and pulled apart. It was evident this product was very much liked in Germany, and some day, no doubt, it would be equally popular in England.

Mr. Ludwig Mond said he had been much struck by the difficulties which Mr. Siemens had so successfully overcome in this manufacture. As he and his lamented brother had been the originators

of processes which had wrought such great changes in our art industries, it was quite certain that, in undertaking this question, Mr. Frederick Siemens would solve it successfully, although it had baffled the efforts of others. He regretted that Mr. Siemens had not given some description of the furnace he used. As a chemist, he might remark that, when the question of glass turned up, it was found to be now practically the same substance chemically as that it was at the time of the Phœnicians. He certainly thought it would repay the chemist to attack the question from the chemical point of view, and see what could be done, not by mechanical processes—by heating and cooling—but by attempting to produce new chemical compounds which should possess the qualities desired.

Mr. J. Head then drew attention to the various specimens exhibited, particularly to the ornamental glass, which was ornamented and tempered at the same time. Another useful application of this process was in enameled plates with names and addresses for streets, &c., which might be made very strong and cheap. A sample of this kind was not on the table, but one was being made, bearing the name and address of the society, which would be presented to it. Amongst the other specimens he drew particular attention to a large casting in the shape of a tuning fork, which was very resonant on being struck.

Mr. Siemens remarked that that shape could not be produced at all in ordinary glass in a mold, as it would be sure to crack. It could only be produced by pouring it into a mold formed of material having the same heat-conducting power.

Mr. R. W. Wallace referred to some experiments he had tried some months ago with the De la Bastie process, with the result that in almost every instance, with a less glow than had been shown to-night, the object had suddenly cracked, and the appearance of the fractured glass was altogether different; it broke into a great many pieces, and when you examined the fracture, it was evident that the cooling was done in an altogether different manner. The structure seemed to be quite amorphous, and it broke up into a condition like sand. He had had some experience of these drinking bottles, for two years ago they were served out to the Inns of Court Volunteers, who took them to Brighton on one occasion and to Portsmouth on another. During the whole time he only saw one broken, although they were subject to very rough usage, such as knocking the butt end of rifles against them, and throwing them about the floors of the barrack rooms, every effort being made by the volunteers to try and show that they were no better than those they had had before. In the end, however, they gave unbounded satisfaction to every one who used them. He hoped to hear something of the applicability of this glass for chemical experiments. Some years ago he tried some experiments with retorts toughened by the De la Bastie process, but they were not successful. Glass could be used in the manufacture of sulphuric acid, but only in one process; you could not manufacture the acid continuously. If you brought the heat up gradually and allowed it to cool gradually, you could concentrate sulphuric acid in a retort; but if you tried to do it continuously the retort cracked immediately. With a De la Bastie glass retort he got a slightly better result than with ordinary glass, but not a successful one, and he should like to hear whether any attempts had been made in that direction with this glass. Of course he could see there would be a difficulty in getting the annealing surface on the inside of the retort, but perhaps that might be met by making the retort wider at the top. All who were interested in these matters owed a great deal to the firm of Siemens for the energy and skill with which they had attacked these problems. Some day, perhaps, they might have bells made of glass.

Mr. Perry F. Nursey said, about ten years ago toughened glass by the De la Bastie process was brought under his special notice. He was requested by some friends to investigate and report upon it, and for that purpose he went to the factory near Paris and saw the process, which was heating the glass and plunging it immediately into hot oil. It was singular that long before De la Bastie worked out that process, he attempted the very means which Mr. Siemens had adopted, but without success,

for he attempted pressure, adopting the idea from Sir Joseph Whitworth's process. He had had the honor of reading a paper in that room on the subject, and perhaps some were present who witnessed the experiments on that occasion. To his mind, those shown that evening did not compare with those which he then showed. He might say that when he visited M. De la Bastie's factory, he tried an experiment with a number of champagne tumblers which were placed edgeways on a shelf. He fired at them with a saloon rifle at twelve paces; several times he knocked them off their perch, and one obstinate one he knocked off twelve times in succession, and only broke it at the thirteenth shot. It was a very severe test. In order to ascertain what the real strength of the material was, he carried out a series of tests in conjunction with Mr. Kirkaldy, and their report was made on May 18, 1875. There were ten pieces of tempered and ten of untempered glass of various lengths, 12 to 15 inches, and 4 inches broad, and all the same thickness, about 0.265 inch. They were placed on supports giving a bearing of 5 inches, a block of wrought iron being cut out to make a pan beneath. Under each edge were laid strips of indiarubber, and on the top was placed another piece of rubber, and on that pressure was brought from a knife edge, in some instances by a gradually increasing weight; in others the strips were placed in a testing machine, and the knife edge brought on them horizontally. The mean result was that the ordinary glass stood 206.2 lbs.; the tempered glass 828.1. Mr. Cowper had referred to the desirability of having tensile tests made, and he hoped he would carry that wish out, but he believed it would puzzle his ingenuity, as it did his own and Mr. Kirkaldy's, for they could not get the glass to be held by any known means; they could not get a bite on the glass. With regard to the tests made by Mr. Wallace, he did not think the material could have been properly tempered. In some instances, glass articles made by English manufacturers did not come out so well as those made by De la Bastie himself. Having quoted from the report in the society's *Journal* the account of his own experiments, he added that on that occasion one gentleman dropped a plate of the glass on an iron hearth at a distance of from 1 foot up to 5 feet without breaking it. He did not deny that Mr. Siemens had improved on this process as far as ornamentation went, but he did not think De la Bastie had attempted anything of that kind, though his glass could be ground by the sand-glass process. He had some tumblers at home, beautifully engraved with his monogram, which had been in use for ten years. The process had now dropped into abeyance, but recently he understood it was being brought out by another company.

Professor Grylls Adams, F. R. S., said he was very much interested in seeing experiments with regard to the breaking strain of glass, and one could judge at once by comparison of the heights the amount of energy actually spent on the glass. In one case dropping a ball less than 2 feet broke the ordinary glass, whilst it went up to 5.4 to break the tempered glass, and taking the comparison between the two, it would give an estimate of the actual energy spent on the glass. Of course, it was of great importance, in performing such experiments, that they should be exactly under the same conditions, and these could only be taken as a rough proof that one was much stronger than the other, and it was hard to judge what the actual strength would be. He was much interested in the strength of glass, principally from an electric point of view.

Mr. D. Chadwick said he was disappointed at not seeing any toughened glass experimented on, to show the relative strength of that and Mr. Siemens'. He could endorse all that Mr. Nursey had said about the De la Bastie glass, which he had experimented on over and over again, and he felt certain it would stand greater violence than that which had been shown that evening. Mr. Cowper had spoken of the way in which that glass was cooled, which caused it sometimes to explode spontaneously, and no doubt ten years ago that did occur, but as now made, such a thing was so rare that practically it did not apply at all.

Mr. McLaughlan asked if any attempts had been made to make this glass into

tubes, and also what was the cost as compared with other materials.

Mr. R. M. Lawes also asked the relative cost of manufacture by this process. The other day, on purchasing some toughened glass, he found it was nearly double the price of ordinary glass, if this were as dear as that, it would not be likely to come into general use.

Mr. J. Stone said he remembered attending the meeting when the De la Bastie glass was introduced, and it was then stated that slabs of glass for roofing purposes, if they cracked at all, cracked immediately into thousands of pieces, which of course was dangerous for workmen, but he noticed that this glass did not break at all in that manner.

Mr. Frederick Siemens said his process and that of De la Bastie could scarcely be compared, because the latter was merely a toughening process, whilst his was one of manufacture, by means of which he shaped and hardened the glass in a single operation, either by means of presses or casting into moulds, and in a new manner. The De la Bastie process was only an additional operation, applied after the article was finished to toughen it; but even for that purpose it was wrong, inasmuch as the cooling influence acted in proportion to the surface, whereas it ought to act in proportion to the bulk of the glass. Those parts which exposed much surface to the cooling influence, cooled more quickly than those of less surface, and, consequently, there would be unequal cooling which should be avoided. At each unit of time the whole article should be at one temperature, and that could only be effected by regulating its temperature according to its capacity for heat. If one part was cooled more quickly than another, there was a strain which could never be removed. For that reason the process of cooling in a bath was wrong; whilst the slow cooling applied to ornamental glass. was expensive. By the ordinary mode many articles which he had shown could not be produced at all, even if cooled ever so slowly, for they would have very little strength, and the least accident would cause them to break. Toughened glass was apt to break spontaneously, owing to the tension set up during the process of toughening; generally speaking, if it did not break very soon, it would last a long time; but its liability to break was the reason it was expensive. He had omitted many points of detail from the paper for the sake of brevity, but he described three different processes, in each of which there were many peculiarities. He had already described the construction of the furnace at the Iron and Steel Institute, and a great deal depended upon it, not only as regarded the success, but also the economy of the operation. [Mr. Siemens drew a rough sketch on the board to show the kind of furnace he used, in which the flame was shown to pass over the top of the furnace without touching the articles, radiating the heat down upon them.] On the bed of the furnace were tiles on which the articles were placed. The flame was about three feet from the glass, which caused a uniform heat, and prevented injury to the articles themselves and to the bed of the furnace. The articles were removed with wooden shovels, impregnated with water glass so as to render them incombustible, and they were then placed upon a cool metal plate, upon which another was pressed down. He had only brought forward manufactured articles such as were sent out to be used; the bottles would stand four times the ordinary wear and tear, and the sheets eight to ten times. He arrived at this conclusion from the circumstance that the breakages of the street lamps of Dresden and Berlin were now only about one-tenth what they used to be, and only cost one-tenth for repairs. He could have prepared glass which would stand very much more strain than that tested; he might have selected pieces which had stood the test already; but those shown had been taken quite at random. Sometimes a piece would break at five feet, though it had already stood the test at ten feet, or it might be dropped on the ground several times without injury, and eventually break, the difference of result depending entirely upon how the glass was struck. Bottles and hollow articles were the most difficult to harden; the sheets and hard castings were the most perfect, but hollow articles could not be made very well in the way in which those were made. Pipes might be cast, moulding them somewhat as iron pipes were; but it would require a little ingenuity. The

strength of an article could be increased by heating it to a higher temperature, and cooling it more rapidly, but there was a certain limit when the manufacture became unsafe; and for commercial purposes it was necessary to avoid losses through breakages, which would make the articles expensive. With regard to the price, it differed very much, some articles were very cheap indeed; it did not cost more to glaze a street lamp with tempered than with ordinary glass, but then the glass was supplied in sheets ready cut to the exact size, and the expense of the glazier's time in cutting the glass, and the loss thereby occasioned, was saved; but it could only be introduced for that purpose as corporations of towns came to have lamps or window panes of a uniform size. Decorative glass was produced more cheaply by this process, because the ordinary mode of decorating was by an additional process which was very expensive, the glass having to be heated in a muffle slowly, besides which the heat used to burn in the designs and inscriptions was not high enough to fix them well.

There was hardly any limit to the designs which could be produced in this way; anything that could be done on porcelain could be done on this glass, and it could be done more cheaply. It would be useful for signboards of houses or shops, but there must be an establishment near the place where the glass is to be used, as it would not pay to send single sheets from Dresden to London. With regard to the hard castings, he had not had so much experience, because they had not yet regular manufacturing establishments, and had hitherto only manufactured them experimentally, whereas real knowledge on such subjects was only to be attained by practical working. The hard-casting process, although it had not yet been brought out commercially, was, in his opinion, one of the utmost importance, as it was an entirely new process, by means of which glass might be made in any shape which could be molded, and of a strength which could not otherwise be produced. The material employed for molding was certainly dearer than the molding sand used for castings of iron. This was due to the circumstance that it must not only be suitable for molding, but had to be, when molded, of the same conductivity and specific heat as glass. He was still making experiments, so as to obtain the most suitable material for this purpose, but had found various mixtures of powdered porcelain and glass pots, metal turnings and filings, and such minerals as heavy spar and magnetic iron ore, to be suitable when mixed in certain proportions. On the whole, these materials were not dear, and, as regarded the labor and expense of molding, it would probably be about the same for castings of glass as of iron. As a manufacturer, he looked upon cost as a most important matter for consideration, as, however good a thing might be, it was of no value commercially unless cheap enough to find buyers. As regarded the hard-cast glass, he could produce a hundredweight of castings for about 5s. 6d., which should be cheap enough for any purpose for which it was proposed to be used. He was now erecting a factory which would be at work in a couple of months' time, and later on he should be pleased to give the society further information on the subject. He felt quite satisfied that orders would come in, for his hard-glass castings supplied a want which was felt on all sides. Glass was not liable to oxidation or to wear away, and as soon as it could be depended upon for strength, and could be made cheaply, it would be applied for purposes for which metals, stone and porcelain had hitherto been used. If a factory were established in London, he believed a great trade would spring up.

Mr. Wallace asked if slag could be utilized.

Mr. Siemens said it depended largely on what the slag consisted of, for this varied very much. The only advantage would be in running it direct from the blast furnace, so that it need not be remelted, whilst there was the disadvantage that it contained no alkali.

Mr. Head remarked that, considering these articles were only one-third the density of iron, and cost 5s. per cwt., they would be really very much cheaper.

In reply to the chairman (Professor James Dewar, F. R. S.)—

Mr. Siemens said, one physical property in which this glass was different from other glass was that it stood changes of temperature better. It might be a little

more elastic, but it was not tough, only hard.

The chairman said that the specimens of the De la Bastie glass which he had seen in the field of the polariscope at once revealed enormous strains; he should like to know if this glass showed anything of the same kind.

Mr. Siemens said it showed something, but not much.

Professor Adams said he should like to have the opportunity of testing this glass in that way. There was no question that the toughened glass was very much under strain, because the usual figures of unannealed glass, depending on the shape of the glass, were very clearly marked.

Mr. Siemens said if they could ever come to such perfection as to cool actually in the ideal way, that appearance would be taken away. Some pieces showed it more than others.

The chairman said this was a most interesting communication in every respect, and fully worthy of the Siemens family. They always found in the papers of Mr. Siemens and his brother that they began on some perfectly definite basis to achieve a particular result. Any one attacking this problem for the first time would hardly have thought of trying industrially to eliminate from any given surface of glass a quantity of heat which should be perfectly uniform and definite; and that, achieving that, you might do it at as rapid a rate as you could attain. In fact, he understood that the more rapid the more successful it would be, provided it was uniform. Here the scientific problem was to withdraw a certain number of units of heat quite uniformly, and at a uniform rate, from each side of a piece of glass, so that the old idea that the only way to reach uniformity in a plastic solid of complex nature like silicate, by slow cooling, and allowing the molecules to gradually attain their normal state of want of strain, might be achieved practically instantaneously, if done in a proper way. It was really a most remarkable result, not only in itself, but from the applications it was likely to open up in the future. Nothing succeeded like success, and the figures given as to the demand for this manufacture were sufficient proofs of its usefulness. He concluded by proposing a cordial vote of thanks to Mr. Siemens for his admirable paper, which was carried unanimously.

ON THE ANTISEPTIC TREATMENT OF TIMBER.

By SAMUEL BAGSTER BOULTON, Assoc. Inst. C. E.

From Papers of the Institution of Civil Engineers.

II.

DISCUSSION.

Sir Joseph Bazalgette, C. B., President, said the subject of the paper was an exceedingly practical one. Timber, in the majority of countries, was the most available material for constructive and engineering purposes, and in some countries it was almost the only material which could be used. The great defect in its use was its want of durability. Anything, therefore, which could remedy that defect, and give durability to the timber, must be a subject of great interest to the engineer. The author in the paper had given the result of thirty-four years' experience, together with his researches into what had been done ages before, and the whole had been placed before the members in a manner showing that he had devoted very great ability and attention to the subject. Although the author was commercially engaged in that branch of engineering, he was sure the members would feel that the paper had risen considerably above the commercial element, and had clearly shown that science could be, and had been, brought to bear on industrial art, so as to improve it and make it of great value.

Mr. Boulton remarked that the subject of his paper was one which had occupied his attention for many years. He hoped he had clearly explained the analytical investigations by which he had sought for

some clue to what was a rather complex labyrinth, namely, the kind of substance which was the best to put into timber for its preservation. He had employed many of those substances, and the conclusion at which he had arrived was that, supposing the substance to be a good antiseptic, whether, as in former times, corrosive sublimate, sulphate of copper, or chloride of zinc were used, or whether creosote oils, there was always a close connection between the durable results of the antiseptic and the immunity of that antiseptic from volatility in the air or solubility in water. Timber must be exposed to air for engineering purposes, and also to water; in some cases in marine work, it was in the water altogether, and therefore, the antiseptic ought not to be liable to evaporation or to being washed out by the action of the water. He was not there to advocate the use of the creosote of one district more than another, because, commercially speaking, that was a matter that did not affect him. He thought that all honest creosotes made from coal-tar in England were useful for the purpose of preparing timber; but he thought that some of them were more useful than others, because they were more durable. If, therefore, engineers would take the trouble to follow out his idea, and study the different constituents of the creosote oils, remembering which of them were the most durable and the least soluble, that would give a clue to the formation of fresh specifications for the preparation of timber. He did not think that the present specifications were satisfactory in all respects. Plate 6 represented the products derived from Newcastle coal, such as was ordinarily carbonized in London gasworks. There was, as had been explained in the paper, a different series of products from the Midland coals ordinarily carbonized in other parts of the country. Taking the same coals—those carbonized in the gasworks—and subjecting them by carbonization to a lower temperature, another class of products would be obtained as had been pointed out by Dr. Armstrong in a recent discussion. It was as well that engineers should bear that in mind, as they were now witnessing an inauguration of a new series of industries, namely, the partial carbonization of coal in coke ovens, partly for the purpose of getting the products direct, instead of through the gasworks. Those other products might be valuable, but they were not the same as far as the preparation of timber was concerned, for they were lighter and more volatile. He had taken the trouble to get some truckloads of different coals used ordinarily by the London gas companies; he had carbonized them on a large scale at lower temperatures, and had found that he obtained thinner and lighter oils with a specific gravity of from 930 to 1,030, instead of from 1,045 to 1,060, and he had a different class of products altogether.

Dr. C. Meymott Tidy said it was twenty years ago when he commenced working with creosote, and he was bound to admit that since that time his views had undergone considerable changes; but he supposed there was no great harm in that, for the views of engineers, politicians, and even of theologians, were constantly shifting. The process of creosoting was of a threefold nature. First, there was the physiological action of rendering the wood a poison, so that animals could not or would not attack it; secondly, there was a chemical action, consisting chiefly in the coalgulation of the albumen; and thirdly, there was what he held to be by far the most important action of the three, namely, the simple mechanical action. The process of creosoting was practically a choking up of the pores of the wood so that neither air, moisture nor life could get inside. He well remembered the late Dr. Letheby drawing up his original specification. No doubt he was very strong in his belief of the enormous value of carbolic acid; indeed, he regarded it, as the author had stated, as probably the most important ingredient of the tar. In the last specification which he, Dr. Tidy, had drawn up a year ago, and which was now being employed largely, he had laid down three essentials, and as they practically represented the views which he held on the subject at the present time, he might be allowed to refer to them. The first point of the specification was that the creosote should be completely liquid at a temperature of 100° Fahrenheit, no deposit afterward taking place until the oil registered a temperature of 93°. That point was considered very fully. The temperature at which creosoting was performed was about 120°. It did not appear to him to matter one iota

how solid the creosote was (and he was bound to say that from his point of view the more solid it was the better), so long as it was liquid at the temperature at which the creosoting process was done. Seeing that the process was carried out at a temperature of 120°, he thought he was right in specifying that the creosote should be liquid at the temperature of 100°. The next point was (he had left out a specific-gravity clause) that, tested by a certain process, the creosote should yield a total of 8 per cent. of tar acids. He was aware that in an earlier specification drawn up by Sir Frederick Abel and himself about three years ago, they specified 10 per cent., and of course it was only fair to ask him why he had thus degenerated. Having examined a very large number of creosoted timbers that had been prepared for at least a year, he was unable to detect the slightest trace of carbolic acid in them. This fact had also been very prominently and excellently well brought out by Mr. Greville Williams. But although after a short period there was no trace, so far as he could make out, of carbolic acid in the sleeper, yet the wood continued as sound as ever. It was also a fact that the earlier timbers were creosoted with heavy oils containing only a small quantity of carbolic acid, nevertheless these very sleepers laid the foundation of the success of creosoting at a process. Taking those two facts together, it appeared to him that they had hitherto placed an exaggerated value upon the carbolic acid. He did not wish to be misunderstood. He did not say that the carbolic acid evaporated, nor that it might not undergo certain chemical changes in the wood; he did not know what took place, and that was not the place to discuss the question. It was on the ground, however, he had mentioned, that he had fixed the quantity of the carbolic acid as low as was consistent with obtaining a genuine creosote. In other words, he fixed 8 per cent., not because he regarded 8 per cent. as necessary for the purpose of creosoting, but because he thought from a large number of analyses of London creosote, that by fixing 8 per cent. he should ensure the obtaining a genuine creosote. In the other part of his specification he admitted that he had completely altered previous specifications, namely, in requiring that the creosote should contain at least 25 per cent. of constituents that did not distil over a temperature of 600°. He believed that up to that time almost every specification had required that the oil should contain at least 75 per cent. of matters that did distil over 600°. He entirely agreed with the author that it was to the heavier oils that the success of the creosoting process was due, and it was therefore by the amount of those oils that did not distil over at a temperature of 600° that the excellence of the creosote to be used for creosoting purposes should be determined. It appeared to him to be highly advisable to get the heaviest creosotes for the work, and to insist upon as great a quantity as possible of the creosote being driven into the wood.

Dr. H. E. Armstrong said that, on the whole, he concurred in the views expressed by the author. He thought that creosoting, instead of being an operation of a three-fold character, as Dr. Tidy had stated, was of a one-fold nature. Water had to be excluded, because in excluding water everything was excluded which was likely to be harmful. When water was introduced, other things were introduced with it, especially certain organisms which there could be no doubt played a most important part in effecting the rapid decay of timber. He agreed with Dr. Tidy that, mechanically, it was of great importance to choke up the pores, but the object was not so much to choke up the pores as to prevent the perpetual moistening of the wood. When wood was moistened, and was subject to frequent variations in pressure, it necessarily became after a time reduced to a very spongy condition mechanically, and its quality was in that way materially affected. If, therefore, the access of moisture could be prevented an important point was gained. The author had briefly referred to Pasteur's experiments, which perhaps were not so well known as they deserved to be. He supposed that the experiment to which special reference was made was that conducted with sawdust. M. Pasteur had shown that if ordinary moist sawdust had air passed over it for a few hours, there was obvious evidence of decay afforded by the production of carbonic acid. But if precautions were taken to kill all the organisms attached to the sawdust by heating it, and if it was then

moistened with water deprived of organisms, and exposed to a current of air carefully deprived of organisms by filtering through cotton wool, the current of air would pass over it for hours without there being any evidence of the decay of the wood. That was the fundamental experiment upon which the views of chemists with regard to the part played by organisms were based at the present day. With reference to the author's remark as to the difference between creosote, properly so-called, and coal-tar oils, he thought there was a little misunderstanding on that point. The author stated, "Creosote, correctly so-called, is the product of the destructive distillation of wood, and coal-tar does not contain any of the true Creosote, which has never been used for timber preserving." That was not quite correct, because true creosote contained a considerable quantity of carbolic acid and cresylic acid, which had been commonly regarded as active constituents of ordinary creosote oil. With reference to what was really the active constituent in creosote oils, the remarks of Dr. Tidy met with his approval to a large extent, but he should be inclined to predict that before many years Dr. Tidy would drop his limit from 8 per cent. to 6 per cent., and perhaps eventually sink it altogether. It was very much to be hoped that that would be the case, because he thought that engineers were using a material for creosoting that ought not to be employed for that purpose, and probably the carbolic acid was practically of very little use. He did not think that the coagulation of the albumenoids to which reference had been made, took place to any large extent, or was an essential part of the process. That was, he thought, the only part that could be assigned to the carbolic acid. There could be very little doubt that, within a comparatively short time, either by evaporation or by being dissolved out, the carbolic acid disappeared. It was not there in any form, but actually went away. There was no probability that it would be fixed in such a way as to escape attention and detection by the tests employed by Dr. Tidy and Mr. G. Williams. He was inclined to think the action was mainly a choking action as described by Dr. Tidy, the access of water to the wood being prevented. It was therefore simply a question of obtaining an oil which would do that in the best possible way, which could be introduced into the wood with the greatest readiness, and would remain in it under ordinary conditions, for the greatest length of time; and if, as the author had said, with the oil which would exercise that action engineers could introduce substances like acridine and other compounds of a poisonous character, so much the better.

Professor A. Voelcker remarked that, as had been pointed out by the author, the antiseptic treatment of timber had almost entirely superseded former methods, and justly so, for on the strength of past experience there could be no question that, when properly carried out, the impregnation of timber with crude creosote was the most efficacious, the least troublesome, the most persistent, and the cheapest process that could be adopted. He gathered from the paper that the author was rather inclined to think that chemists had attached too much importance to the presence of carbolic acid in creosote oil. He had pointed out that certain alkaloids in coal-tar possessed antiseptic properties, even in a higher degree than phenol, and had suggested whether it would not be desirable to modify somewhat the specifications issued by the Crown Agents for the colonies, by the War Office, and other public bodies. Professor Voelcker agreed that the heavy tar-oils were extremely useful, and perhaps more so than the light tar-oils, for preserving timber intended to be used for railway sleepers. He could not go so far as Dr. Tidy, who had suggested (he granted somewhat vaguely) that chemists had attached far too great importance to the presence of carbolic acid in crude creosote. But he would go as far as to say that for preserving well-seasoned old timber, it did not, perhaps, matter so much whether there was a high percentage of carbolic acid in the creosote, as it mattered that there should be present in it a high percentage of the oils which passed over on distillation above 610°, because, as Dr. Tidy had pointed out, the effect of these tar compounds was to close up the pores of the timber, to render it impervious to water, air, and other debasing influences; being at the same time in itself, comparatively speaking, an imperishable substance, like all pitchy products when completely

dried. But it should be borne in mind that it was requisite not only to preserve well-seasoned old timber, from which the moisture was expelled almost completely, but that of late years a great deal had been done in preserving telegraph-posts, gate-posts, wooden fencing, hop-poles, and the like, for which sapling-wood, or at any rate young wood, was used. There was a great deal of difference in the chemical constitution of the two kinds of wood. Sappling-wood was more or less filled with sap, and in the liquid which circulated in it there were perishable substances belonging to the class of albumenoids, which acted as ferments, and caused otherwise imperishable substances to decay. The primary causes of decay of green wood were unquestionably the albumenoid substances; and all the older processes, such as the corrosive sublimate, or kyanizing plan, or the impregnation with other metallic salts, were based on the principle that by those metallic salts, notably by the bichloride of mercury (corrosive sublimate), the albumenoids were precipitated, and rendered insoluble and incapable of acting as ferments. In green wood also, the celluloise was in a more tender condition than in old wood, where there was a larger proportion of incrusting matter; there was, therefore, a greater reason for preventing the first state of decomposition; and he questioned whether creosote, which was sometimes extremely poor in carbolic acid, was the proper material for preserving wooden structures of the kind he had mentioned. No doubt there was a good deal to be said in extenuation of the qualities of creosote, for the process of preserving wood in open tanks, was sometimes unskillfully—not to say carelessly—conducted; but, making all allowance for the imperfections of the methods for preserving wooden poles in that way, there could be no doubt that sometimes creosote answered remarkably well, and in other instances the same process tended to make the wood more perishable than it would have been had it not been creosoted at all. That seemed to be a contradiction, but according to the evidence of those who had carried on the process with more or less success for fifteen years or longer, the same kind of creosote would answer extremely well for preserving hard timber used for railway sleepers, while for young wood it did not answer the purpose. He had found, from the examination of samples which had been sent to him, that there were great differences in the composition of different creosotes. Not long ago he had received a sample which yielded, on distillation from the boiling point up to 610°, only 39 per cent. of distillate, containing no more than 3 per cent. of carbolic acid; while another sample yielded 14¾ per cent. of a watery liquid with a little light oil, the water being strongly ammoniacal; and it was well known that any ammoniacal water left in the creosote was extremely injurious to the timber. The same sample only yielded 47½ per cent. of distillate; including 4¼ per cent. of crude carbolic acid. In a third sample he found only 5 per cent. of carbolic acid. He ventured to think that creosote containing as little as 5 per cent. of crude carbolic acid was not a good liquid for preserving immature wood, simply because it was not strong enough to precipitate or render ineffective albumenoid substances. Even Dr. Tidy, in the recent modifications of his views, still recommended that the creosoting liquid should contain as much as 8 per cent. of crude carbolic acid. A great deal had been said about the specific gravity of the creosote. He confessed that he did not attach very great importance to specific gravity, but he did attach great importance to the presence of a fair percentage of phenol, or crude carbolic acid, if it was wished to preserve green wood, such as that used for telegraph posts or hop-poles; because it was essential that the first tendency to subsequent decay should be counteracted, and that could not be done without the introduction of a sufficient quantity of carbolic acid. In the case of hard timber the main object was to fill up the pores. There was not so much albumenoid matter present, and the timber would keep fairly well if moisture was excluded; that was effected by heavy tar oil which filled up the pores, and rendered the wood very hard, so that there was not the same necessity for the presence of carbolic acid. He could even understand that if crude creosote contained a very small quantity of carbolic acid, as in the case he had mentioned, and if there should be a large proportion of the more solid constituents of creosote, the pores externally at any

rate would be closed up, and the same thing would take place by painting or pitching unseasoned wood; the solid constituents closed up the pores of the outer layers, introducing nothing to render the albumenoids ineffective as a ferment, so that the moisture was kept in, and in that way decay was actually hastened, whereas if free passage were allowed, the wood would be washed out and would keep longer. Engineers knew from experience that green wood, when thoroughly painted or pitched, decayed more rapidly than wood in its natural state. In order that the point might be settled he would suggest that some experiments be tried with creosote containing various proportions of carbolic acid, not with reference to the preservation of railway sleepers, but of younger wood. Strictly comparative experiments should be made with crude creosote, one sample containing 5 per cent., another 10 per cent., and a third 15 per cent., that being the usual range of carbolic acid in commercial creosote. Some well-conducted experiments in open tanks with creosote of various strengths would, he thought, finally settle the question. He could not help thinking that it would be requisite for the proper carrying out of the process that there should be something like 10 per cent. of crude carbolic acid in the creosoting liquid; at any rate, without further information on the subject, he should be disinclined to recommend anybody wishing to preserve hop-poles or telegraph posts to use any liquid containing a less amount than he had mentioned.

Mr. H. K. Bamber said it appeared to him that the whole secret of the paper was to be found in the paragraph, "By the light of the evidence now accumulated, it may be advisable to review the question as to the relative value of these various bodies contained in the heavy oils as regards the preservation of timber. Some of them are becoming valuable for other purposes. Which of them should the engineer retain for injecting wood?" Carbolic acid, if left in creosote, was worth 2d. a gallon, and if taken out from 4s. to 40s. per gallon. according to the state of purity. The author's idea appeared to be that nothing should be left in the creosote which it would pay him better to take out; in fact, there should be nothing left that was worth more than the proverbial 2d. He did not appear to see the difference in our color between the two pennies and the two sovereigns. The difference in the town-made tar and the country tar did not arise so much from the difference of coal used as from the degree of heat used in making the gas. In London it was desired to obtain a harder coke that would do for engines, and for that purpose a much higher temperature was used, and the most luminous portion of the gas first formed was decomposed on coming into contact with the sides of the red-hot retort, the result being gas charcoal, naphthaline, and a gas of less illuminating power. With regard to Dr. Letheby's specification, that very specification was advocated and recommended by Messrs. Burt and Boulton, but then carbolic acid had not become so valuable when separated. Now the specification recommended was drawn up by Dr. Tidy, and if the author could get his views adopted there would soon be a very tidy specification to work from. The author had mentioned some experiments of Dr. Tidy's with naphthaline injected into wood, but he had given no facts or data, merely expressing his own opinion. Again, the experiment was not similar to the exposure of creosoted sleepers, which were not subjected to a temperature of 150° Fahrenheit in a closed vessel. He would give the results of two experiments that he had made in 1882, one with country oil, condemned by the author, and one with Mr. Boulton's own oil, full of naphthaline. He took a piece of wood (deal), 3 inches by 3 inches by 8 inches, and dried at 230° Fahrenheit, until it lost no more weight, so that there was no water left in it to cause loss. He then kept it in a vessel containing the author's London creosote, heated to 180° Fahrenheit, having a weight on the wood to keep it under the creosote. It took up 1,020 grains, after being wiped from excess of creosote outside. It was then, on February 7th, 1882, placed on a mantelshelf, where the temperature was never above 70° Fahrenheit, and generally between 40° and 50°. It was repeatedly weighed, and the loss was constant until June 5th, 1882, when it had lost 487 grains, equal to 47.75 per cent. of the creosote put in. Now, as there were only 10 per cent. of crude tar acids in the creosote, what was it that made up the 47.75 per cent. loss?

Water there was none. The loss arose chiefly from evaporation of the naphthaline. At the same time he treated a similar piece of wood, 3 inches by 3 inches by 6 inches—2 inches shorter—dried at the same temperature, until it ceased to lose weight. It was then immersed in country oil, specific gravity 1,045, and kept under the oil by a weight, but without applying heat. It absorbed 1,788 grains of the creosote, so that the wood, which was a quarter smaller, took up, actually, in cold, more than double the quantity that the other piece did of the author's oil at 180° Fahrenheit. The piece was placed side by side with the other on the mantelshelf, and in four months it had lost 575 grains, or 42.33 per cent. of the oil taken up. But that was not fair to the second piece of wood, for it was so saturated that some of the oil drained out on the mantelshelf, and of course contributed to the loss of weight, although it was not by evaporation. The oil contained about 20 per cent. crude tar acids. Those were plain facts, and showed that the author's contention "that country oils are not good for creosoting timber because of their instability" was contrary to fact. The beautiful white substance, naphthaline, was liable to sudden changes. It might at one moment be a black dirty-looking substance, and, by the application of a moderate heat, it became volatilized and condensed into a lustrous substance. The author, by Dr. Tidy's experiments, had tried to make out that it was not volatile. Camphor, although it could not be volatilized by heat without decomposition, yet it was well known that a piece of camphor, even when wrapped in paper or any porous material, would soon pass away by evaporation; and it was so with the naphthaline. Many attempts had been made to prepare tar colors, &c., from naphthaline, but as yet without success; it was worth nothing (except in small quantities in the albo-carbon gas-burners) when separated from the creosote; and that was the reason why it was so valuable, according to the author, in the creosote. But if ever, by chemical research, naphthaline became as valuable as carbolic acid, it would then become so volatile as to escape from the creosote altogether, and chemists would be asked to reconsider their creosoting specifications. As to the solubility of carbolic acid in water and alkaline solutions, which the author said was a disadvantage, he maintained that it was an advantage, for it enabled the acid gradually to dissolve in the water and sap, and thus get into the substance of the wood and prevent decay, while the other portion of the creosote remained like beauty, only skin-deep. To say that because carbolic acid could not be found in creosoted sleepers after twenty or thirty years, and that therefore it had nothing to do with stopping decay, was absurd. It might as well be said that a few days after a large fire only one or two policemen were found and no fire-engines, and that therefore the policemen put out the fire and not the engines. Carbolic acid was an oxidizable substance, and would protect the wood from oxidation or decay. It instantly prevented decomposition, and destroyed the life of the germs which caused decay, being also poisonous to most insects. Dr. Tidy had mentioned the number of analyses he had made, and how long his experience had lasted. Mr. Bamber might therefore be allowed to state that he had tested samples of creosote for the last twenty-five years, and had practical experience in the process of creosoting. It was his opinion that to creosote timber properly the creosote tank must not be only the "waste tub" for distilling works. It was easy to get good country oil with 18 to 25 per cent. crude tar acids, yielding no deposit of that volatile substance, naphthaline. He had met with some samples of so-called creosote that contained nearly half their bulk of filth, consisting of charred oil, &c., he presumed the residues from anthracene manufacture, yet when the creosote was rejected every effort was made to induce the belief that it was some of the best creosote.

Dr. Albert J. Berneys said that no one could doubt the conclusion that the substances preferred should be germ excluders as well as germicides, and those contained in the oils which were heavier than water. His contention would be to retain, at least in part, and to the extent of 2 per cent., the carbolic acid as well as the naphthaline and the alkaloids. The arguments in favor of carbolic acid were very strong. Where the creosoted

timber was covered up in the ground the solubility in water assisted in diffusing it somewhat in the earth, and thus extended its sphere of action. Nor should that solubility be exaggerated. In a dry soil the loss could only be by heat, and that would also affect other ingredients. It said much for the durability of carbolic acid that, in spite of the employment of heavier types of tar-creosote in early days, it was distinctly present in many cases recorded in the experiments of Mr. Greville Williams. In one specimen wood creosoted thirty years, distilled with water, a distinct reaction of phenol was obtained in a case where most of the oil and all the naphthaline had disappeared. In another, creosoted thirty-two years, the phenol reaction was very distinct. In a third, creosoted twenty-nine years, the phenol reaction was very strong when distilled with acid, but was also distinctly present in a free state; whereas there was no naphthaline and very little oil. In cases where no free phenol was found, it was discoverable in combination. The power possessed by phenol for coagulating albumen could not be exaggerated. He would not describe his own experiments; but in his hospital work he was very familiar with the high antiseptic power of carbolic acid. The experiments of Mr. Greville Williams with the white and yolk of an egg only showed that the alkaloids of the tar-creosote, weight for weight, were equal to the carbolic acid as germicides, but certainly no more; and that the $\frac{1}{1000}$th part of phenol bore no relation to the amount of albumen present. If all the albumen had been coagulated it would not have putrefied. For Mr. Williams further stated that 1 per cent. of phenol and 1 per cent. of alkaloidals were of equal value. He believed in the coagulation theory by phenol of the albumen of the wood, but unless enough was used it was as with disinfectants. If he had a quantify of hydrogen sulphide in the air of a room, and he only used enough chlorine to unite with one-half of the hydrogen present, where would the disinfection be? It was the worst of disinfectants that generally they could not be used in sufficient quantities. The benefit of them was (as Miss Nightingale had said) that it was necessary to open the window when they were used. It was the same with phenol. If he did not coagulate the albumen, of which there was but little in the wood, he failed. But the phenol had the property and the additional advantage of volatility. It took a long time for even the free phenol to evaporate, so much was it protected and shut up by the oil and naphthaline in the tar-creosote. And he believed that not only was carbolic acid more potent as an antiseptic than any other constituent of the tar-creosote, but that it was present in larger quantities than the alkaloids which, according to Mr. Williams, were equal to it weight for weight.

Mr. W. Foster regarded the question brought forward by the author, as to the value of alkaloidal substances, as a very important one. In a paper which he had recently read before the Institution, he had remarked on the possibility of some of the nitrogen, which he was then in search of, being in tar in the condition of alkalodial bodies. The author had mentioned five or six of those nitrogenous bodies, and there were probably others. The recent investigations of chemists had shown that pitch itself contained an appreciable amount of nitrogen. Acridine, of which an example had been given in the table, contained the lowest percentage of nitrogen, and had the highest boiling-point. Having regard to the pitch, it was possible that there were other nitrogenous bodies which had a still higher boiling-point, and a lower percentage of nitrogen than in the case of acridine. The quantity of those alkaloidal substances in the tar was very small. There was no information as to their relative proportions; and as the percentage of nitrogen varied from 17.7 to 7.8, it would not be wise to specify how much of those bodies was present in the tar. He thought he might say that there was not more than from 3 to 4 per cent. If, therefore, they had any value in the preservation of wood, their effect must be very powerful. He was inclined to look at the question of the preservation of wood by the aid of some facts which were a little outside the subject. He might be pardoned for referring to the corrosion of iron. Iron could remain permanent in dry oxygen, in pure water, or in pure carbonic acid gas; in any two of those it remained permanent; it was

only by the conjoint influence of the three that corrosion was effected. Pitch, he believed, was the best preservative of iron that was to be had, and if applied to a clean surface free from oxide (rust), it was impossible to say when the surface of pitch would fail to protect the iron. He was of course speaking of the continuity of surface being preserved. Pitch was a substance of a most permanent character, being almost destitute of any chemical attributes; if, therefore, the cellular structure of the wood could be thoroughly permeated by it, as long as the continuity was perfect, it would be preserved. Of course, that could never be fully realized in practice. In the case of green wood, the question arose as to the coagulation of albuminous matter. No need to go far afield to get plenty of instances showing that if water and impure air could be kept out, the preservation would be prolonged. The author had referred to the experiments of Pettigrew; but the inference he had drawn was not the only one. If albuminous matter was dried, it could be kept as a horny substance for an indefinite length of time. A piece of glue could be preserved intact in the same way. If white of egg or glue were moistened and exposed for a certain time, it putrefied. The inferences deducible from Pettigrew's experiments could, he thought, be traced to the removal of water from the muscle (the heart), which had been the subject of the experiments. The whole thing might be summed up in the gravedigger's reply to Hamlet, "Water is a sore destroyer of the dead body."

Mr. W. Carruthers thought that the botanical aspect of the question should be at the basis of the inquiry; for without a proper appreciation of the circumstances under which vegetable tissues were destroyed, it was impossible rightly to appreciate the means by which that destruction could be prevented. While he agreed with much that had been said, he felt bound to differ from a great deal that he had heard. He acknowledged the great importance of pitch for preserving the external surface of wood. But wood decayed not only from chemical agents, air and water; but much more from the action of parasites. He could easily see that if a body was entirely protected externally by pitch, it would be preserved from chemical changes, but not from the more injurious and dangerous attacks of fungal parasites. They were developed from spores, and the attack might be made through a flaw or crack in the wood. When the wood was exposed to the desiccation of the air, flaws continually appeared, and wherever a spore could get access, there would begin development of the mycelium or root of the fungus, which penetrated the wood wherever nutritious materials were supplied through its whole course; so that unless the wood was preserved by some substance which would prevent the life of fungi, its destruction was certain. He exhibited a specimen of wood the date of the creosoting of which was not known, but it had been used in a hurdle for at least ten years. The lower creosoted portion, embedded in the earth, did not show the slightest injury; but the upper part, exposed to the air, and cracked, had been attacked from the outside by minute vegetation. Some of the spores had obtained access to the interior, which had not been antiseptically preserved, the fungi had enormously developed, and the interior had been destroyed by them. The same thing had occurred in the case of two specimens of telegraph poles. The exterior of the specimen which he exhibited had been fairly preserved, but the interior had been destroyed. It was remarkable that the interior was colored by the injection of what he supposed he must call creosote; but it had not been sufficient to serve as an antiseptic, as it had permitted the free growth of fungi, which ramified through the base of the pole and completely destroyed the cellulose or lignine, leaving it a fragile skeleton. It appeared to him that what was needed was a sufficient impregnation of the wood with creosote, and with that element in it which was destructive to vegetable life. He did not know from experiments what that element was, but he did know that there was an element in crude creosote that was extremely destructive to vegetable life, viz., carbolic acid. Not, however, in all strengths, for Koch, a distinguished German mycologist, had found that certain liquids, with 5 per cent. of carbolic acid, would support fungi; so that the presence of a small percentage was not destructive to vegetable life. That was extremely important in relation to the ob-

servations of Prof. Voelcker. Another specimen from a telegraph pole had been completely destroyed by a fungus (*Reticularia*). There was on one side a yellowish dust, consisting entirely of the spores of the plant. But in a specimen from a hurdle, which had been in use since 1861, when it was creosoted, the exterior, although it had no coating of tar, still exhibited the minutest marks of the tools employed upon it, and the interior, which was completely saturated with a brown substance, was as good and fresh as if it had been creosoted yesterday, without a particle of fungus. There was a little greenish vegetation on the outside, but it was epiphytic, and not injurious to the wood; there was no fungal vegetation whatever. The wood had been enormously increased in weight, and he had ascertained microscopically that there was no deposit in the interior of the cells. The whole of the lignine and of the secondary deposits had been colored by that material, so that the tissue had been completely altered. It appeared to him that there had been a new combination through the injected material, producing an antiseptic condition of the wood which was fatal to the fungi. There was a little free carbolic acid crystallizing in the interior of the cells, but it did not seem to him that that was the explanation of it. He should be glad if those who were conversant with the chemical aspect of the subject, would inquire into the real nature of the change which had produced the discolored and altered condition of the lignine. In his opinion nothing had been introduced for preserving timber that could compare with the creosote used in the specimen he had exhibited, which had been exposed to the air nearly twenty years, and yet the ragged edges of the chips on the outside had not even been touched by atmospheric or other destructive agents.

Mr. Henry Maudslay observed that, in the case of Old London Bridge, the decay of the timber piles of the piers varied according as they were constantly under water, or exposed to water, air, and sun; or exposed especially to salt water or to fresh water on the rise and fall of the tide. There were many combinations of circumstances that tended practically to destroy timber, and it was therefore most desirable to ascertain the exact position that would be occupied by a solid pile driven into the earth to support a structure—whether it was to be exposed to the constant action of the water below in the earth, or to a change in the rise and fall of the tide, or to the influence of moisture gradually attacking it above the highest springtide level. On the Arran and Snowdon mountains he had been lately excavating soil in order to form a reservoir, and had come across some of the largest roots of red pine timber that he had seen in that locality. There were no trees on the mountains at the present time, and it must have taken many years for the timber to have grown at that elevation—1,200 to 1,500 feet above the level of the sea. The timber was of a magnificent character; these roots had been submerged perhaps centuries. The roots had been found *in situ* covered with a layer of disintegrated earth saturated with water from the copper mines. They had been preserved in that way by nature, but now that they were being exposed to the air, they were in some cases beginnin to crumble away. The props and supports in old workings of copper mines were preserved, and would burn with great difficulty. Since the Royal George had sunk in 1782, all the timber had become saturated with sea-water, which was so destructive to the cast-iron cannon, that they were made as soft as plumbago; but salt water had a great effect upon the preservation of the oak wood, making it quite green. The timber was so hardened that all the pores seemed to have been filled witn some material that was suitable to its preservation. It still retained that quality, as shown in the case of a billiard table made for Her Majesty, and by another now in his late father's house at Norwood. This table had been made by Thurston in 1860, from the wreck which was raised in 1841. With regard to the decay of iron. he might be permitted to mention that Queen Anne's statue at St. Paul's cathedral, was one of the finest of London specimens of decay of iron that engineers could examine. It consisted of cast iron, wrought iron, lead, and stone, all of which were mouldering away by the action of nature, the character of the air, and the water. The whole of the ironwork was a magnificent specimen of age and deterioration. If chemists would examine the question as to effects produced

upon timber subjected to the continual action of water and its components, or to the rise and fall of the tide, whether salt or fresh, or only to the effects of a certain amount of moisture, as in the case of railway sleepers afterwards dried by the action of the sun, the practical results of their investigations would be of great value.

Mr. E. A. Cowper said he understood that an examination of the old pieces of timber successfully creosoted that had been exhibited, showed they were not at present protected by tar acids, and if they had had any in them in the first instance, it had long ago evaporated. The unsuccessful telegraph pole exhibited by Mr. Carruthers, from which a specimen had been taken, had evidently not been put into a creosoting cylinder, for it had a mere slight covering of creosote outside. Hop-poles were often put into an iron pan with a fire under it and made hot, and there could be no doubt that steam came out from the water evaporating, and the very action of which the author had spoken took place to a slight extent. The piece of wood that was cut to a taper had a little creosote in its end, but on its sides the creosote did not go in $\frac{1}{16}$ of an inch, it was merely paint on the outside; where the mortise-holes had been put through the post the spores had entered and attacked the inside. The effect of a spore getting into a piece of timber that had been preserved only on the outside surface was no argument against the preservation of timber by creosote. The piece from the Victoria Dock fence, which had been well creosoted, had been preserved, and was as sound as it was twenty-nine years ago, when it was put down. The creosote had gone to the middle of the wood and protected it. The other specimen had not been preserved, and, therefore, it was rotten. A very extensive series of experiments had been carried out by Mr. Charles Coisne, and they were of a very instructive character. Samples of creosote had been taken from England, Scotland, Belgium and France, showing 15, 15, 8 and 7 per cent. of tar acids, and there was a fifth specimen of heavy oil without any tar acid. Other mixtures were made by putting in an extra quantity of tar acids, except in the case of the one kept without acid, and the result showed that where the heavy oil was used the wood was preserved in the best manner, whilst those samples of wood preserved with creosote, having an extra dose of acid, were not so well preserved, and that which was unpreserved was entirely rotten. He had gone to Silvertown to examine the apparatus to which reference had been made. There were a number of pipes in the bottom of the creosote cylinder with superheated steam in them. When the timber had been put into the cylinder and warm creosote run in upon it, the temperature was gradually got up, and the water was as effectually driven out of the wood by evaporation as would be the case if water was put in a boiler with a fire under it and kept without any fresh supply of water. A temperature of 220° or 230° would evaporate every particle of moisture out of the wood, more especially when a vacuum was put on. He might mention that the vacuum should not be turned on suddenly, otherwise the creosote, steam, and water would all boil over. Water was deposited in a vessel in connection with the condensing pipe, together with some light hydro-carbons. The creosote supplied to the creosoting vessel being heavy oil, would not commence to boil until about 392°. London creosotes contained about 4 to 7 per cent. of tar acids. He had himself tried some experiments in coagulating and precipitating albumen, and he found that considerably less than 2 per cent. of carbolic acid in the creosote would precipitate the largest amount of albumen found in wood, so that there was amply sufficient carbolic acid in the London creosote for that purpose. Not only was the albumen coagulated by the two per cent. of carbolic acid, but by the mere fact of its being boiled. If an egg was boiled for a short time the white would set, and in an hour or two it would be very hard. After the vacuum had been on for a sufficient time, and the whole of the water and moisture withdrawn from the timber, the cock was turned, and the pressure put on with pumps up to 120 lbs. to the square inch. Not only did the pumps put on the pressure and force the creosote into the wood, but directly the temperature was lowered a little, steam condensed, and there was a vacuum in every pore of the wood. The whole of the

wood was made a condenser; in every pore that had previously contained water there was a vacuum, so that the creosote went in, and, besides that, there was the pressure of 120 lbs. to the square inch. At the works he saw a whole range of tanks, following one after the other. He thought the method was a very practical and mechanical one. There could be no doubt about the creosote thoroughly entering the timber. He thought the thanks of the members were due to the author for the admirable way in which he had developed the subject. The only thing wanted was a sort of skeleton specification for their guidance in the future.

Mr. W. H. Preece said that as the behavior of certain of Her Majesty's telegraph posts had been called in question, he ought to say something in their behalf. For the past thirty years he had devoted all the attention and skill that he could command to the inquiry as to the best modes of preserving timber. In the telegraph service of the country many millions of poles had been preserved in various ways, and one of the methods—that explained by the author—had proved to be the survival of the fittest. A great deal had been said as to the various causes of decay. Reference had been made to chemical and physiological causes, but there was a third cause, which might be called mechanical, of the decay existing at the "wind and water" line, or the ground line, where the timber was exposed to incessant changes of moisture and temperature. A careful microscopic examination showed that the process of decay was a purely mechanical one, that the wood disintegrated by a process of bursting. The fibers appeared to be minute boilers, and the change of temperature produced evaporation, minute explosion, and rapid deterioration. It was a simple thing to meet the chemical cause by the insertion of salts of various kinds, and it was possible to meet the physiological cause by antiseptic treatment; but the mechanical cause could only be obviated by coating the fibers of the wood with waterproof material, and filling them with a thick, viscous mass like creosote in its best form. In 1844 the first line of telegraph was constructed between London, Southampton and Gosport, and the posts were made of the best Memel timber, preserved by the burnettizing process, simply impregnating the wood with zinc chloride. In 1857 he made a personal observation of a great part of the line in different grounds, and found that in sand about 40 per cent. of the posts had gone, in clay about 33 per cent., and in chalk about 28 per cent. In 1860 he found that the proportion was much greater, and in 1871 they had all failed, so that they had to be removed. The burnettizing process materially added to the life of the pole without rendering it indestructible. Kyanizing was tried to a small extent, but the poisonous character of the salt deterred him from carrying it further. The favorite process about twenty years ago was that of boucherizing. The authorities had purchased whole forests, and in the middle of them established the boucherizing process, by which they had succeeded in lengthening the life of timber considerably. While the life of an average telegraph pole unprepared was about seven years, the life of a boucherized pole was about fifteen years. In 1848 a line of poles was erected from Fareham to Portsmouth, a distance of about 20 miles, and all the poles, three hundred and eighteen in number, were creosoted by Mr. Bethell. It 1861 he examined them all *in situ*, and only two showed the slightest trace of decay, and they had begun to decay at the top. In 1874 he had them again examined, and every pole was sound. Last year, owing to the requirements of the service, and the necessity of increasing the number of the wires, the line of poles had to be taken down, and although they had been put up in 1848, they were as sound as when they were first erected. About the year 1861 the question of the proper mode of preserving timber was one of great consequence. The authorities were not satisfied as to which was the best, boucherizing or creosoting, and consequently, as the Yeovil and Exeter line of the London and South-Western Railway Company the poles were put up alternately: first a plain pole, next a boucherized pole, and next a creosoted pole, the line extending about 40 miles. In 1870 he had them carefully examined, and it was found that of the plain poles that had been up ten years not one existed, all

having decayed; while of the boucherized poles 30 per cent. had gone, and of the creosoted poles not one had decayed. The result was that the Government had decided for years past to creosote all their poles. He did not remember the exact specification thut was used. At present the millions of poles existing in the country were all creosoted. It was true that some of them had failed, but, as Mr. Carruthers had pointed out, there was creasote and creosote. There were unreliable firms, and others in whom confidence could be placed; there were inspectors who could be trusted, and others who could not. There were poles about the country supposed to be creosoted that were rotten; and it had been found that those particular poles had not been inspected, and that they had been hastily and improperly impregnated. He could state, as the result of thirty years' experience, that he had never seen a case of a properly creosoted pole showing the slightest sign of decay.

The reply of Mr. Boulton upon the Discussion and Correspondence is given at the end of the Correspondence.

CORRESPONDENCE.

Mr. A. Bouissou, of the Western Railways of France, stated that in 1859, on the line from Rouen to Dieppe, sleepers creosoted by the Bethell process had been adopted for the first time. These sleepers were of beech. They had been creosoted in England in the works of the author's firm, and when an examination of them was made twenty years later, on the occasion of the Paris Exhibition of 1878, it was shown that not a single one of them bore the slightest trace of decay. Since 1864, the railway company of which he was engineer of the permanent way, had adopted creosoting for their sleepers, and from that date they had applied it to about five million sleepers, of which at least three million and a-half were of beech wood. In these latter, as in the trial sleepers of 1859, no sign of decay has as yet been distinguished, and the lasting powers of the sleepers seemed only to be limited by the wear and tear to which the materials were exposed. Beech wood placed in the ground, without having been prepared, completely decayed at the end of two or three years, which rendered impossible the use of that wood unprepared in the form of sleepers. Also sulphating, employed for a long time for beech sleepers, not having given the good results expected, had been abandoned by all the French railway companies.

The employment of creosote for the preservation of sleepers had given every satisfaction, and its use had only been limited at certain periods by the difficulty sometimes experienced in procuring a sufficient quantity of creosote. As regarded the quality of the creosote, he simply required that it should contain 5 per cent. of phenic acid.

Mr. W. A. Brown remarked that a preserving process, of which much had been said and a great deal expected by engineers a few years ago, had been referred to in the latter part of that portion of the paper devoted to "Apparatus for Timber Preserving." This process was Mr. Blyth's system of "Thermo-Carbolization," which had been carried out by Messrs. Conner & Co. at their works at Millwall, when a large number of sleepers had been prepared for some of our railways, together with telegraph poles for them and for the Post Office. It became his duty, about four years ago, to inquire into the subject, and he made an investigation into the different stages of the process at Messrs. Conner & Co.'s works, which led him to the following conclusions:

1st, that the strength of the wood was impaired through some of the celluloise and its incrusting materials being carried off in the form of pyroligneous acid by the superheated steam.

2d, that the peculiar "Creosote mixture" used as part of the process, contained so large a proportion of water that it was not at all likely to act as a preservative of the sleepers to which it was applied.

It would be interesting to hear now how the sleepers and poles thus prepared had actually lasted in this country. In Austria the experience of Mr. Seidl, and in France that of the author, as recorded in the paper, appeared to confirm the conclusions at which Mr. Brown arrived in the course of his investigations; but so far as he was aware, there were no published results as to the process in England.

Mr. John Cleminson observed that the

question of preparing timber against decay was occupying more attention now than formerly. It was therefore to be regretted that the author had not referred in detail to many good processes with the above object in view, namely, that of Sir John MacNeill, Gardner, Beer, Blythe, and others. The author's remarks in reference to carbolic acid as an antiseptic would lead to the idea that it was necessary the acid should remain when injected; such was not the case, nor was it necessary. The mere fact of its presence (the most powerful antiseptic known), with superheated steam, was all that was required to produce coagulation of the albumen, and so to render preservation practically complete. With the old process of creosoting, the surface exteriorly only was preserved, the interior if unsound decayed uninterruptedly. All depended upon the selection of the timber. No amount of creosote would avail to save its destruction ultimately, if the interior was not sound. Where sleepers were adzed, the greater part, and in many instances the whole of the part, penetrated by the creosote was cut away, thus leaving the interior open to destruction from damp and other causes. The same disadvantage was experienced in the case of piles, when the ends were pointed for receiving shoes after creosoting. With carbolic acid once in contact with the albumen, and in the event of any interior unsoundness, the coagulation arrested decay, and prevented it from spreading, by entirely enclosing the defective part or parts. Combined when necessary with an outer application of creosote, thorough soundness and preservation internally and externally were thus secured. Blythe's process was a double process. The object, preservation internally and externally, in the case of sleepers and piles, was most effectually obtained by carbolizing the interior, and creosoting the exterior. A result had been obtained that had placed this process foremost with French engineers for several years, and it was now largely used by them. In England where used it had met with much favor. The author of the paper was employing this process in France.

Mr. Richard Cowper remarked that the value of creosote for preserving timber depended partly on the mechanical effect which it had in excluding from the pores of the wood air and water, and the germs of destruction which they contained, and partly on the power possessed by certain of its constituents of destroying those germs. For the purposes of germ-exclusion, it was generally admitted that the heavier portions of the creosote, from the less degree of solubility and volatility which they possessed, and their property of solidifying at ordinary temperatures, were the more efficacious. As regarded the germ-destroyers, the phenols and the alkaloids alone need be considered. Phenols, namely carbolic, cresylic, and other acid bodies occurring in creosote, had long been known to possess remarkable antiseptic properties, but they were easily soluble in water, and comparatively volatile. Much stress had been laid upon their power of coagulating albumen, but it had been shown that no stable chemical compound was formed, and that the albumen thus coagulated might be freed from the phenol by washing with water, when it would decay. It had been shown by the experiments of Coisne, Greville Williams, and the author of the paper, on pieces of old creosoted timber, that in many well preserved specimens no phenol can be detected by the ordinary test, whilst in most cases they had found naphthaline, and in all cases oils of the heaviest character in considerable quantity. It had been shown by Mr. Greville Williams that all the old timbers examined by him contained a considerable amount of alkaloids, and his experiments proved not only that these alkaloids were powerful germicides, but that they were more powerful than phenol. They were at the same time much less soluble and volatile. Evidently if creosote containing a high percentage of phenol were required, it could not contain so high a percentage of the heavier constituents, which were those possessing the greatest value as germ-excluders. At the same time, some of the alkaloids which had been shown to be of more value than phenol as germicides would be removed.

Mr. W. Langdon remarked that in 1874 a paper by him upon the subject had been read before the Society of Telegraph Engineers, in which he warmly advocated the employment of creosote in preference to any other preservative for timber, and

he had since seen no reason to alter the views expressed on that occasion. Of late years, however, the appearance of the timber so treated had suggested the belief that the oils now employed did not contain that amount of tar or other heavy compounds which was apparently possessed by the creosote supplied in the earlier eays. His attention in the application of creosote to timber had been more in the direction of telegraph poles than otherwise, which class of timber was much more exposed to the weather than were railway sleepers, and which might in consequence be accepted as affording a more complete test of the value of the oil than did railway sleepers. These to a great extent were buried in the soil, and had but one side exposed to the influence of the atmosphere. Of late years numbers of the poles had presented anything but the appearance of a well creosoted pole. The surface had become partially or wholly bleached, and almost white. This generally occurred on that portion of the pole subject to the sun's rays; but it was also equally marked upon that side of the pole exposed to prevailing winds and wet weather. It would therefore seem as if the bleaching was the result both of the influence of the sun and of the weather; in fact that the creosote disappeared from the surface of the pole under the influence of the sun and of wet. If telegraph poles creosoted many years back were examined, as a rule the surface of those poles would be found covered with a pitchy compound, and that mainly on the side of the pole exposed to the sun. There was no washing out from the weather. This he thought was easy of explanation. The warm atmosphere would always exercise an extractive influence upon any oil injected into wood or other like substance; its tendency would be to bring it to the surface, where the lighter portions would be evaporated, and the heavier portions congealed. Creosote no doubt was a strong antiseptic, but where timber when felled was decayed, it could not give fresh life to the decayed portion. Timber, if properly seasoned, would last many years if not exposed to the vicissitudes of wind and weather, as in the instance of many articles of furniture made from the very same wood from which telegraph poles and railway sleepers were obtained, and which seemingly never decayed indoors. It was here that the creosote process enabled an equally long life to be obtained for it when employed out of doors, and he imagined that the heavier oils played a much higher part in procuring this immunity from decay than the creosote oil, inasmuch as it was to these heavier oils that the exclusion of moisture from the timber was due. A telegraph pole, or a railway sleeper, free from disease, if properly seasoned, and encased in such a manner as to prevent moisture getting into its fiber, was practically indestructible from rot or decay. The coating given to it by the injection of these heavier oils into the fiber to a depth of from 1 to 2 inches afforded the timber this coating, excluded moisture, and thereby secured its duration.

Mr. C. De Laune Faunce De Laune remarked that the author had attempted to prove that only a very small quantity of carbolic acid was necessary in creosote for the preservation of wood. He approached the subject with diffidence, as he lay claim to no scientific knowledge, merely discussing it from the purely practical side; and because he had been instrumental in extending the use of creosote among landowners and farmers. The author referred to his having used creosote too hot, and thereby having damaged the wood, much in the same way as if he had taken a warm bath too hot. He certainly stated to the author that he had used a material called creosote which contained a very small percentage of carbolic acid, and that the wood had failed to be satisfactorily impregnated with it in an open tank, even when submitted to a great heat; but he scarcely anticipated that he would infer that it was his general custom to use extreme heat, as he only wished it to be understood that even under such conditions the creosote did not perfectly penetrate into the wood. The process of injection, in the case of telegraph poles, might preserve them to an indefinite period, but such a course was frequently impracticable to the former, and in the case of hop-poles impossible; wherefore an open tank was indispensable. For the last twenty years he had used creosoted wood, and the process had always been performed in an open tank. The wood was first cut to the required shape, and then immersed in the creosote which

had previously been liquefied and warmed by a furnace built underneath the tank. No thermometer had ever been used to regulate the heat, and the only precaution taken was to prevent the creosote from boiling over, though it was sufficiently heated to make a few bubbles appear on the surface. Wood of all kinds had been used, and no difficulty in applying the creosote was at first experienced, but he believed that the creosote had gradually been becoming worse and worse, and so he submitted it to Dr. Voeckler for analysis, and got the following reply: "Your creosote has a specific gravity of 1.103, and on being subjected to distillation yields only 61 per cent. of volatile oils, of which 4 per cent. are carbolic acid." My experience in creosoting timber, small as it is when compared with that of public companies, is large for a private individual, as I have at this time 46½ miles of fences where creosoted wood is used; and whereas the system, when employed some years ago, was satisfactory, the present results are as much the contrary. The pieces of creosoted wood exhibited by Mr. Carruthers were creosoted by me in 1866, and, as was pointed out by him, are perfect in their preservation. Unfortunately I have no analysis of the creosote then used, for such an analysis would prove that a material of the same constituents would be suitable for preserving wood in an open tank. It was obvious, therefore, that a creosote was formerly used that could and did preserve inferior wood in an open tank perfectly, and which could be used so easily that no particular precautions as to the dryness of the wood were necessary, and it was in the hope of ascertaining the component parts of the creosote which he once used with such admirable results, that he ventured on these remarks; for the creosote that he formerly used for preserving wood was as valuable as that which he was now using was useless and worthless, and all he asked of manufacturers was to give him material like what he had before.

Mr. W. Lawford wished to inquire how it was that, in the face of such undoubted proofs of the value of the creosoting process, some of the large railway companies, and notably the Midland, had given up creosoting their sleepers? He considered it the duty of every one who used timber largely to adopt either this or some other antiseptic treatment, since large encroachments were annually made upon the timber-growing districts of the world, without an adequate supply of timber-producing trees being planted for the use of posterity.

Mr. C. Lowe, in reference to the constituents of the creosotes employed for "pickling" or preserving timber, was disposed to attribute to the tar acids only a very small amount of the effective results obtained by the application of the creosote, for the following reasons:

1. Carbolic and cresylic acids were both completely volatile even at an average summer temperature in England, and in hot climates could not long remain present (except as traces) in any timber to which they had been applied.

2. Both these acids were readily soluble in water, and would consequently be rapidly removed from the timber in case the latter, previously saturated with them, was subjected to the action of water in motion. He regarded the action of coal-tar creosote in preserving timber as presenting a two-fold character; first, a mechanical action, by which the wood was rendered waterproof from the filling up of the cellular tissue with matter insoluble in water; second, a chemical or antiseptic action, due chiefly to the presence of the tar acids. These tar acids were roughly divisible into the readily volatile acids soluble in water (carbolic and cresylic), and the heavy, almost non-volatile, acids insoluble in water. The latter class had not been thoroughly studied, but it was known to be powerfully antiseptic, and anti-parasitic. He therefore considered the creosote best adapted for the "pickling" of timber to be a creosote containing sufficient solid hydrocarbon, such as naphthaline, to be solid at a temperature slightly above the average climatic or other temperature to which the timber was to be ultimately exposed; at the same time, to prevent the attacks of parasitic insects, etc., the heavy tar acids should be present. No reliance should be placed on carbolic and cresylic acids for pickling timber, seeing they were so readily removed by the action of water and climatic heat. It was well known

that their albuminous combinations were readily broken up by simple washing with water; as germicides and antiseptics, when retained *in situ*, these acids were invaluable for surgical use and disinfection, and to these purposes they should be relegated.

Mr. T. E. M. Marsh exhibited specimens of timber used by the late Mr. Brunel in 1839. These were fair samples of the bulk of the timber of the ribs of the skew bridge over the River Avon, at the Bath station on the Great Western Railway. The timber was cut from Memel balk, and was kyanized. It was quite sound after forty years' service. The kyanizing process had been employed extensively by the late Mr. Brunel in the early works of the Great Western Railway. The permanent-way timbers were thus prepared, and gave excellent results as to preservation from decay, as was shown by specimens cut from various parts of the line, between London and Bristol, after having been laid from fifteen to twenty years. Mr. Marsh had gained much experience in the preparation and uses of creosoted timber, both while acting for Mr. Brunel, and subsequently up to the present time. In the early days of the process, the tar from which creosote was prepared was not subjected to the extraction of so many chemical ingredients as was now the case, and the naphthaline, or salt precipitated was comparatively small, and considered of little value. No difficulty was then experienced in getting a good admixture of light and heavy oil in a fluid state, of satisfactory color, consistency and taste, and complying with the rough and ready tests adopted. Mr. Brunel adopted the process extensively from its early introduction by Mr. Bethell, in bridges and permanent way, and much of those timbers and structures remained in use at the present day. It was, however, soon discovered that it was of great importance the timber should be well seasoned and dry, and that it was worse than useless to creosote unseasoned, damp or wet timber. Some alarming cases of internal decay had been discovered, attributable to these causes. Of late years, on account of the greater demands on the timber merchants, and for other reasons, the preparation of creosoted timber had not always had such careful consideration. The processes were often carried on, not only not under cover, but water in variable quantities was generally found in the tanks from which the oil was pumped into the pressure-cylinders, and solid salts and a mixture of mud and the residuum and drainage of objectionable matter from the timber of preceding charges, accumulated in the tanks and returned again, to the detriment of subsequent charges. It not unfrequently happened that timber coming from the pressure-cylinders might be found with some portions presenting no trace whatever of creosote even on the surface, but showing only signs of the contact of dirty water, when the quantity of creosote injected was supposed to have been 50 gallons to the load. Such facts, Mr. Marsh asserted, were sufficient to account for many reported failures, without reference to the chemical questions as to the relative values of the constituent parts of the oil. Mr. Marsh's instructions to his inspectors for the preparation and pickling of timber, where thorough efficiency was desired, were based on his own personal observations, and were as follows:

"The state of the tanks from which the creosote is being drawn while the pressure progresses, and before any creosoting is done, must be examined, and if found to contain salty or muddy sediment at the bottom, or water at the top, or the nature of the creosote otherwise bad, its use must be protested against. Samples must be taken by a tube dipped to test the liquid at various depths, particularly the upper and lower portions of about 12 inches of the top, and the same at the bottom. This must be strictly attended to. No steam shall be let into the creosote anywhere. The numerous pipes used for heating, and sometimes hoses and joints, may give the means of mixing in steam during the process, and hence the condensed water, which must not be permitted under any circumstances. Sometimes the appearance of the timber after creosoting will show that water has been in contact with it. The thorough good creosoting must also be checked by a chisel at the sound hearty parts of the timber, and the penetration checked by weighing trial sticks with each charge (these should

not be open sappy timbers, and they should be the least dry rather than those to favor absorption more than the bulk in the same charge). A good percentage, over 50 glallons to the load, must be injected so as to allow for outside drainage when drawn out of the cylinder. In weighing, 50 gallons may be reckoned as 550 lbs. If the timber be not quite satisfactory and perfectly dry, and immediate delivery is urgently wanted, then a considerable extra quantity must be injected, as much as 10 per cent., or further drying, and under cover, must be insisted upon, but in no case must positively wet or damp timber be allowed to go into the pressure cylinders."

Mr. Benjamin Nickels observed that he was much gratified in noting that the author had drawn special attention to the compound acridine, pointing out, at the same time, its high antiseptic value as a constituent of creosoting materials. It would appear that his impressions had been based on certain marked properties exhibited by this peculiar substance, notably its intense pungency, acridity, and high antiseptic value, also its immunity from loss by evaporation and the solvent action of water. As little beyond a mere reference to the compound had been made, it might be of interest to state what had been done in other directions, and so far as it might corroborate the views advanced by its author. In the year 1882 he was induced to take out a patent for a composition to be used as an insecticide, and for the coating of ships' bottoms and other submerged surfaces, and in which acridine played an important part. He had, during a previous experience, met with many opportunities of observing the painfully irritating action of the heavier tar oils, arising from handling during the treatment and purification of anthracine, due to the presence of acridine, and as an outcome of the observation it had occurred to him that this substance should constitute an effective "antifoul," inasmuch as it would be almost impossible for animal life to remain in contact with it. Experiment in numerous directions fully supported the idea; but the question arose, would the acridine resist the prolonged solvent action of water, and remain effective for a lengthened period, and in the thin coating of any composition that could be applied as a paint to a ships' side? Opinion varied considerably as to ultimate success when attempted on a practical scale, although laboratory trials had shown that such composition was unacted upon in still water. The first experiment of any importance was made on a small iron barque (the "Cordova") which sailed from London for the Falkland Islands about the end of January, 1882, returning at the end of October, after an absence of nine months, during which her hull had been constantly submerged. Previous to sailing, portions of her plates towards the lower part of the vessel, and where subjected to the greatest wash, had been coated in the ordinary way of applying a ship's paint with acridine composition, prepared in conformity with the patent referred to. He was present on her return to England, and upon the vessel being docked for repainting and repair, he made a close inspection of the portion that had been originally coated with the composition. He found that the paint had remained intact, presenting a smooth and unbroken surface; it had adhered most tenaciously to the iron plates, completely protecting them from the action of the sea. There was no adhesion of barnacle or weed, and the evidence of contained acridine was very manifest on applying the tongue to portions of the composition scraped from the side of the vessel. Subsequent examination showed that there had been little or no loss of acridine, and that the prolonged and beating action of swiftly-running and boisterous seas had failed in removing or washing out the acridine originally incorporated in the paint applied. Since the date of this experiment many others had been made, and were still on hand, with vessels on long sea-voyages, and, as far as he was enabled to state, the results obtained had been of a satisfactory character.

It would be difficult, perhaps, to cite more complete illustrations of the indifference of a substance to severe water action; and the author might, he thought, rest well assured that his statements concerning this singular tar product were in nowise overrated or exaggerated. As regarded the antiseptic character of acridine, he might mention that it was of high value, extremely small quantities be-

ing sufficient to arrest the change in many organic substances prone to rapid decomposition.

Mr. Martin F. Roberts wished to direct attention to a point which had influenced engineers in their preference for the so-called "Country oil," viz., that of economy. Engineers would be aware that in drawing up specifications it was usual to stipulate for a certain quantity of creosote to be injected into a cubic foot of timber, usually 6, 8 or 10 lbs., the contractor's price for creosoting being regulated according to the quantity specified; and it thus became necessary for engineers to consider whether, say 8 lbs. per cubic foot of the thick, heavy, London creosote penetrated as far into the timber as 8 lbs. of the thinner country oils. He was sure all engineers would agree that it would not; and from his own experience he was able to say that, with telegraph poles, in many places where 8 lbs. of London creosote per cubic foot had been injected, it had not penetrated more than half through the sapwood, whereas a similar quantity of country oil would have penetrated completely to the heartwood, although, of course, the country oil would not leave as large a deposit of solid substances in the pores of the timber. It was, therefore, desirable to consider whether it was better to have the sapwood completely injected with thin oil at a certain price, or the outer portion only injected with thick oil at the same cost, and his experience led him to prefer the complete injection by the thin oil. His ground for arriving at this conclusion was that, although he had met with many samples of creosoted timber in which a portion of the sapwood had decayed where the creosote had not penetrated, he had never met with a piece of timber having decayed where the creosote had penetrated, except in one instance in a Government telegraph pole, referred to in the discussion; and even in this case he thought it well to ask if the decay had taken place before or after creosoting. Engineers acquainted with red fir timber would remember that what was called a "foxey pole" was occasionally found, in which, although the outer portion or all of the sapwood might be quite sound, some of the inner portion of the pole had decayed before felling; and it was often a difficult matter, even for an experienced inspector, to detect such a pole. It would easily be conceived that in such a case the decay might be, and often was, attributed to a defective quality of creosote having been used, instead of to the fact that a portion of the pole was rotten when treated.

The remarks made by the author under the heading of "The Conflicting Theories of Putrefaction," in which he spoke of the "gaping orifice of a crack produced by the sun in a piece of timber," would appear to specially point to the necessity for the use of a thin, penetrating oil, as timber would crack after long exposure in the sun, even if it had been creosoted with the thickest London oil; and in these cases the oil which had penetrated the deepest would be more effective, as it was the most likely to have genetrated beyond the depths of the crack. If it were the practice to completely saturate the entire mass of timber with creosote, and if it were found possible to do so in all cases, there would then be no objection to the use of London oils; but as the question of cost had to be considered, and the smallest quantity of creosote per cubic foot which was found to answer the purpose was therefore specified for, the thinner country creosote was preferred, owing to its greater penetration, weight for weight. In Mr. Coisne's experiment with shavings, the conditions were so totally different to those met with in ordinary practice, that too much reliance should not be placed in them. It was obviously an easy matter to completely saturate shavings either with thick or thin creosote, but with telegraph poles and railway timber the creosote never penetrated completely through the timber, and it could not be contended that the exclusion of germs alone prevented putrefaction, as, if so, a coating of tar would prevent decay. What was necessary was that the germs of decay in the timber should also be destroyed, and this could only be accomplished by bringing all that portion of the timber more liable to decay—viz., the sapwood—under the influence of a creosote of considerable penetrating power. If evidence in support of this assertion were needful, it would only be necessary to refer to the fact that engineers strictly barred the use of whitewood timber for telegraph poles and other purposes, owing to its

being found impossible in practice to inject creosote into whitewood to a greater depth than ½ or ¾ of an inch from the surface, and whitewood timber so prepared, either with London or country creosote, was found to decay rapidly. It would appear that the best system of creosoting would consist in first injecting the timber with thin "country oil," then running the thin oil off and filling the cylinder with London creosote, which, being forced in by increased pressure, would drive the thinner oil further into the timber, and the thicker creosote would hermetically seal the outer pores of the timber. Failing this process, owing to its increasing the cost, it would appear advisable to use thin creosote, and if it was considered that thin oil did not sufficiently fill the outer pores of the timber, the process, at a trifling cost, could be supplemented by giving the timber a coat of hot tar.

Mr. Greville Williams stated that he regarded the paper as the most valuable and exhaustive contribution yet made to the literature of the subject. He agreed with Dr. Meymott Tidy and the author in considering that the value of the carbolic acid in creosote oils had been overrated. He believed that an oil from which the carbolic acid had been removed would sterilize wood, if thoroughly impregnated with it, partly by virtue of the organic alkaloids present, and partly by the protective influence of the heavier oils themselves. He had satisfied himself by careful experiments that the alkaloids exercised a potent influence in preventing the development of bacteria, mould, and microscopic fungi in vegetable infusions. He thought, moreover, that where wood had to be exposed to the action of sea-water, it would be advantageous to use a creosote containing a high percentage of the alkaloids; this could easily be attained by well-known methods. Although the minute quantities of carbolic acid remaining in old creosoted timbers were too small to account for their preservation, he considered it right to say that, by a sufficiently delicate method of manipulation, he had rarely failed in getting evidence of its presence even thirty years after the wood had been creosoted. He found traces of it in eleven out of fourteen specimens which had been creosoted from twenty-five to thirty-two years before. The organic alkaloids, however, which remained, were sufficient to allow quantitative estimation. He thought that no chemist, who had examined very old sleepers for carbolic acid, could come to any other conclusion than that the traces remaining were insufficient for their protection. A point, moreover, of great importance for the proper comprehension of the subject, was involved in this almost entire disappearance of the carbolic acid. If the coagulation of the albumen by the carbolic acid were the cause of the preservation of the timber, how was it that this acid almost entirely disappeared? The instability of the compound, of albumen with carbolic acid, was well known to those chemists who had minutely examined it; nothing more conclusively proved this instability than the disappearance of the carbolic acid. With regard to the naphthaline, he thought it significant that it was only absent from two of the sleepers he had examined. There could, he considered, be no question that naphthaline, although perhaps feeble as a germicide, properly so called, was very valuable as a sterilizer; it was insoluble in water, and once in the wood, clung to it tenaciously. He was also most decidedly in favor of the removal of all restrictions as to maximum boiling-point, and considered that, if the oils were fluid at the temperature of injection (say 100° to 120° Fahrenheit), that was all that was needful. On the whole question, he found himself able to thoroughly indorse the conclusions of the author and Dr. Tidy, and he considered that specifications which excluded the use of London oils were framed under a misapprehension of the true nature of the condition requisite to afford a good creosote.

A PATENT taken out for a yellow metal, by T. Parker, describes it as of great tensile and compressive strength and hardness, and made by melting copper, 50 parts; spelter dross, 25-30; spelter, 12-17; tin, 2½ parts; with a flux of the following composition made into a paste: Salt cake, 5 parts; coal-dust, 5; silica, 15; bone ash, 20 parts. Manganese or copper sulphate, or the chlorides of these metals, and also common salt, may be used in place of the salt cake. This flux is also applicable to the founding of brass and bronze generally.

TECHNICAL EDUCATION.

By HENRY CUNYNGHAME.

From the "Journal of the Society of Arts."

The question of technical education is one which is daily growing into greater importance. The report of the Technical Education Commission has afforded fresh information as to the condition of technical schools abroad, and the efforts which have been lately devoted to the subject have attracted public attention at home. There is, however, one application of technical education of which I wish particularly to speak this evening, namely, its relations to the apprenticeship system.

By technical education is meant instruction in the art of applying the discoveries of science to the requirements of modern industry. It is not scientific teaching, in the strict sense of the word, nor, on the other hand, is it mere craftsmanship. It is rather the application of science to craftsmanship. To take an example, the mode of treatment of electrical subjects given in the works of the late Professor Clarke Maxwell is purely scientific and mathematical; in his hands electricity is an applied mathematical science. In the hands of Faraday, electricity becomes an experimental science. But neither the teaching of Faraday nor Maxwell is of exactly the kind that is suitable for a mechanic—something more practical is needed; the body of truth must be arranged rather with a view to action than to knowledge. This requires a bent of mind that is midway between science and craft; it is the liberal part of the education of an artisan. We require manual dexterity, guided by wide views, and scientific knowledge accompanied by executive power. Hence it follows that it is useless to expect the technical school to take the place of the workshop, or a technical course of instruction to replace the apprenticeship system.

How often does the engineering training given to some young gentleman result in making him a mere theoretician, an architect who cannot lay a brick, or a surveyor who does not know as much of detail as a small practical builder? And, on the other hand, how many artisans are there, of excellent practical skill, whose efforts are not guided by any scientific knowledge, and who work entirely by rule of thumb? They even distrust and dislike theory, not recognizing that the true aim of theory is to classify and embody the soundest rules of practice.

This truth is seen more clearly when we read the lives of original thinkers like Watt or like James Nasmyth. Their success consisted in the art for which, as a nation, the English are so conspicuous, of reducing theory to practice, and again, of evolving theories out of practical results. So far from theory being opposed to practice, no great success can be achieved without an intimate union of the two, and no rule is safe nor result sound, unless both are blended into harmonious union. Here is the true aim of technical education.

In the report on technical instruction of 1884, it is recommended not only that charitable endowments be applied to technical instruction, but even that local authorities should be empowered to establish, maintain and contribute to the establishment and maintenance of technical schools and colleges. This recommendation is calculated, and with reason, to alarm the mind of the already burdened taxpayer. He may well ask whether, having undertaken to educate all the children in England in the three R's, he is, in addition, to be saddled with the expense of teaching every artisan his trade. And when the enormous expense of technical instruction is considered, as displayed in the details of the two volumes of the commission, the ratepayer may well shrink from the prospect.

It is, therefore, incumbent on the friends of technical education to consider how it may be most economically carried out; and I believe it will be found that the solution of this question lies, not in attempting to replace workshop training by classes or lectures, but rather to supplement it by theoretical instruction. And the best time of life to do this is during the period when the mind

is most receptive, namely, during those years in which the artisan is serving his apprenticeship.

It is often rather hastily said that the apprenticeship system is dead. I think that nothing is a greater mistake. I believe that if careful inquiry were made, it would be found that most good artisans have been apprentices, and that the greater part of boys now learning trades are serving five or seven years. But there a great difference between the apprenticeship system of to-day and that of the past. In former times the apprentice was usually an inmate of his master's house, who boarded, lodged and clothed him. Now, the boy, in general, lives with his parents, or else with some relation or friend, generally paying for his keep, at least in part, out of his earnings.

Moreover, the spread of handicraft literature has almost destroyed trade mysteries or secrets, and the result is that, in the greater number of trades, no premium is now required from a boy on his going into articles for seven years. On the contrary, he generally begins with low wages, averaging apparently about 5s. per week during the first year, and rising to 15s. or 18s. per week during the last.

The strictness of the system depends greatly on the nature of the trade. For instance, in those trades which are not greatly subject to foreign competition, trade societies are usually powerful—as, for instance, the bookbinding trade. Most of the shops or factories in these trades are society shops, and therefore it is exceedingly difficult for a workman to obtain employment unless he has served his term of apprenticeship, and his articles have been duly *vised* by the officers of the society.

In other trades, such as clock and watch making, the competition from France, Switzerland and America is so formidable that the societies have but little power, and hence the apprenticeship system is laxly administered.

Now, I think that all attempts at technical education will be imperfect that do not, to a certain extent, deal with the apprenticeship question. It is in the workshop that the artisan must be really formed; just as the barrister must be trained in chambers, or the doctor in an hospital. At the same time, what the university is to the advocate or medical man, the technical school should be to the artisan; only, however, with this difference, that it is generally necessary that while he is learning, the artisan should also be working, and that his technical instruction should go on contemporaneously with his workshop employment. At present considerable sums are yearly spent out of charity funds in paying fees on indentures. With some exceptions, I believe that money so spent is almost wholly thrown away. Since the masters are, in most trades, willing to take boys without a fee, the payment of a fee only enables the charity trustees to bargain with the employer for higher wages for the boy, and thus to cause the fortunate inhabitants of some ancient precinct or parish to secure one or two shillings per week pay more than their fellows in the same shop—a result which, it must be admitted, is not desirable.

Moreover, under the present system, sufficient care is not taken to select for the boy a trade that is suited to his ability. Indeed, under the present system, this is impossible. He is asked by the trustees, or by their clerk, what trade he would like. Now, it is notorious that boys have the very vaguest ideas of what they desire. Some tale by a friend, or some story in a boys' journal, supplies them with a picture of life, and in most instances they only ask for a light trade and a kind master. The articles are then signed and the money paid, and the seven years' contract is irrevocable. In too many instances the boy only finds that he has embarked in a career that he dislikes, and the master, that he has got an unsuitable apprentice.

Moreover, under the present system, the boys are not sufficiently looked after out of work hours. Whatever may be our opinion of the undesirability of over-parentalism towards men, there is no doubt that it is good for boys to feel that there is someone who has authority over tkem, and who desires their welfare, who takes an interest in their work, and who will endeavor to rescue them if they fall into dissolute habits. No one who has witnessed the work that the late General Gordon did among the ragged boys of the suburbs of the East of London can ever under-estimate such influences. To exercise such supervision

has ceased to be the master's duty, and now it is left to chance. Hence it is that anyone who will visit the lower and cheaper music-halls of London will find them crowded with apprentices about seventeen years of age. Here it cannot be said that their taste is improved, or their habits of sobriety encouraged, and here too often they form friendships which lead them into extravagance and idleness, and often connections with the other sex that are immoral and undesirable, and end by producing lives of misery for others besides themselves.

To counteract the evil I have spoken of, there appears to me no more useful plan than to form youths' institutes. The duties of the officials of these institutes should be to see to the apprenticing of boys, encouraging them to enter such trades as offer good prospects, and endeavoring to guide their choice. In all cases a month's trial without pay should be insisted on, and free liberty given to master and boy to refuse the proposed contract at the end of the time. There is no fear that, under such conditions, a boy will desire to change too often.

At such institutions lists should be kept of masters desiring hands, and of boys desiring to learn trades. The character of the masters should be carefully investigated with a view to ascertaining whether they are fit and proper persons, or whether they have lately been bankrupt, and are likely not to be able to fulfil their engagements.

Moreover, the boys should be periodically inspected, a monthly report being given by the master respecting their conduct; and, on the other hand, steps taken to compel the masters to stand fairly by their side of the contract.

It may be a matter for question whether the society should aid a master in punishing an apprentice who runs away or steals. At present, when an apprentice breaks his articles, few masters will take the trouble to prosecute him, and hence is laid the foundation for the idea, so prevalent among all classes of mechanics, that no working man is to be considered in any way bound to a contract. This is a source of constant loss to masters, and results in lower wages to men, and nothing would counteract it so effectively as to force boys, from their earliest years, to see that if they make a contract they are to be bound to stick to it.

I do not believe that it will be necessary or beneficial to pay the apprenticeship fees out of charity money. Quite sufficient aid would be given by lending it, subject to gradual repayment, say, of 6d. per week out of weekly wages.

Such institutions should be closely connected with, and form part of technical schools. Most of the classes would naturally be evening classes, and the subjects so chosen as to be useful to the boys in their various trades. And it would undoubtedly much conduce to the efficiency of such institutions if a certain number of working men could be placed on the committee, to give the benefit of their experience, and to inspire the boys and their parents with confidence.

During their apprenticeship, the boys should be encouraged to exhibit proofs of their skill, for which exhibitions should be awarded; and one of the best forms that scholarships could take would be the setting free of the industrious boy for a few more hours a day, in order to devote the time to study at the institute. Periodical exhibitions give a boy pride in his work, and encourage the feelings of enthusiasm with which the apprentice in older days was wont to regard his "masterpiece."

And to my thinking, the technical school should not stop here. Amusement and exercise are as needful for boys as instruction; and while it would not be right to spend either money given by charity or raised by taxation in providing amusements, still institutions such as I have named might lend spare rooms or yards for gymnasia or recreation purposes, or might provide baths and refreshment rooms, where tea and coffee, or even dinner could be got, but so always as in these respects to be self-supporting. A public library available for all classes would be a useful adjunct.

The above scheme may perhaps seem very extensive, but it is no more than I feel persuaded can be done without an extravagant cost, provided economy is carefully studied.

Moreover, a system akin to this is now in operation with the best results, in the excellent charity organization in the East-end of London managed by the

Jews. They have unusual difficulties to contend with, owing to the boys, from religious reasons, not being able to work on Saturday; and yet they have so arranged matters as to make the apprenticeship system adopted by them almost self-supporting.

And this leads me to lay before you a few considerations upon the expense of technical education. In such institutions scientific apparatus is a necessity.

It behooves, therefore, the friends and advocates of technical education to endeavor to discover whether the cost of this apparatus cannot be so decreased as to bring it within the reach of the masses, for certain it is that if technical instruction is so costly as it has hitherto proved, it is not to be expected either that the working classes should pay for it, or that the expense should be inflicted upon the public.

Every amateur who has dabbled a little in science, no less than every man who has made it his profession, knows the expense of scientific instruments. If he wants to measure the length of a wave of light, there is a bi-prism to be bought, mounted in its stand of polished brass, with a parallel slit in another similarly polished stand, a collimating lens, and a similarly mounted telescope, which, added together, make up a pretty heavy sum when purchased from one of our instrument makers.

But people are not sufficiently aware, because they have not been taught, that as far as the verification of the theory of light goes, such wave lengths can be measured with all the accuracy that educational purposes require, with a small piece of visiting card, a piece of fine wire gauze an inch square. and a two-foot rule, total cost, say 1d.

In the same way, a spectroscope can be made out of paper tubes, cheap lenses, a few pieces of glass, and a little bisulphide of carbon, at a total cost of, say, 5s., which will divide the D sodium line in a manner quite sufficient for the instruction of mechanics. Instances of this kind might be multiplied indefinitely.

There is no scientific instrument of any sort which cannot be made to serve for educational purposes, at a cost of as many shillings as it now costs pounds. But such a mode of study and teaching requires certain rules to be rigorously adhered to. In the first place all lacquer, French polish, ornamental paint, and varnish, must be strictly discouraged, and even forbidden. The students must be taught not to spend one moment on ornamentation of any kind. The one aim must be accurate fit and adjustment of essential parts, and absolute indifference to all others. This rule is harder to enforce in practice than might be imagined. When a new microscope comes home from the maker's, its polished mahogany case is opened, and to the eyes of the beginner, a vision of splendor is revealed in the golden gleam of the shining lacquered brass. All this he must studiously avoid. It is the mere joy of a child in a new toy. His only care or thought ought to be whether the lenses are optically true and achromatic, and, with these conditions satisfied, he should be carefully weaned from the prejudice in favor of varnish and veneer.

I saw, lately, a spectroscope by a leading London firm. Nothing could be more elegant than the finish and polish of the brass, but on investigation the axis was found too short, and not truly in the center of the dividing plate, and the tube of the collimator was actually half an inch too long. A better result could have been obtained with some pieces of wood and cardboard than with this forty-guinea instrument. If half the time spent in polish and lacquer had been bestowed on accuracy in the essential parts, the instrument would have been worth double the money.

The first benefit of the adoption of the system of making the students construct their own apparatus is, therefore, economy. All that is needed is a store of flat glass, glass tubing, wood of different sizes, brass discs, screws, wire, and various chemicals, and a few simple tools, such as a fine tenon saw, pliers, and a few files, while for general use, a glazier's diamond, a grindstone, and a large dividing protractor and steel scale, a pair of accurate balances, and a few such apparatus, should be in every laboratory.

The comparative list in page 138 will show, as an example, corresponding sets of instruments. In the second column is placed the price as usually charged by a good instrument maker; in the third,

the price at which an equivalent instrument can be constructed by the student; in the fourth, the time a skillful workman would take to make the instrument and adjust it. (Of course beginners would take far longer.)

On the table I have here a set of tools and chemicals with which many of the apparatus here before you were made, and which is amply sufficient to make all that are given in the above list. The cost of the whole comes to £6 10s. The details are given on a card placed on the table. In this way a sum of £20 would go far to set up a technical class with the necessary tools and appliances, while if it were desired to do more elaborate work, the addition of a lathe, bench-vise, set of drills, and other simple tools could be made for about £60. But not only is economy consulted by this system of teaching, the instruction is far more efficient. Suppose, for instance, it be desired to exhibit the qualities of polarized light. Whether will it be better to buy a shop-made polariscope with all the adjustments already made, or to cause the student to place his reflector at the true polarizing angle, to secure it there with neat pieces of cork and sealing wax, to place his bundle of microscope glass plates in a tube, again securing the proper angle of inclination, and to tinker up the instrument till it works, at an outlay for materials of, say, a shilling? Which of two students will use a really fine instrument the best, one who has been trained in the manner here advocated, or one who has always been provided with instruments ready made?

It must be remembered, too, that this course has always been followed by the great discoverers. We read of Newton discovering the shadow fringes of light, by means, as he tells us, of two pointed knives ground flat, and with their points pricked into a board. He misunderstood the causes of the phenomenon, but Fresnel afterwards discovered and explained them with no better apparatus than some pieces of cardboard and sewing thread. Mr. Justice Grove's first battery consisted of a wine glass, a tobacco pipe, and some bits of metal.

It is not sought here to depreciate accurate instruments. For purposes of measurement by an observer engaged in

	£	s.	d.	s.	d.	Hrs.
Electricity.						
Thomson's reflecting galvanometer.	10	10	0	8	0	12
Ordinary galvanometers	1	17	0	2	6	6
Tangent galvanometer	3	0	0	2	6	4
Resistance coils	2	10	0	4	0	9
Lamp and scale	0	15	0	1	6	1½
Magnetic needle	0	2	6	0	1	½
Set of magnets	0	1	6	0	2	¾
Thermopile	0	17	6	0	8	2
Four-battery cells	0	6	0	0	8	1½
Magnetometer	1	1	0	0	2	2½
Small portable do.		?		0	1	¼
Induction coil and condenser.	0	10	0	4	0	5
Electrophorus	0	5	0	2	0	2
Leyden jar	0	2	0	0	8	1
Condensing electroscope	0	3	0	0	1	1½
Dry pile	0	5	0	0	6	1½
Quadrant electrometer	10	0	0	3	0	15
Light.						
Spectroscope	5	5	0	5	0	8
Wave length measurer		?		0	1	½
Liquid prisms	0	15	0	0	6	¾
Refraction measurer	5	5	0	1	0	4
Polariscope	2	10	0	1	6	2½
Concave and convex mirrors.	0	4	0	0	4	1
Telescope*	2	10	0	2	6	8
Photometer	0	5	0	0	2	½
Spirit level	0	1	6	0	2	¼
Sound.						
Monochord and weights	1	10	0	0	8	3
Glass tube for dust figures	0	5	0	1	0	2
Telephone	0	6	0	0	6	1½
Microphone	0	5	0	0	4	1
Heat.						
Expansion machine	1	0	0	0	8	2
Differential thermometer	0	5	0	1	0	2
Alcohol thermometer	0	1	6	0	3	2

* An excellent lecture on "How to Make a Telescope for 2s. 6d.?" was lately given to boys by Prof. Norman Lockyer.

original research, no refinement can be too delicate, no mechanism too good, but the path of the student should be upward towards these things, through a course of study with self-made apparatus.

Moreover, such construction teaches the learner the use and nature of materials. You may, perhaps, remember the construction of a reflecting galvanometer if it has been taught to you—perhaps you may forget it; but if you have once wound a coil with 3,000 turns wrong, and had to unwind it, we may certainly count on your not doing so a second time. If you have cemented a liquid prism with marine glue, and filled it with bisulphide of carbon, you will not probably repeat the error.

The neat use of blowpipe, the handling of heated glass, the use of paraffin wax for the countless purposes to which it is applied in the laboratory, are of themselves an education of the highest value, not only to those who are to be engaged in original research, but also to those who are to become foremen or artisans. But it might be, perhaps, objected that the method consumes too much time. This, of course, would greatly depend on the teacher. It is not proposed that a student should make every conceivable piece of apparatus. For instance, it would be absurd to expect him to try and rule a diffraction grating. And, again, his instruction will be supplemented by daily lectures, at which more elaborate experiments will be performed by the professors. It is, however, always found that all knowledge takes time to penetrate, and as it were to suffuse the brain, and while the fingers are working the mind ought to be thinking.

It may further be asked whether such a system as is here advocated has ever been tried, and with what results. The answer to this is that such a system has been, and is being, practically worked in a few places, and it is much to be wished that it were greatly extended.

The merit of practically applying, if not of inventing this mode of instruction, is probably due to Professor Guthrie, of the Science Schools, South Kensington, and every year in the summer teachers are taught in this manner how to teach. At the end of the course an industrious student will go away armed with a whole cabinet of scientific apparatus (for the apparatus becomes the property of the pupils who make them), and with this he would be able easily to teach a school. In technical schools, wherever practicable, it should be made a condition of appointment that at least the assistant demonstrators and lecturers should be able to use their tools well.

Too much pains cannot be taken to inculcate this system. Let prizes, if needful, be given for the simplest and cheapest apparatus which shall secure a result of a certain specified degree of accuracy, and encouragement of this kind will speedily provide a scientific set of apparatus in every village school.

Therefore I urge that it is time that if technical education is to be widely spread, it should take the form of a system, that rigorous economy should be practiced, and while no salary is grudged to a competent professor, he should be required to work with cheap materials. The problem is not so much how to do it, as how to do it cheaply.

When, however, we reflect on the splendid ability that is in this country being devoted to technical education, when we read the names of the professors at the great educational establishments scattered over England, we must certainly feel that we have ample guarantee for the solution of the problem.

Moreover, these establishments are on the increase. Not to speak of the Finsbury Institute, there are several smaller ones which are well worthy attention. The Horological Institute in Northampton-Square, aided by a small grant from the City Guilds, is doing admirable service; and the munificence of a private gentleman, Mr. Quintin Hogg, has provided an institution which now numbers 3,000 youths as members, 2,000 of whom attend evening classes, which are also attended by 4,000 more youths who are not members of the institute. By this institution, which occupies the site of the old Polytechnic, 7,000 young men are benefited and provided with physical and mental training and recreation, and assisted in keeping out of mischief. I believe that the original outlay did not exceed £30,000, and that the yearly total cost of maintenance is about £7,000, of which a large part is covered by the fees and subscriptions.

To attempt, at public expense, to do what can only be done in the workshop, is a mistake; it will fail in its results, and it is unjust to expect the nation to pay for it, but it has been endeavored to show, first, that in attempting to improve technical education, some attempt should be made to deal with the apprenticeship question; and, secondly, that at an almost nominal expense, and by the application of a proved method already in use, practical technical science may become a part of the education of our town and and rural population.

It need only be added that it is highly desirable to make the boys and their parents pay, as far as possible, for the benefits they receive, and no money is such

an excellent investment of capital as that which is wisely laid out in education.

DISCUSSION.

Dr. Gladstone, F. R. S., agreed with the reader of the paper that this question was one of primary importance. Whether the system of apprenticeship still continued or not, there was no doubt that it was less common than it was formerly, and that in some trades it was dying out. In any event, technical classes were required, for though he held that no trade could be learned except in the workshop, still those would make the best of the instruction given them in the workshop who had had a certain amount of theoretical foundation laid beforehand. These classes might be formed in various ways, and an excellent example had been referred to in the case of the Polytechnic Institution, where so many students were being well taught various matters which would be of great service to them in connection with their handicrafts. He did not think it was desirable to teach trades in the elementary schools; what should be taught there should be what would be useful to all the scholars, not to a portion of them only. People were gradually working up to this idea, but still vaguely. Many wanted something definite, in the shape of carpentry or iron-working, and seemed to think this was the true kind of technical education; but it should rather be their aim to give such a notion of the value of materials and the use of tools as could afterwards be turned to use in any required direction. There were two great difficulties in the way of doing this in elementary schools. The first, and greatest was the inveterate notion that education consisted of book-learning. No doubt, centuries ago, when education was the privilege of the few, and schools were intended mainly for those who were to enter the professions, the chief part of education was of a literary character; but now that education was become the right of the masses of the people, the problem was entirely altered. Children should be taught that which would be most useful to them in after life; they should not be trained for professional men, or even for clerks. No doubt they should have that simple kind of literary education which would make them all clerks, because mere clerkship ought to be the lowest kind of work; but, beyond that, they should be encouraged to develop some kind of skill. It was very difficult to get over this kind of prejudice in favor of a literary education, and, while it lasted, the knowledge of science and the arts would not take their right position. Another difficulty was the ignorance of teachers in this respect. If an endeavor were made to introduce some knowledge of science into schools, they generally found that the teachers had some kind of theoretical knowledge, but it had been obtained mainly from books; and what was chiefly wanted was that things should be taught as well as words, and before words. He did not say things only. They wanted *res et verba*, but the *res* must come before the *verba*. Pupil teachers had to give object-lessons, but they were not taught or examined in this subject. This was one of the greatest wants in connection with the system of education. Some of these difficulties, however, could be, and he hoped were being, overcome. The idea that education must consist in the teaching of literature was giving way, and subjects of fictitious importance, such as spelling and good pronunciation, would, he hoped, soon be considered less material, whilst a knowledge of the world in which we lived, the forces with which we had to deal, and the materials which must occupy the attention of the great mass of the community, would attract more attention in future. Some of these difficulties might be got over by the peripatetic system of teaching science which was employed in Liverpool and Birmingham with great success, and he was glad to say the London School Board was about to introduce it experimentally. In one way or another he trusted the desired end would be reached, and that the long-suffering ratepayer would have more for his money than he had hitherto, and that the next generation would grow up more capable of doing the work of the world.

Mr. B. Lucraft regretted that he could not agree with Mr. Cunynghame in his view of the present state of the apprenticeship system. His opinion was that there was scarcely a good firm in London who would take apprentices. An employer wanted to get his work done as quickly as possible, and would not be

bothered with apprentices. He should like to see, in place of the apprenticeship system, and with a view of preventing all the clever boys in London turning their attention to clerkships, scholarships given in technical schools with a workshop attached. At present most of the clever boys went into the Post-office, or a place of that kind, for he found that nine-tenths of those who won prizes, when asked as to their future career, expressed such an intention. Some of the scholarships now given to clever boys were, he was sure, an injury, rather than a benefit to them. If institutions were established where the whole theory and science applicable to certain trades were taught, combined with workshop instruction, a class of artisans would be raised up superior to any which could be obtained in any other way. The apprenticeship system in London was almost done with. He had worked in workshops, and most of his acquaintance did the same, and they had the greatest difficulty in getting their sons apprenticed in any way. Some employers would take a boy and give him so much a week, so long as he behaved himself, but they would not take apprentices. Boys trained in such an institution as he had referred to—learning both theory and practice—would become overseers and foremen, and ultimately masters. He gave evidence on this subject before the Royal Commission, and suggested that some of the City endowments which were left to encourage trade might be devoted to such purposes which would be quite in accordance with the views of the original donors, though carried out in a way more suited to the present time.

Mr. G. N. Hooper said this question was now moving a little, though not fast enough in the opinion of many who were interested in it. In the first place, the Council of the London Chamber of Commerce passed a resolution, about a fortnight ago, to support and make known the benefits which would result to trade and commerce by a further development of the system of trade education; and, secondly, on Friday next, a meeting would be held by the Artisan's Technical Institute, at which Mr. Woodall would preside, when both employers and workmen would be present to consider this question. This was a step in the right direction. The subject had been treated by scientific teachers, and by the City Guild, many members of which were not practically connected with trade, and it had not been so favorably received by manufacturers as if they themselves and their workmen and foremen had been more consulted. As chairman of a technical class, he had seen excellent work done at an expense of less than £100 a year, the class averaging 40 to 50, and some who had passed through the course had done remarkably well, and would exert an influence on their trade which could not fail to be beneficial. Some people thought that technical education must be extremely expensive, but that was an error. There were a large number of school-rooms which were totally unused after dark, and they might be made available for this purpose, at a small expense for cleaning and lighting. As one of the jurors at the Paris Exhibition of 1867, he found the French were making considerable advances, and he followed up the matter by visiting the schools, becoming acquainted with the teacher and the students. On the first opportunity he had sent a suitable man over to Paris to be trained, and having had previous instruction in science, drawing, and manual labor he was able to take full advantage of the opportunities offered him, and since his return had been very useful as a technical teacher. A second one had since been sent, and thus the method of teaching usual in France had been transferred. A few days ago, in the *Chamber of Commerce Journal*, there was a quotation from the *Bourse Lyonnais*, showing that the French were dissatisfied with the progress they were making compared with other nations, and stating that in Germany upwards of 100,000 workmen were passing through the technical classes. But these facts did not come out very often. During the Paris Exhibition he attended a meeting of a group of syndical chambers, and took notes of the discussion, but the president afterwards requested him to give these notes to him, saying it was not permissible to take away any record of the proceedings. That, perhaps, might account for the fact that they did not hear much of what was going on in foreign countries; but the manufacturers in this country were beginning to feel

the pinch. The most efficient teachers were found to be properly trained artisans who had studied the sciences applicable to the occupation they followed. Difficulties were often met with in practice which no mere professor could get over.

Prof. Guthrie, F. R. S., said he only claimed a part of the merit which had been attributed to the work which had been going on for ten years in the Science Schools at South Kensington, though he was responsible for it, for he had been most ably supported throughout by the demonstrators, whose assistance it had been his good fortune to secure. When he first entered on the work, he found that the teaching of physics in this country was almost purely theoretical; and, having been trained as a chemist, he reflected what sort of a chemist that man would be whose training had been derived entirely from the lecture theatre and from books; and the question occurred to him, could not elementary physics be taught practically. Then it came to a question of expense, because he had to teach teachers. So it became necessary to devise a system, a sketch of which had been given in the paper, of getting typical apparatus for showing the elementary principles; it had to meet the wants of the teacher, and it must be absolutely and scientifically correct in principle. Mr. Cunynghame had spoken rather too slightingly, he thought, of some of the instruments, saying they did not reach the accuracy attained by an ordinary instrument maker; but he should say that such apparatus as was shown was far more scientifically and absolutely accurate than that which would be produced by the ordinary optical instrument maker. It was simple in construction, but truthful in conception, and without any adornment. This method of bringing the hand and the mind to work together really lay at the basis of all true technical instruction; where the mind alone was employed, the knowledge acquired passed away, but when the mind and the hand had been educated together, the knowledge was never forgotten. For the last ten years his life had been devoted to the development of this particular branch of knowledge, and he could not understand its being so little known and appreciated.

Mr. E. C. Robins said the impression left on his mind by visiting the technical schools in Germany was that the difference in the education of the two countries lay rather in the sections of society above the artisan class, and that the middle classes in England were most backward with regard to scientific education as compared with the Germans. All our schools were practically literary, they were not divided into polytechnic and real schule, and even science was taught more from the literary side, and less awards were given for it. He was astonished to find that so many of the working classes were profiting by the advantages offered them as Mr. Hooper had stated, but if it were true that information was not readily obtainable, that might account for the general ignorance on this subject. If foreigners were making such progress, it was all the more important that England should wake up to the importance of the matter; and, as an architect, he might say he found very few men who had passed through such a training as enabled them, properly, to execute the work they were called upon to undertake. He knew one large firm in the West-end where there was not a single apprentice taken. Another large firm, a little way out of town, employed chiefly apprentices, and some years ago, when a strike took place in the building trade, they were able to carry on their works without any trouble in consequence. There was a great difficulty in this country in getting proper instruction for youths entering the building trades; it was a favor to got them into a shop at all. He thought the trades' unions, if they were worth anything, ought to see to this; but, on the contrary, he understood they rather set their faces against apprentices, and seemed only to think of getting as much money as they could out of the master, and of giving as little as they could for it. He had heard that stated, broadly, by a representative of a trades' union at a public meeting, much to the astonishment of everybody present. There was a great fright with regard to technical education, on account of the expense involved in teaching apparatus, and he hoped this paper would do some good in helping to remove that impression. To that end he would suggest that the simple apparatus exhibited

should be photographed, so that the idea that nothing could be done without expensive appliances might be exploded.

Mr. William Trant (Secretary, Artisans' Technical Association) said the reader of the paper seemed to make a great point that a certain piece of apparatus which took fifteen hours to make only cost 4s. 6d. He did not know whether he meant to convey that technical education would enable people to work for less than 4d. an hour, but if so, that would not meet the views of artisans. All were agreed that technical education was a good thing, and almost all were agreed as to the mode by which it might be carried out; but with regard to the question of apprenticeship, there was much more room for discussion. Mr. Robins seemed to think trades' unions were very much at fault in this matter; but he would remind the meeting that they were not altogether to blame. They had found hitherto that they had to teach the apprentices for the benefit of the employers alone; and not only that, but that they enabled the youths to enter into competition with themselves, and thus the feeling naturally arose that they were cutting their own throats. The rapid improvements in machinery and other things had really revolutionized the system. Apprenticeship as it formerly existed could no longer be maintained, and they had now to consider how youths were to be trained in their respective trades. The question was whether some responsibility did not rest on the employer, and also on the Legislature, to see that lads who were apprenticed should be turned out skillful workmen. The committee which was to meet on Friday, to which Mr. Hooper had referred, would have to specially consider this question. Allusion had been made to lads who ran away from their work, and ought to be compelled to go back. It seemed to him that was just the way to make bad workmen, for unless they had a dislike to the trade they would not leave it. He agreed with what had been said, that the best persons to teach were those who understood the trade itself, and who in addition had received a scientific training. Such men were always looked up to and respected by the students, and produced the best results. As much as possible should be taught at school, and he wanted to see how technical education could be given before it was known what trade a boy was going to adopt.

The Chairman said the account just given of industrial schools was certainly not universally applicable. He had been particularly interested in seeing the Artane Industrial School in Dublin, where several trades were excellently taught, and he believed, and should be glad to hear, that the same thing was done in England. The endeavor of Mr. Cunynghame to show how apprenticeship might best be combined with technical instruction was of great value, whether they agreed with his conclusions or not. He believed him to be right in saying that the technical school could not take the place of the workshop, for experiments made in this direction in various countries had not proved very successful. There was the school, which was very much praised, of the Boulevard de la Villete, in Paris, which was visited by the Commission of which he was chairman; the expense was enormous, and the results were certainly not satisfactory to their mind. Notwithstanding that, similar schools were being established in Paris, and he could only say he was glad the experiment was being made on the other side of the channel. He did not believe that artisans and manufacturers distrusted theory to the same extent as formerly, but, on the contrary, it was more understood that theory and practice must go hand in hand. At the same time, if it was necessary to choose between theory and practice during the time which a boy had to spend in learning his trade, practice should have the preference. There the Germans seemed to make a mistake; they kept the young men, especially of the higher grades, at the Polytechnic School to the age of 22 or 23, and when they entered the workshop to begin their practical training they were, especially from the excessive attention paid to the higher mathematics, unfitted for it. He was glad to hear that Sir F. Sandford approved of the introduction of drawing as a class subject into the new Code; in his opinion it was more important that a boy who was to be an artisan should learn to draw than that he should learn to write.

Drawing not only trained a man to represent correctly what he had seen, but it forced him in the first instance to observe correctly, and no quality could be more valuable to an artisan than that of accurate observation. They would all be glad to hear from Mr. Trant that employers and employed were about to discuss the question of apprenticehip, but he did not think it was a matter in which the Legislature could interfere. It seemed to him a short-sighted policy on the part of artisans to discourage apprenticeship; if they felt an interest in the pursuit in which they were engaged, they could not evince that interest better than by training up good men to follow after them. Besides, many of them were fathers of families, and it seemed to him that, by an interchange of services in this way, the whole of their class must be benefited. He felt bound to say a word in praise of the good work in which Mr. Quintin Hogg was engaged; he believed the amount he had expended was nearer £80,000 than £30,000, but at any rate, the work was a most excellent one, and he had devoted his whole life thoroughly to it. He was also glad to hear what was proposed with regard to Christ's Hospital, and a better model than the Allan Glen's School could not be found. It was carefully examined by the Commission, and he was prepared to say that no more practical system of technical education for boys of the age received there was to be found in Europe.

He was extremely glad to hear that the Horological Institute was so successful; a school of a similar kind at Besançon was not by any means a success, nor was a similar school lately started in Paris, there being only twelve or eighteen students in the latter. No material impression could be made on the trade at large by a school of this kind, which was only adapted to those who could expend a considerable amount on the training of their children. What could be done was to encourage apprentices to combine with their practical training theoretical instruction, and of that he did not know a better example than the school which Mr. William Mather, of Salford, had established in connection with his own works. By the introduction of such schools into workshops, by the combination of several masters whose workshops were not sufficiently large to enable them to do the work alone, or by encouraging the youths to attend technical schools, like that at Manchester and Oldham, which had been supported liberally by Messrs. Platt, the great work of technical education would best be forwarded. He concluded by proposing a hearty vote of thanks to Mr. Cunynghame.

Mr. Cunynghame, in reply to Mr. Trant, said he had not included anything for time, but only for materials, in the figures he had given of the cost of apparatus. The value of the time would be difficult to estimate, as the students' time would be worth nothing, while the professor's would be worth a great deal. In some trades he believed it was true, as Mr. Lucraft said, that the apprenticeship system was practically dead; in the building trade it was very rare indeed, but in the bookbinding trade all the best shops in London were society shops, where no man was allowed to work unless he could show seven years' indentures properly discharged, and with the mark of the Bookbinders' Society upon them.

He hoped that some of the societies which were inquiring into this subject would endeavor to obtain statistics, without which their knowledge could not be exact. He had tried to get them from Mr. Quintin Hogg's schools, having asked numbers of the boys if they were apprenticed, whether they paid any premium, and so on, and in the majority of cases he found they were apprenticed. A single person, however, could not obtain full information on this point; it must be the result of organized inquiries. If they were to replace the apprentice system by industrial schools, as Mr. Lucraft proposed, was it only to be for the benefit of clever boys? If so, what was to be done with those who were not so clever? And if it were to extend to all boys clever or not, the expense would be so enormous that it would be impossible to carry the scheme out on a large scale. They must do something cheap, as well as they could, and it had been his endeavor to show that a great deal could be done very effectually, and at a small cost.

THE PROTECTIVE POWER OF ARMOR PLATES AS PROVED IN ACTUAL WARFARE.

From "The Engineer."

In view of the protest so ably put forward by Sir E. J. Reed and other well-known naval authorities, against the system adopted by the Admiralty of leaving a large portion of the hulls of our modern ironclads wholly unprotected by armor, a brief summary of the resistance to shot afforded by armor protection in the ironclad actions which have been fought up to the present time may be of interest. We purpose, in this article, to refer to those engagements only in which ironclads have been opposed to armored and unarmored ships of war, and shall reserve the subject of "Ironclads *versus* Forts" for a future occasion, omitting in both cases the actions fought during the war between the Northern and Southern States of America, as both the armor and ordnance employed by the contending parties was of too makeshift a character to be of lasting importance.

In their resolve to denude the ends of our modern ironclads of all armor protection, the Admiralty appear to have been governed by the opinion that thin armor plating is worthless. Theoretically this assumption is correct, and the various experiments at Portsmouth, Shoeburyness, Gâvre, Kummersdorf, Amager, Kolpino, Steinfeld, Muggiano, &c., apparently give this theory a practical backing, which, however, is completely overthrown by the experiences of actual warfare, as will presently be shown. It is, of course, desirable that the heavy guns, &c., of our ironclads should be provided with the thickest possible armor protection, provided that the efficiency of the vessels as fighting machines is not thereby impaired. The question naturally arises: "Are the unarmored portions of our latest ironclads so constructed that, if riddled by shell from even the worst gun at present afloat, no detriment will ensue to the steering qualities of the vessels?" The answer to this question is obvious to those who are acquainted with H. M. S. Colossus and her sisters.

The Battle of Lissa, fought in July, 1866, by the Austrian and Italian fleets, under the respective commands of Rear-Admiral Tegetthoff and Admiral Persano, affords the first instance of a modern ironclad engagement—modern in so far as both parties were in possession of *bona-fide* ironclads and of rifled ordnance. The Austrian ironclads engaged at Lissa were all armored along the entire extent of their water lines, the plating ranging from 2½ in. to 5 in. in thickness. The aggregate armament of the seven armor-plated ships consisted of 173 guns, 74 of which were 6-in. rifled, cast-iron, Wahrendorf breech-loading guns, and the remainder 48-pounder smooth bores. The Italian ironclad fleet numbered twelve vessels, four of which were but imperfectly protected at the ends, viz., the Re di Portugallo, Re d'Italia, Varese, and Palestro. The total number of guns was 248, all rifled, ranging in caliber from the 6½-in. Cavalli breech-loader to the 9-in. Armstrong muzzle-loader. It is unnecessary to dwell further on the events of this battle than to refer to the damage by shot sustained by the respective fleets. The official Austrian report says: "The resistance of the ironclads was not generally put to a very severe test. With the exception of the Habsburg and Don Juan, none of the armor-clads exhibited shot marks with impressions nearly approaching those produced on trial by 48-pounder cast-iron shot with a charge of 14 lbs. of powder at a range of one cable. The greater portion of the projectiles struck obliquely, and a single coating of paint generally sufficed to render the shot marks invisible. Several shots struck the Habsburg below the armor belt, bulging and cracking, but not perforating the wooden hull. The Don Juan exhibits the most important shot marks, three in number, produced by 300-lb. shot, two of which, with a penetration of about 4 in., are on a level with the ports, whilst the third is forward, just below the water line, having penetrated to the extent of nearly 4½ in. The

formation of these marks shows that they were caused by 9-in. rifled shot. The armor plating stood remarkably well, not a single crack being visible on the surface. . . . The only remaining shot mark of consequence is one produced by a 7-in steel projectile, which perforated the thin armor of the Ferdinand Max in a slanting direction, and remained embedded in the backing." The above extract of the official report refers only to the effects of the Italian fire on the armored portions of the Austrian ironclads, and it will be observed that even the 300-lb. Armstrong shot failed to perforate the 4½-in. armor, although this projectile is supposed to be capable of penetrating 8 in. of iron armor at a range of 500 yards.

Referring to the Battle of Lissa, the *Times* of August 31st, 1866, observes that the Austrian projectiles had very little effect on the Italian armor plates, owing chiefly to the light caliber of their guns. This does not, however, agree with the Austrian report, which says that the great loss in killed and wounded sustained by the Italian fleet was chiefly due to the fact that the Austrian projectiles struck the edges of the plates near the ports, sending a hailstorm of fragments into the interior of the vessels. No such splintering occurred with the Austrian plates." It should be observed that the Italian naval authorities then, as now, were in favor of hard and brittle armor plates of French manufacture, whilst the Austrians followed the Sheffield system of tough armor. Early in the battle the Italian ironclad, Re d'Italia, was disabled in her steering gear—which was unprotected by armor, and in this condition she was rammed and sunk by the Austrian flagship, the Ferdinand Max. The loss of the Palestro was due to a similar cause; and the Austrian ironclad Drache was, in consequence of her superior manœuvring qualities, able to pour broadside after broadside of shot and shell into her unarmored stern, until she caught fire and blew up.

The next engagement between ironclads did not occur until October 11th, 1873, when an action was fought off Cartagena, between the squadron of the insurgent chief Contreras and the Spanish Government vessels under Admiral Lobo. Contreras' flagship was the ironclad Numancia, 7,305 tons, 5-in. armor, eight 10-in. and eight 7-in. Armstrong guns; whilst the chief vessel of Admiral Lobo's squadron was the Vitoria, 7,250 tons, 5½-in. armor, eight 9-in. and three 8-in. Armstrong guns. The remaining vessels which participated in the engagement need not be enumerated, as their performances have no connection with the subject under consideration. The chief interest of the action centers in a short duel between the Vitoria and Numancia, which ended, however, by mutual consent when affairs began to assume a serious aspect. This occurred when, according to the Spanish report, "a shell from the Vitoria killed seven men on board the Numancia, including M. Moya, Vice-President of the Junta of Cartagena, and wounded eighteen others. This shell penetrated the unarmored portion of the vessel on the port side, exploded on the quarterdeck, knocked away the wheel and the head of the aft capstan, and seriously damaged the mainmast at a height of 8 ft. above the deck. One portion of the shell shivered the main yard, whilst another fragment entered the battery through the after hatch, and destroyed an iron deck beam on the port side. In this action the armored portions of the Vitoria and Numancia were struck eight and fourteen times respectively by heavy shot, but no serious damage was done, as the projectiles failed to pierce the plates.

The engagement between Her Majesty's unarmored cruisers Shah and Amethyst and the rebel Peruvian ironclad Huascar, on May 29, 1877, off the Port of Pacocha, affords further proof of the protective power of thin armor plating. The armaments of the respective vessels were composed as follows: Shah, two 9-in., sixteen 7-in., and eight 64-pounder muzzle-loading guns; Amethyst, fourteen 64-pounder shell guns; Huascar, two 9-in. muzzle-loading, and two 40-pounder and one 12-pounder breech-loading Armstrong guns. The armor-plating of the Huascar varied in thickness from 5½ in. at the turret ports to 2 in. at the bow and stern. The following is an extract from the official Peruvian report of the damage sustained by the Huascar during the above action: "The hull—A 300-lb. projectile passing through the armor plating, 3½ in. in thickness, near the side

light of the second sleeping cabin, starboard side exploded and destroyed the bulkhead, injured the tube of the cabin pump &c., besides killing one and wounding three men. Another projectile, of the same caliber, first striking the same side of the ship, and making an indenture of 3 in. in the armor plating. . . . Another, of 150 lbs., striking the same side, injured the armor plating about 1 in. in front of the foremast and 16 in. above the deck. Another shot grazed the forecastle without causing damage. Another, of 150 lbs., penetrated the armor, port side, to the extent of 2 in. . . . Another, of 300 lbs., struck the ironwork of the stern, and, passing to starboard, exploded, doing considerable destruction and wounding a sergeant of marines. . . . Turret—A 300 lb. projectile made a 3-in. indent 3 ft. from the left embrasure. . . . Different kinds of projectiles and fragments of shells destroyed the irons which served for holding the sacks, as also the wooden base on woich they rested. . . ." Nearly a hundred projectiles struck the vessel, principally about the upper works, funnel, masts, boats, &c., all of which were destroyed or seriously damaged; but the 64 pounder shells were useless against even the thinnest portion of the Huascar's armor.

The engagement in July, 1877, between the Russian auxiliary cruiser Vesta and the Turkish armored gun-boat Feth-i-Bulend, may be dismissed without further comment, for although the former vessel was badly mauled, the latter received only one shot through the funnel and another through the mainstay.

Much more serious, however, were the injuries sustained by the Peruvian ironclad Huascar in her celebrated action with the Chillian ironclads, Almirante, Cochrane and Blanco Encalada, off Punta Angamos, on October 8th, 1879. The Cochrane and Blanco Encalado mounted 6 9 in. muzzle-loading Armstrong guns each, and were protected by armor varying in thickness from 4½ in. to 9 in., exclusive of an inner skin of 1¼ in. As these vessels were built from the designs of Sir E. J. Reed, they were, of course, well protected in all vital parts by armor plating. The force of the Peruvian vessel has already been given. At 9.27 A. M. the Cochrane opened fire on the Huascar at a range of about 200 yards, and continued to engage at close quarters for about forty minutes, when the Blanco Encalada came up and joined in the action. The commander of the Cochrane, Capt. Latorre, aware of the superior manœuvring qualities of his ship, as well as of the weak points in the design of his antagonist, kept in the wake of the Huascar, directing his fire chiefly against her unarmored stern and other vulnerable portions of her hull. In the course of the fight the hull, turret, &c., of the Huascar were struck twenty times by heavy shot, ten of which perforated the armor, whilst five glanced off. The remainder of the shots took effect in the unprotected portions of the hull, principally in the stern, destroying the steering gear, and rendering the vessel unmanageable. The 5½-in. turret armor was pierced twice, the 4-in. armor once, the 3-in. armor four times, the 2-in. armor twice, and the 2-in. armor once. The projectiles which perforated the turret armor and partially disabled the guns were fired by the Cochrane at a range of only about twelve yards. A few moments later the Blanco Encalada came up, and, passing within twenty-five yards of the Huascar's stern, discharged a raking broadside into her, which killed or wounded many of her crew. The only damage sustained by the Cochrane was caused by two shells which penetrated the unarmored portion of her hull on the starboard quarter above the water-line armor and wounded ten men, whilst the Blanco Encalado received no injury whatever. Notwithstanding the terrible battering sustained by the Peruvian vessel, her engines at the close of the engagement were in perfect working order, thanks to the protection afforded them by the water-line armor. This circumstance alone is of sufficient importance to justify the demand for the utmost possible protection in our new ironclads. Had the steering gear been equally well protected, the Huascar might perhaps have effected her escape, or have succeeded in ramming the Cochrane during the first stage of the action, in which case her superior speed would have enabled her to outdistance the Blanco Encalada. Unfortunately, however, her steering gear was shot away three times, so that she was unmanageable during the greater

part of the action. Another fact worth attention is that no less than 50 per cent. of the projectiles which struck the armor plating glanced off, though in some instances the plates were only 2½ in. in thickness, whilst every shell which hit the unarmored parts of the hull penetrated into the interior of the vessel, where it exploded. The armor plates of the Huascar appear to have been of a very good quality, for though several were pierced by the heavy Chilian projectiles, not one was "wrecked," as was the case with many of the brittle French plates on the Italian vessels at Lissa.

In conclusion, the chief lessons, as regards armor, taught by the foregoing ironclad engagements may be briefly summarized as follows: (1) The details of construction, and consequently the weak points of every ironclad are known to the enemy. (2) The want of strongly-armored transverse bulkheads led to the destruction of the Palestro at Lissa, and in a great degree to the surrender of the Huascar at Punta Angamos. (3) Armor of the thinnest kind in use affords a considerable amount of protection against oblique fire, and, if penetrated, has a tendency to localize the effect of the explosion of the shell.

It must, of course, be borne in mind that all the vessels referred to above were armored with either hammered or rolled-iron plates, the best of which are about 30 per cent. inferior in resisting power to the modern compound armor, as now employed in the British, German, Russian, and most other navies. It may be observed that the maximum thickness of armor has, for the present, at least, been reached in the case of H. M. S. Inflexible, viz., 24 in., and there is now a tendency among English and French naval architects to reduce the maximum thickness to about 18 in., as demonstrated by the latest designs. There is, however, a great difference in the manner in which the weight so saved has been utilized in the respective navies, for whilst we devote the same chiefly to various arrangements and fittings of secondary importance, the French have strengthened their bulkhead and water line armor.

At present the ends of our partially-protected armor-clads are, in a sense, at the mercy of even the worst naval gun afloat, viz., the British 64-pounder. Considering the enormous size of these vessels, exceeding in some cases 10,000 tons, it is hardly an exorbitant demand to insist on the introduction of a few hundred additional tons of armor along the water line, and at other vital parts.

THE EVOLUTION OF MACHINES.

By Prof. H. S. HELE SHAW.

From the "Journal of the Society of Arts."

If we look back through the history of man, we find that his progress in civilization stands in close relation to, and, in fact, is measured by, his power over the material world around him. When we examine the means by which this power is obtained, it becomes evident that it is limited to the physical operation of changing the relative position of the materials at his disposal. At first, man was content to accomplish his purposes in order to obtain food, or for self defence, with such materials as he found in their natural state; but when a certain measure of progress had been made, and the struggle for a bare existence became less severe, he began to realize the advantages of a previous arrangement before the actual operation in view, and evidenced the possession of intellectual faculties by the design and construction of tools rather than by the mere use of them. It was the power to give materials definite form, and to effect combinations with which desired operations could be performed, that enabled further progress to be made, and so, from step to step, flint implements replacing those of bone, bronze and the easily worked metals those of flint, culminating at length in the employment of iron, the most difficult to work, but most valuable of all; man advanced slowly at first, but after a while at an increased rate, to-

wards the present mighty achievement in the industrial arts, the comprehension of only a small portion of which now demands the study of a lifetime.

For a long period man, to supply all his wants, used only such simple appliances as would, without hesitation, be called implements or tools. Gradually, however, attempts were made, with ever-increasing success, to obtain the desired result with less labor and more certainty by the construction of what might, even in their simple state, be truly called machines. Bearing in mind man's only physical mode of producing any change in the world around him, it is not hard to understand why the great extension of power and capability of making further progress may be traced to the development of machines. The relation of machine development to civilization is a subject of the greatest interest, but of vast range, and scarcely less extensive is the history of machine development itself; but they might be well brought directly, as they have so often been indirectly, before this Society, which was founded for the encouragement of arts, manufactures, and commerce. Neither of these, however, forms the direct object of this paper, but one which, it has been said, the Society has ever steadily kept in view, viz., "the application of science to practical purposes." In order to study the application of science to the development of machines, the past must be examined, so that the course of development may be seen; and though it is obviously impossible to follow in detail the growth of machinery, a few examples may be selected which will be sufficient to illustrate the fact that the growth has taken place on certain well-defined principles. It will afterwards be possible to see more clearly what is the present state of this branch of science, not only with reference to the other sciences, but also to the practical requirements of future machine development.

The history of the early machines is lost in antiquity, but it has been shown that there are strong grounds for considering the fire drill or twirling stick, first revolved between the hands of one or two operators, as one of the earliest examples of machinal motion, and that a long period must have elapsed before the introduction of continuous, instead of alternating, rotary motion. It is extremely probable that the first continuous motion was employed in connection with the grinding of corn. The use of a simple stone to pound the wheat, was followed by the use of the pestle and mortar, which, as Beckmann, in his "History of Inventions," has remarked, was probably the kind of mill possessed by every family, which Moses forbade to be taken in pawn, as being, for obvious reasons, the same thing as to take a man's life in pledge. This early mill was first worked by a female slave, then by bondsmen, and afterwards by cattle; and though at first, no doubt, only a heavy kind of pestle was used, it became evident that the end would be better and sooner accomplished if the flat cylindrical stone, with a vertical spindle, were employed, and thus arose a true example of a machine. It is evidently necessary at this point that as clear an idea as possible should be obtained of what constitutes a machine, and, therefore, without attempting to add another to the many definitions already existing, we may consider a machine to be a combination of materials arranged by man, so as to enable determinate motions to be obtained. Possibly, long before the corn mill had been advanced to the form which might entitle it to be regarded as a machine, there were many simple machines for other purposes, such as for drawing water, preparing clothing, and for agricultural purposes. These were actuated by the muscular effort of men or animals; and though the employment of the latter evinced considerable progress, it was a far greater and more important step when the forces of nature, other than muscular, were first turned to do work in machines. No doubt, the power of flowing water was the first so used, though whether for irrigation or with corn mills, or for any other purpose, it is impossible to say. Beckmann considers that corn mills were first introduced in the time of Mithridates, Julius Cæsar, and Cicero, and states that a floating mill was employed by Belisarius on the Tiber, in the year 536; but the Chinese used water-wheels for the purposes of irrigation at a very early date. The use of the more uncertain and refractory element, wind, for motive power, showed a still further advance, and though difficult to fix the exact date of the first

windmill, it has been shown by Beckmann that it is very improbable that the Romans knew of them, as the first authentic record does not occur in the classics, but in the account of a mill in France, as late as the year 1105, one in this country being mentioned in the year 1143. The greatest difficulties of all had to be overcome in bringing into direct application the molecular forces to actuate machines, but at the same time this step has been by far the most productive of results. The invention of the steam engine may truly be said to mark a new era of progress, for it has given to man the direction of almost unbounded, and at the same time perfectly controllable, machine power. On account of its importance, and also because its history is better known than that of most other machines, no better example could be chosen to illustrate the manner of machine development.

The first proposal to use the expansive force of steam was made by Hero of Alexandria, more than 2,000 years ago, but for several hundred years after, not even a reference appears to have been made to the subject. At lenglh, however, with the revival of learning, attention was directed to the properties of steam by one philosopher and another. In the 16th century, Cardan mentions the vacuum formed by the condensation of steam; and there are on record the suggestions, more or less vague, of Matthesius, Besson, Ramelli, Leonardi da Vinci, Porta, Solomon de Caus, Branca, Wilkins, and others, all belonging to what Professor Thurston, referring to the steam engine, has called the age of speculation; but the time was not ripe for practical results, and neither knowledge of scientific principles nor of the use of materials was sufficiently advanced to enable an application on more than the smallest scale, and in more than a tentative manner, to be made of these proposals. It is not till about two centuries ago that the period of application is reached, and then we find that science had progressed, the art of working iron was well established, and many machines existed. The inventor who then took up the subject and carried it to a point far beyond what it had hitherto reached, was the second Marquis of Worcester, who, in addition to the advantages already mentioned, which time and workers in other directions had brought him, possessed both wealth and influence, and was a thoughtful and studious man. This nobleman set to work upon the problem, which he prosecuted with the greatest ardor to the end of his life, and of the ultimate success of which he entertained the most exalted opinions. Yet, in spite of all this, he died poor and unsuccessful. His "water-commanding" engine, as he called his steam pump, was neither yet demanded with sufficient emphasis, nor was the practice of working materials yet sufficiently advanced to make his trial at Raglan Castle, and later still, at Vauxhall, in London, more than a partial success. Still the problem of steam power was thus brought prominently forward, and Sir Samuel Morland published, soon after, a table of volumes and corresponding pressures of steam, which, considering the date (1683), was a remarkably close approach to the best results hitherto attained. Time went on, and the miners in Cornwall were suffering from the water in their shafts; so, again, in the west of England, we find another inventor working at the question of steam. This inventor, Thomas Savery, of Devonshire, was also an educated man, being a military engineer, and it is probable that he was well acquainted with the work of the Marquis of Worcester. This is to be inferred, not only from certain statements published at the time but from the fact that his engine bears considerable resemblance to that of Worcester, at any rate as far as can be made out from marks on the walls at Raglan Castle, and the mystified description of it which was published. Now, it must be borne in mind that both these engines applied the expansive force of steam treated of by Hero, with the principle of condensation mentioned by Cardan, for forcing water in the way suggested by Porta and Solomon de Caus, but had increased the complexity of the apparatus proposed by De Caus, but the addition of a separate vessel in which the forcing was performed, and by sundry valves, which, however, greatly increased the efficiency of the engine, and, in fact, rendered it a practicable machine. Savery's engine worked to a certain extent satisfactorily, but a limit to its powers was soon reached, simply from the

imperfect materials at his command. This is made clear from the comically serious account of Desaguliers of the expenses Mr. Savery was at because he had to use hard solder instead of soft, as the latter would not withstand the heat or pressure at the joints of the boilers. It is a significant fact that one of the most successful of recent inventions, viz., the Pulsometer, is nothing more than an automatic Savery engine, which would be as useless as the engine of Savery, were it not for the present increased knowledge of the nature and properties of materials. To Denys Papin, a distinguished Frenchman, belongs the honor of suggesting, in 1690, the use of the cylinder and piston, with which the pressure of the atmosphere could be utilized when the steam was condensed inside. That he afterwards went back from this, and endeavored so use the inferior design of Savery, in order to obtain continuous motive power by raising water for a water-wheel, does not, as it has been asserted, testify to ignorance of its superiority, and cause him to forfeit the credit for its invention. He himself said that "the principal difficulty is that of making these large cylinders," and his attempt to overcome this difficulty is proved by the present existence of a large cylinder of his construction in a court of the museum at Cassel, in Germany. The next inventor moved forward a great step, and this step was the practical application of the steam cylinder of Papin. Here, again, the west of England, where previous inventors had worked, and where the steam engine was urgently needed, supplied the inventor, in the person of Thomas Newcomen, blacksmith and ironmonger, of Dartmouth, who was assisted by John Cawley, a glazier. The former, living but fifteen miles from Savery, was probably well acquainted with his work, even if he had not been actually employed upon a portion of it, and had certainly the advantage of knowing Papin's proposal, as is evidenced by his correspondence with Dr. Hooke. The engine, under Newcomen, now assumed the form of a train of mechanism more complex, but far more efficient, and down to a certain limit, the pits were cleared of water. An accident led to the use of internal injection, and consequent increase of power in the engine. The ingenuity of a youthful attendant some time after led to its conversion into a self-acting motor, which only occurred when the boy, Humphrey Potter, to save himself the trouble of working the valves by hand devised an arrangement of strings and catches to perform this operation automatically. Beighton replaced the latter arrangement by a beam with sundry levers and tappets, and Smeaton still further improved, enlarged, and at the same time complicated the engine. It was, however, to James Watt that the greatest advance was due. The inventor began his study of the subject in 1763, at the point to which it had been brought by previous inventors, as he was led to examine the defects of the steam engine from a model of that of Newcomen at the Glasgow University. His first and greatest contribution to the problem was the separate condenser; but it is only possible to sum up all his work, and it may be said that Watt found the steam engine single-acting, and merely capable of exerting a force in one direction. He left it double-acting, capable of giving continuous rotary motion, self-regulated, vastly more economical and reliable. To attain all this, he, however, had to invent and apply much more complicated valve arrangements, packing for glands, parallel motion, governors, separate condenser, hot-well air-pump, crank, or its substitute, the sun and planet motion, and fly-wheel, not to mention a great number of mechanical details, such as extra bearings, connections, &c., for carrying these additions into effect, and for constraining the motion of the various parts, so as to make the action more certain and reliable. In reading the life of Watt, it is evident that his greatest difficulties were not in conceiving ideas, but in executing them, and that the struggle to find material and workmen represent largely his efforts during the interval of twenty years which elapsed before success rewarded his efforts. It is now just 100 years ago since Watt took out the patent for his rotative engine; and, excepting in one or two points, no improvement which has resulted in simplification can be said to have taken place. Improved machine tools have enabled the more complex parallel motion to be replaced by the guide bars and crosshead of the ordi-

nary horizontal or vertical engine; but if the condenser is often, for practical reasons, removed, this necessitates a loss of efficiency as a heat engine. On the other hand, neatness of design must not be mistaken for simplification of parts, and the compact and apparently simple modern engine may yet have reversing gear expansion valves, automatic lubricators, and a variety of appliances, which render it really more complex than the old beam engine of Watt. But to obtain an idea of the direction which progress has really taken, take the case of the engines of a first-class Atlantic steamer. The details in the annexed table of the number of parts of such have been furnished to the author through the courtesy of the builders.

TABLE SHOWING THE NUMBER OF PARTS IN THE ENGINES AND BOILERS OF A FIRST-CLASS ATLANTIC STEAMER.

Jam nuts	238
Split pins	400
Levers	87
Guard rings	108
Pins	1,144
Moving parts	100
Total number of pieces in engines	6,000
Auxiliary engines	23
Steam pipes	271
Pumping-out arrangement	172
Valves	147
Gauges	9
Lubricators, impermeators	147
Bolts	7,868
Studs	3,000
Nuts	10,407
Rivets	64,888
Boiler tubes	2,270
Condenser tubes	4,456
Boiler stays	1,582
Furnace bars	1,356
Furnaces	24

Perhaps one of the most significant items is that of the twenty-three auxiliary engines, each a separate self-regulating, self-contained, motor, supplied simply to work separate portions which, at first, used to be worked by the main engines or by hand. Consider the 764 parts made up of jam nuts, split pins, and guard rings, placed solely for extra security, not to say the 1,144 pins, many of which are for this purpose; and lastly, the enormous total of which appears to amount to 104,642 parts, each requiring separate construction, fitting and securing, and, truly, it will be said that progress does not take place in the direction of simplicity. But if the visitor is led to turn from the difficulty of even understanding this complex system, to the thought of what a marvelous achievement the design of such a machine must be, perhaps, what strikes him even more than its complexity is the perfect interdependence of the parts, and the extraordinary ease with which it is all controlled, and, in short, the wonderful unity of the machine as a whole.

The progress of the invention of the most important class of machines, viz., prime movers, has now been briefly traced, and it will be found that there are certain salient features in their development common to that of all machines.

The first of these seems to be that progress does not, as a rule, take place in the direction of simplification, but rather from simple to complex forms, accompanied by more definite, reliable, and constrained machinal motions. In proportion as the machine becomes capable of performing more extended operations, and accomplishing them by itself, requiring less human intervention—that is, becomes more automatic and self-regulating—so the parts multiply, and complexity increases. This, Professor Kennedy, in his translation of Reuleaux's work, has called an extensive and intensive growth; extensive, in decreased range of operation; and intensive, in increased internal power of action. This tendency is certainly that which is visible in past progress, and this replacing of manual effort and intervention still goes on, and is, for instance, illustrated by the enormous increase in the number of small auxiliary motors.

In the next place, it is seen that there is a conspicuous dependence upon the growth of the other arts and sciences, especially upon the knowledge of the use of materials. Again, it is clear that machines are not the product of one man's brain, but of the successive labors of many minds, and are a growth rather than a sudden creation. That this is true, even of ideas, is illustrated by the fact that nearly every successful inventor in the case of the steam engine seems to have had the benefit of knowing not only the successful, but often also the unsuccessful, attempts of previous workers, and to have even then required considerable time and labor to project further advance.

In addition to these points, there is another which comes out more clearly when a study of the lives of the inventors themselves is made, and this is that the course of invention has not been by any means continuous, but that often a retrograde movement has taken place. Machines have not only been unsuccessful because proper materials have not been forthcoming, but also because attempts at improvement have been made upon wrong principles. A striking example of this is furnished by the immense number of unsuccessful attempts to make a rotary steam engine, towards which end inventors have been (and are) apparently urged by false views of the value of simplicity in a machine. This is not only a procedure in a false direction, on purely machinal grounds which have been explained and exposed by Reuleaux, but also appears to be opposed on certain general principles to the best method of employing steam as a working agent, and the only rotary engine which at present promises to be a success has elaborate arrangements for packing purposes, rendering it anything but simple in construction.

The truth of the foregoing conclusions becomes increasingly evident upon a review of the history of other machines, whether for weapons or tools, for preparing food or clothing, for the purpose of measurement, or the communication of ideas. Only one example in each of these important classes of machines can be referred to, and that only in a word or two.

Of weapons, the most important is the fire-arm, and of this the forerunner is seen in the cross-bow, which replaced, with more perfect guiding arrangements, the simple bow and arrow. The elastic force of the bow-string was, after long use, replaced by that of the heated gases from the exploded powder; but the old matchlock was, as its name implies, a crude weapon compared with the flintlock, and the latter was vastly more simple than the musket with percussion cap; then came the early Enfield rifle, followed by the breech-loading Snider and Martini, each more complex than its predecessor, but still simple compared with the Gattling, the earliest machine gun, which was first brought forward during the American war; this was followed by the Hotchkiss, the Lowell, the Nordenfelt, and Gardner. It was reserved for Mr. Hiram Maxim, of London, to conceive and quite recently to carry into execution a weapon which is a marvel of automatic action, and illustrates in a remarkable manner the tendency towards that intensive development of machines already referred to. This gun actually performs all the functions of carrying the cartridge to its place, of locking it in, cocking, firing, withdrawing the shell of the cartridge, and ejecting it, and this not by the force of human action, but simply by the force of its own recoil, which has only hitherto produced an injurious shock upon the supporting frame. Yet the external appearance of this gun is remarkably simple, and it has but one barrel; but if anyone thinks its construction is really a step in the direction of simplicity, let them go through the process of understanding its action from written description, aided merely with diagrams. Yet, in the history of guns, the progress has not always been in the forward direction; the big breech-loading guns first introduced were, after a while discarded, and a return made to muzzle-loaders; but this was simply because of the difficulties of safe and reliable construction, a matter which the progress of knowledge has in time enabled to be satisfactorily accomplished.

In the case of tools, the early hand tools preceded the primitive lathe, in which, when two operators were not required, the two hands of the worker were supplemented by the assistance of one foot, his big toe being also brought into requisition as he sat on the ground in front of his work. The lathe, with a simple hand-rest, was the factotum of the early machinist; by its means his boring, turning, and drilling was at first done, but by degrees the slide rest and screw-cutting gear, speed cones and surfacing arrangements, pulleys, and various other appliances, were gradually introduced. The increased complexity is not only seen in the particular machine in question, but in the variety of special tools derived from it, such as meet the eye of the visitor in vast array in such a place as, for instance, the machine department of the locomotive works at Swindon. There are seen

special turning, boring. surfacing, drilling, screwing, facing, polishing, grinding, centering, and other machines, all to do the special work of the early lathe, and of many of these numerous varieties exist, often differing apparently in external appearance. These are only a small portion of metal-working machines, and equally striking examples of tools for working wood and other materials might be taken. Special tools, like the steam hammer, have a history which testifies to the nature and mode of progress, not less than the lives of Maudslay, Whitworth, Nasmyth, and other great tool makers. The progress of the hammer, from the first stone weapon to the tilt hammer, and then to the great step of the steam hammer by Nasmyth, which no one would take up in this country even after its application, at first unknown to him, at the Creusot Works in France, because "it was not likely to be required," its improvement by Condie, and the limit of its powers, not by any inability to construct still larger forms, but for reasons which render hydraulic pressure upon huge masses of metal a superior kind of action, all of this being in accordance with the views previously stated.

In connection with machines for supplying and preparing food, there might be taken the whole class of agricultural machinery, besides a large number of special machines for various purposes. The early history of corn mills has already been referred to, and in the gradually increasing number of appliances which were introduced into the water-mill, increasing, to a certain extent with the introduction of the windmill, and in a still greater degree with the modern steam flour mill, might be found undeniable increase of complexity. But if the system of low grinding with the simple circular stone has itself grown more complex, what might be said of the system of high grinding (invented to deal with the hard grains) as practiced in Austria and America, where the flour is, by a complex series of mechanical processes, divided into as many as ten or twelve different qualities, or of the various refinement for cleaning and separating the grain for grinding, for sifting or dressing, and lastly, for purifying. A high authority on the subject, Mr. Proctor Baker, says, "One rule must predominate over all other considerations, viz., that the material in the process of manufacture must not require to be moved by hand labor, at any stage, from its reception into the mill until it is finally packed in the bags in which it is to be delivered to the consumer." But, in this case, many millers have found, to their cost, that there is a very close connection between the machine and the work required from it, and that costly and expensive plant does not ensure the best results, if the correct requirements of the case are overlooked. The tendency is to make the process more independent of external intervention from manual sources.

Coming now to the important class of machinery employed in connection with clothing, the history of machines for working either cotton, wool, hemp or flax, all tell the same tale of gradual improvement and more intensive forms of construction; but instead of any of these, which have really become systems of machines, a single machine of more modern introduction may be taken. The sewing machine is universally known and employed, and is commonly supposed to have been invented, perfected, and brought into general use almost at once.

This is, however, very far from the case, and a writer in Knight's "Dictionary of Mechanics" thus describes its invention:

"The earliest machine first used the needle and needleful of thread in making a running stitch. Then the eye was placed in the middle of the needle, which was sharpened at both ends, to save turning it about when returning it, the needle being pushed and drawn by steel fingers on each side of the goods. The invention was yet an implicit copying of human manipulation, and the next step merely shifted the mode from the stitch of the seamstress to that of the tambour worker. The needle was passed through the goods and returned, leaving a loop, which was to be detained so as to be entered by the needle at its next descent, leaving another loop, and so on. A modification is mentioned of a crotchet hook; . . . . but a man of mark will find a new departure. He must devise new modes of procedure adapted to the needs of the new steel man who is

automatic but unskillful, and one of whose principal requirements is continuity of motion. . . . . The new elements were not invented all at once. One of the most important was overlooked for fifty years after it had been patented. Another was invented, made, and exhibited, and then slept a profound sleep of twelve years; another was invented and patented, but was in a useless shape, and lay dormant until really valuable inventions were made, when it arose and claimed them as mere adaptations. There is no important machine for sewing fabrics now manufactured that does not use all of the three elements mentioned—the continuous thread, the eye-pointed needle, and the continuous feed; but the former two of these had been in existence for sixty and twenty years respectively before they were united to the latter, which, coming in the fruition of time, was more quickly recognized as a necessity."

Yet, curiously enough, this clear account is prefaced by the following words:

"The growth of invention is in the direction of simplicity; but it is necessary, in the first place, to conceive needs, and then to follow a host of temporary expedients—mere patchwork, as it afterwards appears. In the course of time rises a reorganizer, who proposes to devise means adequate to meet the changed conditions which supervene, when a machine is called upon to take the place of the human operator."

Now, from what was before quoted, it is clear that the sewing machine really became more complete in proportion as it became more efficient; and indeed, the last lines show that the writer, to a certain extent, recognizes this fact; so that this is an evident case of the common error of confusing the idea of apparent simplicity from improved external design, with the real simplicity of essential parts.

Time does not permit of any account to be given of the growth of that typical instrument of measurement, the clock, even one stage of which, viz., the invention of the chronometer by John Harrison, the history of which has been recently so ably told by Dr. Smiles in his "Invention and Industry," is so full of instruction. Neither can machines for the communication of ideas be more than briefly noticed by a reference to the also typical printing machine. Blocks of burnt clay, stone and wood, had been used from the earliest times, and from the handles upon those in the British Museum, which were in use by the Romans before the Christian era, the mode of operation is evident. It is not very clear when the first printing press was invented, but the early screw press was evidently in use for a long time prior to the year 1620, when Blaew, of Amsterdam, introduced the traveling bed, the lever-screw for depressing the platten, and the spring for raising it. Only slight modifications were made until the invention by Lord Stanhope, at the end of the 18th century, of the toggle joint press. In 1790, Nicholson invented the first cylinder machine; in 1811, Koenig (whose history is also as well told by Dr. Smiles) set the first steam-printing press to work, and the *Times* of November the 29th, 1814, was the first instance of a newspaper printed by steam. The course of invention has since then moved onwards, and anyone who has watched an edition of the *Times* being printed off will be quite prepared to admit with, perhaps, as much emphasis, the marvelous definiteness and certainty of action of the present machine, and they will also admit the fact that simplicity is not the most striking and obvious feature.

The foregoing examples will suffice to make it clear that machine development has followed the path of progress from simplicity to complexity, and at the same time from indefiniteness and uncertainty of results to definiteness and certainty; also that this has been accompanied by the tendency to multiply different special parts, or even separate special machines, as in a machine workshop or mill, but, at the same time, to render the machine or machine system more and more one compact and integral whole. But this is exactly following what is known as the law of evolution. This law, long ago applied by Herbert Spencer in his first principles to social progress generally, is defined by him thus: "*Evolution is a change from an indefinite incoherent homogeneity to a definite coherent heterogeneity, through continuous differentiations and integrations.*" Perhaps

the thought may occur to someone present that evolution might with profit hold with regard to this mode of statement of the law itself, but it is evident that its application to machines is fully justified. But the law is borne out in details, notably by the fact that progress, as has been shown in several cases, has not been continuous and uninterrupted, for that some time a retrograde movement has taken place, and sometimes the environment, in other words, the state of other arts and sciences, has required modification for any further change at all to be made. Thus again may be applied to machine development Herbert Spencer's statement in reference to progress generally, that "throughout that re-arrangement of parts which constitutes evolution, we must nowhere expect to see the change from one position to another affected by continuous movement in the same direction. We shall everywhere find a periodicity of action and re-action, backward and forward motion, of which progress is a differential result." Hence there appears to be abundant grounds for taking, as the expression of an important truth, the idea conveyed by the title "The Evolution of Machines."

All that has been hitherto done in this paper is to consider the actual growth of the machine itself, and the ideas which have guided inventors have been only alluded to incidentally. It is, however, the latter subject, viz., the line of thought followed by the inventor, which really forms the object of our investigation and to which the study of the growth of machines was only a preliminary but necessary step.

The points concerning the inventor which appear to have been brought out, are:

1. The increasing complexity of the machines which it is his task to improve upon, and the already highly developed state in which he finds them. This fact necessitates careful preliminary study of the subject if—as was, for instance, the case of James Watt—he would start level with the age in which he lives, and from that stage of development to which other workers had brought the problem he essays to carry to a still more complete solution.

2. The successful inventor is not, as a rule, the entirely original genius commonly supposed, but has depended upon others for many suggestions and experimental results.

3. That however sound may be the ideas of an inventor, he depends both upon the progress of the other arts for its successful carrying out, and upon the demands of the age for its successful introduction and application.

It will be well to consider briefly the modes of thought and precedure of inventors, and without pretending to do more than mention typical cases, it may be said that there is, first, the man who, knowing little of what has previously been done, upon conceiving an idea, at once endeavors to put it into a practical form with such rude materials as he may be able to command, and after many failures, even supposing that he succeeds, probably finds that his idea is either of no practical value or has been invented many times before. Next there is the man who, with little experience or knowledge of principles, yet has the wisdom to spend time in a vigorous preliminary effort to ascertain what has already been done, and the effort will need to be vigorous if we search the Patent-office records in their present state; but even though ill-equipped with appliances, he at any rate starts with a knowledge of previous attempts, and does not waste time in re-invention. Lastly, there is the man who, either trained in science, or, what in the present case amounts to the same thing, has correct views of the principles which concern his end, starts with a full knowledge of the special machine, as far as it has hitherto been developed, and who has command, either by means of good workmen or by his own personal knowledge and skill, of the best appliances of the existing state of the arts. Though it has been said that great inventors are born and not made, yet, however naturally gifted the inventor of the first class may be, it will take a long and weary struggle to give him anything like a chance of doing what one of the last class may at once accomplish. In spite, however, of this, it will be found that the difference between these invetnors, however great it may appear, is really one of degree, and that the general mode by which the result has been arrived at is, as far as it can be discovered, practically the same. Though the last may

have used one or more known sciences, it was, to a certain extent, by a process unknown to himself that even he arrived at the result. It was more or less a groping in the dark—in one case it was almost all darkness—but on some of the steps in the other the light of knowledge was flashed. Thus, as Reuleaux has pointed out, there arises that reverence for the inventor which is greatly due to the fact that he has succeeded by a process which is unintelligible even to himself. And what has been said of inventors generally may, with equal truth, be said of the every-day practice in a drawing office, of designing machines, even with only slight variation to previous forms. But this is clearly not science but art, the art partly natural, partly acquired, of putting together knowledge, and of applying the results of science. It may, therefore, not unnaturally be asked—Why is not a distinct science of machines directly applied to the problem to be solved?

In order to answer this question, let us briefly glance at the history of the study of machines, and particularly at its present state. It was not to be expected that a science of machines would be originated before machines had themselves reached a certain stage of development; but that after it had been originated it would itself follow, as other sciences have done, the law of evolution. Such has been the case with the study of machines, which at first was merely descriptive; but as machines multiplied, some classification became necessary, and attempts were made to effect this. Reuleaux, in his sketch of science, considers Leupold to be the first to separate (in 1724) single mechanisms, but that nothing like a system was formed until quite the end of the 18th century; he proceeds to review the various attempts to found a science of machines upon fundamental principles, but it must here suffice to say that though each worker has taken a part in the progress of science, yet no one succeeded in founding a system which has had a practical application, and we therefore come at once to the system of Professor Reuleaux himself.

Rather more than ten years ago, the "Theoretische Kinematik" of Reuleaux was published in Germany, and about two years after was translated into English, and edited by Professor A. B. W. Kennedy, who, for excellent reasons, adopted for it the title of "The Kinematics of Machinery." The kinematics of machinery may be considered as the science of the motion of constrained bodies, regarded specially from the machinal point of view. Bearing in mind what was said at the commencement of the lecture, no arguments are needed to show why this must, to a great extent, be regarded as the science of machines; for it is clear that if the only physical mode of operation by which man can act is to effect a change in the relative position of matter, this not only shows, as there explained, the reason of the importance of machines, but also points to the science of machine motions as the branch of greatest importance. This it does by making it evident that the object of a machine is to effect a desired definite movement or motion of some portion of matter. The practical bearing of this consideration is forcibly illustrated by Reuleaux's ultimate division of all machinal arrangements into place-changing machines, as, for instance, locomotives, cranes, clocks, &c.; and form-changing machines, as, for instance, corn-rolling—or saw-mills, lathes, paper-bag machines, &c.

The deep and far-reaching principle upon which the system of Reuleaux is based, is that of the mutual contact of the moving parts of a machine. In all cases these parts must have the property of mutual envelopment, if their relative motion is to be constrained and definite; and this correspondence or pairing, which is an essential feature of the machine, leads to the statement that a machine consists solely of bodies which thus correspond pairwise reciprocally. Pairs join together links, and links form a chain, and a chain is a machine. It is by the combination of chains that any and every machine, no matter how complex, is formed. This method of viewing the machine problem leads to a matter of the greatest importance, viz., the construction of a "notation" for machines upon scientific principles. Previous attempts to introduce symbolic representation of machine parts had only been partially successful, even for trains of wheelwork, while for general purposes they had been eminently unsatisfactory. The

value of Reuleaux's system is shown by the concise way in which it represents the remarkable results of his analysis of machines, and puts the otherwise impossible task of understanding at any rate the leading features of all the various machines, within measurable distance of being grasped by the human intellect. But more than this is done. After analyzing a large number of machines, Reuleaux concludes by what must be regarded as the first effort to found a system of kinematic synthesis, or building up of machines. The problem of kinematic synthesis is divided by him into direct and indirect, each of these divisions being again subdivided into general and special. General direct synthesis should give immediately the mechanisms to effect a given place or form change; but it is shown, from practical reasons, that useful results from this cannot be expected. Special direct synthesis would furnish a pair of elements suited to effect the above result, but neither can this method give results of practical value.

General indirect synthesis should give beforehand the solution of all machine kinematic problems in advance. This is obviously an enormous requirement; but it is shown that there are six principal chains or mechanisms upon which most machines are based, and these Reuleaux examined. Special indirect synthesis, however, does lead to really practical results, and the whole possible number of pairs are determined and arranged in twenty-one orders. The author of this system does not, however, recommend the special study of this branch of the subject, but rather that machines should be treated under applied kinematics generally, and arranged according to their practical application. Indeed, beautiful and interesting as this part of the subject is, it must be said that it appears to be the least satisfactory portion of Reuleaux's great work, and this, with our present object in view, is certainly disappointing. Perhaps the reason for this is to be found in the fact that the motion to be obtained, though it truly forms the practical object be sought, and therefore makes the branch of the subject already considered the most important, can, after all, only be carried into actual operation by practical means. In short, the definite and constrained motions sought after can only be obtained by employing suitable materials of the necessary strength. The progress of knowledge concerning materials has already been shown to have always borne an intimate relation to the progress of machines, and obviously upon the scientific application of this knowledge must further advance depend. Reuleaux, it is true, introduces the idea not merely of rigid solid bodies and flexible solid bodies (as belts, cords, &c.), as Willis had done, but also fully considers, from a kinematic point of view, the machinal properties of liquids and gases, and one of the most important and interesting parts of his work deals with this subject. This is a great advance, but still there is an immense field untouched by this system, as, for instance, the nature of the best forms to be given to the links, or, more important still, the actual effects from the contact between different materials, the nature of which very often entirely decides the fate of a new machine. On these points our knowledge is yet far from complete. Thus take the modification of the time-honored laws of friction when applied to lubricants; resulting from the recent experiments by the Committee of the Institution of Mechanical Engineers, or consider how much is even now known of rolling friction, and its exact effect on frictional contact. Knowledge of materials is increasing with the increasing use of testing machines of all kinds, but unless the unlikely event happens of very great discoveries in the preparation of materials continuing to be made, some more compact and final arrangement of the present knowledge in connection with the science of machine development should be possible. In addition to mere motion and the question of materials, the scientific application of the principles upon which the forces of nature can be made to overcome the resistance to the required motions in the machine, have also frequently to be considered by the inventor. These three branches of knowledge now considered, viz., motion in machines, the nature and strength of materials, and the forces which are required, are studied in this country under the heads of Mechanism, Machine Design, and Prime Movers, and collectively represent the present state

of the science of machines. It must, therefore, be admitted that at present no definite synthetical science exists which can be directly applied to the "evolution of machines."

When the difficulties in the way of such a science are realized, it may, perhaps, be doubted if its ultimate evolution be possible. Such a doubt might, not many years ago, have been well felt about chemistry, and what chemist would have been led by the highest flight of imagination to anticipate a tithe of the results which have ensued from the introduction of chemical notation, applied to carefully collected facts. Organic chemistry, before that event, had not an existence, and, to say nothing of inorganic chemistry, what human brain could carry all the facts necessary for further investigation, if a descriptive notation did not render classification upon scientific principles possible? But, beyond this, the most important results have been in the direction of synthesis; and no one would venture to assert that the building up of such complex bodies as indigo, benzene, or even of acetic acid, would, in all probability, ever have been made without the aid of a scientific notation. A complete synthetical science of machines appears, however, to present, in some respects, greater difficulties than even those in the corresponding branch of chemistry. Without attempting to institute too close a comparison, it may be said that in the latter, the object in view is the building-up of a body, of which the parts are known by analysis, but not their grouping or arrangement; the nature of these groups, and the mode of re-forming them, have to be determined by careful reasoning and experiment. In the machine problem, a result has to be obtained, the necessary parts required for which, as well as their grouping or arrangement, have to be determined. And this we have, in addition to mere synthesis—the selection and determination of these parts in the form of pairs and links, as well as their combination into definite groups or arrangements, in order to form complete chains, or series of chains, necessary for the constitution of the complete machine. But, beyond even this, there is the selection and preparation of suitable materials with which to construct these parts, and this has been shown to form in itself a matter of no little importance. In spite of these difficulties, there appears to be no reason why, at any rate, a great advance should not be made on the present methods of machine evolution, for it must be pointed out that, in one respect, the machine problem is vastly more simple than the chemical, in that we can combine at will machine parts, and understand their mode of combination; whereas we have no certain physical conception of the nature of the molecular combinations which result in the production of such different external forms. Even in its present state, however, much might be done by a more systematic teaching in this country of the science of machines as it at present exists. At this point, however, the vast results already attained in the direction of machine development may occur to you, and at the same time, the nature of the methods—or, rather, want of methods—of previous workers; and the doubt may arise as to the need of such an arduous search for a more scientific treatment of the question, and such a careful preliminary study of the subject. It will, therefore, be well, in conclusion, to briefly consider this question.

The respectful admiration with which a successful inventor is regarded has been already alluded to, and one, indeed perhaps the chief reason, has been mentioned; but there is another, and perhaps equally significant one, which is that the lot of the large majority of inventors is simply failure. The statistics of the Patent-office furnish striking testimony to the testimony of this. The diagram on the wall represents the curves obtained by plotting the number of applications in each year for patents, since the important act of 1852. The years are given along the horizontal scale, and the number of patents applied for and proceeded with for 3, 7 and 14 years, are shown by the corresponding height on the vertical scale, so as to give the different curves. The curves are partly taken from the Commissioners' Report of 1880, and filled in, as far as possible, up to the present date from data kindly supplied by Mr. Lloyd Wise. The point to be first observed is the large number of applications not proceeded with, and the great drop in the number

carried on after three years; lastly, how small a portion survive the seven years' period. Now, the greater number of these patents are for machines or mechanical devices, and it may be asked, why all this failure? which, however, large as it is, only illustrates a small proportion of the non-success of workers at new and improved machines. Some small portion of it is, no doubt, due to want of funds to carry on the work, but chiefly it arises from either a discovery of want of novelty, or of impracticability in successful execution of working, or of improved methods of carrying the idea into operation; that is to say, from (1) ignorance of previous achievements, (2) or of principles, (3) or of practical details.

1. Every day makes it harder to keep abreast with the multiplication of machines; thus, take one book, and that not the largest on the subject, viz., Knight's "Dictionary of Mechanics," in which are nearly 4,000 pages and 10,000 engravings of inventions which are, or have been, in actual use, but this does not represent a tithe of those essayed and found to fail. Nearly a century ago, Beckmann, writing of the invention of a machine for sowing seed, remarked, "The case with Laua (the inventor) was the same as with many ingenious men who possess great powers of invention, as they never read, but only think, they are unacquainted with what others have done before them, and therefore consider every idea which comes into their minds as a new one;" and thus, as Sir John Hawkshaw, in his address as President of the British Association, remarked, "Many a patient investigator has puzzled his brain in trying to solve a problem which had yielded to a more fortunate laborer in the field some centuries before." But if Beckmann's saying had force in his day, when the number of machines was so few, what should be the reading necessary at the present time? The truth is, that in the present state of the literature of the subject, the reading necessary to determine what has been previously done, in the case of any one special class of machines, or very often of a single machine, is well nigh appalling, and anyone, for instance, who had attempted a weary search among the patent records, would testify to the urgent need of a more scientific treatment and classification. The work of indexing at present being carried on in our Patent-office will prove a great boon to inventors, but a great deal more might result if that classification could be made on really fundamental principles, such as have been proposed and fruitfully applied by Reuleaux.

2. Ignorance of principles is another cause of failure. Correct principles are not by any means of necessity intuitively grasped, an instance of which fact is given in the case of George Stephenson, than whom a clearer-headed man or better mechanic, perhaps, never lived, but who, at one period of his life, spent no little time in the attempt to make a perpetual motion machine. Even those principles which, as the result of long experience, have been handed down in the form of apparently simple laws, require long and careful study for their realization. How often does an inventor, possessed by one idea, go round and round in a circle in the endeavor to carry it into practice, and, like a mouse in a cage, return, after a cycle of attempts, to the original designs, sometimes quite unconscious of the fact, and sometimes with a faint hope that he has not yet tried it in quite the correct way. This may lead, and sometimes does lead, to discovery and a successful invention, but is far more often the mere pursuit after an *ignis fatuus* in the shape of an impracticable scheme. A better acquaintance with principles might prevent this, and it needs no argument to prove the advantage to the inventor of being able to clearly realize the main principles upon which he intends to construct a machine, such as the employment of a given chain, and the mere variations in the mode of carrying it out by the alteration, such as by inversion or expansion, of certain pairs of elements of which it is formed.

3. Again, there is an ignorance of practical details, as, for instance, of the most suitable materials and mode of employing them. The inventor on these points often calls to his aid the practical man, and, if his idea is correct in principle, he may thus be enabled to carry it into execution. But there is an unsatisfactory aspect to even this course, for it may be that the principle, though correct in itself, requires for its successful car-

rying out conditions which the so-called practical man is not very likely to realize, at any rate at first. This is, in fact, the important subject of the limits imposed by the materials available, and this becomes in itself a scientific question. Take, as an example, the case of heat engines. In the case of the hot-air engine, it may be said that precisely the same chief difficulties exist at the present day which led to the rejection of the Stirling engine some time after its trial in 1845, at the Dundee Foundry. The limit of power was due to the limit at which the vessels could be heated; yet, up to the present time, inventors continue to try and increase the power of this, in many respects, admirable motor, by different small and minor alterations, and yet have only at their command, and so can only employ, the same materials as were used forty years ago. The late lamented Chairman of the Council of this Society expressed, some three years ago, as his opinion, that the gas engine was in the same condition as was the steam engine at the time of Newcomen, and that the necessary cooling of the cylinder caused a loss of from one-half to two-thirds of the total heat generated ; but at present this heat still continues to be thrown away, notwithstanding the labors of innumerable inventors, simply because of the working limit of temperature with the available materials in use. To this cause also must be traced the comparatively small increase in the efficiency of the steam engine since the time of Watt. But while, on one hand, the fact has to be clearly realized by inventors that the limits of temperature, strength, and the velocities of surface contact are known and have to be reached, yet the possibilities opened to them by an advance, such as the employment of the electric current or of the use of steel, affords fresh ground for further study and advance. The comprehension, therefore, of what is practicable and what is not becomes of the highest importance.

Enough has been said to show that a more careful treatment of the whole science of machines would be justified from the point of view of the individual inventor. The great advance in scientific instruction in the country, notably under the auspices of the Science and Art Department, but also in schools and colleges, has prepared the way for this. But still it is not to be expected that all inventors will, in the future, be at the pains to acquire the information of real value to them, or even in some cases be more patient of salutary advice than formerly, from the outsider who sees the reason of the futile attempt to escape from the cage of a leading idea. Here, however, a law which has special application to biological development holds rigidly, both with respect to machines and their inventors, and a process of natural selection assuredly results in the survival of the fittest.

There are other grounds for asserting that machine science should be carried to a more complete state, and that it should be more applied than at present, and these, did time permit, might be profitably discussed. Thus, it may be found that, just as from the International Exhibition of 1851, we had to realize the unpleasant fact of Continental superiority in the production of many machines, and were led to make the great changes of the Patent law of 1852, so in the forthcoming International Exhibition it may be made clear that the more systematic and long-continued instruction in machine science abroad has led to results which may stimulate its more careful treatment in this country. Or, again, much might be said about the present basis and grounds of the examination of patents in the Patent office. From the curves of applications for patents already shown, it is evident that though the number has been steadily rising since 1852, yet an extraordinary increase has taken place since first January last year, when the new Patent law came into force, and up to the present date the number promises to be nearly as great for this year. There is little doubt but that this is greatly owing to the reduction in fees during the earlier stages, though it remains to be seen what proportion will be carried through the periods of seven and fourteen years, for which the fees remain the same. It may, however, be safely predicted that this great rise will contain even a smaller proportion of surviving patents, and it will be admitted that Sir Frederick Bramwell had good grounds for not wishing the fees to be so reduced as to lead to an abnormal increase of ill-considered schemes. It is certain

that, even at present, the difficulty of deciding whether an invention is new will be much increased, and will continue to increase, unless some more scientific mode is adopted of determining what really constitutes novelty in an invention. The evils of the present want of a universally-recognized scientific basis of examination is even more strongly felt in applications for patents in various foreign countries, as many a luckless patentee knows to his cost.

To sum up the results in this paper: An attempt has been made to show that machine development has followed a definite course, and has obeyed the general law of progress; that the science of machines is likewise following this law, but at present must be regarded as in an inchoate stage; that there are strong grounds for its increased study and application in this country, and, at the same time, for its further development.

In conclusion, it may be said, the growth of knowledge generally has resulted in laws which have not only brought the numberless facts within our grasp, but have led to further progress. We may not agree with Buckle in his fundamental principle as applied to the history of civilization, which is, that "the totality of human actions is governed by the totality of human knowledge;" but we must admit that the totality of human inventions is so governed. Thus we feel that, in this case, the words of Hermann Kopp, in speaking of the science of chemistry, and quoted by Professor Schorlemmer, apply with force:

"The alchemists worked in vain; it is not in our power to appropriate to ourselves the experiences and results which the future alone can bring. But in a certain sense we are indeed enabled to prolong our life backwards into the past, by appropriating the experiences of those who were before us, and by becoming acquainted with their views as thoroughly as if we had been their contemporaries. The means of doing this is also an elixir of life."

Thus in yet one more respect we have a feature of evolution apparent. Biologists tell us that each animal lives in one short life the whole course of development of its species. So must the inventor live through the whole progress of the special machine he seeks to improve. He may do this by repeating the painful experiences of former workers, or he may avail himself of those lessons which their failures and successes have taught. We should think strangely of a would-be explorer who refused to reach the real scene of his labors by the track which, imperfect and toilsome though it might be, had, in its mere discovery, cost the life labor of many previous travelers; and who, neglecting to avail himself of the previous experience of others, preferred to fight his way to the same end through thicket and jungle. We should scarcely expect him to ever attain that point where his real labors should commence. This must we think, too, of the inventor who, slighting the history of the past, and omitting to benefit by the experiences of the previous workers, essays to take even a humble share in the "Evolution of Machines."

DISCUSSION.

Professor Perry said one was naturally struck on going on board a large ship like the Devastation with the idea that machines were getting exceedingly complicated, and as it was now about 15 or 16 years since he had made such a visit, he had no doubt that still more complicated machines were in use now. Still, on carefully examining the machinery, he found he could understand it. For instance, there were a number of fans for ventilating the ship, and collectively they made a complicated looking piece of apparatus, though the parts were simple enough. If he differed in opinion from Professor Shaw, it was on account of the different meaning which he attached to the word "complicated." To consider the culminating example of the engines of the Atlantic liner giving, say, 10,000 horse-power to the propeller shaft. Now, consider how Watt, 100 years ago, would have given this horse-power to a shaft. If he had been asked to do this, he could have done it, and he would probably have put in 100 of his engines, filling the ship with machinery. It was also quite within his powers to have arrangements which would enable all his 100 engines to stop or reverse at the same time, and instead of the 100,000 parts of existing engines, he would probably have used in all his engines and boilers ten times the total

number of rivets and nuts and other pieces. In fact, to give 10,000 horse-power to a shaft in Watt's days would have needed machinery much more complicated than the machinery in use at the present day. Professor Perry contended that it was on account of the complicated nature of the known machinery necessary, that large steamships were not driven across the Atlantic sixty years ago, and that it was the simplification of construction of steam engines which had enabled huge Atlantic liners to be workable. Although, no doubt, there was a law of evolution in machinery, it would be rather rash to conclude that they knew what that law was. It was all very well to tell inventors of gas-engines that there was no hope of getting much greater efficiency than had yet been attained, but they knew that a great deal had been done in the past, and he believed that if people went on hammering their heads long enough against this wall of the materials not being useable at very high temperatures, they would discover new materials, or some way of getting round the difficulty. It was said that by Taylor's theorem, if you knew the complete motion of a particle, its exact position in space, its velocity, acceleration, and a variety of other things, all included in an exact knowledge of its present state, you could prophecy the position of that particle millions of years hence, and tell what its condition was millions of years ago; and it semed to follow certainly, from the theorem, that you could do so. In the same way, if they knew the law of the evolution of machines, as some higher beings might know it, they might be able to say what machines would come to a few million years hence. But they did not know the law of the evolution of machines; they knew very little of it. The country of Erehwon would probably be known to most of the audience who could spell backwards, and there the people thought they knew the law of the evolution of machines exceedingly well; they saw machines getting more and more powerful, and men getting more and more the slaves of machines, which were becoming more and more automatic, and more and more capable of self-production, so that after a time they feared there would be no room left for human beings on earth, except as parasites, and so they broke up the machines, and when the great traveler who discovered the country visited them, he found they were using very common implements, the fields produced very little wheat, and they were not surrounded with very many comforts. All this evil proceeded from their having the notion that they knew a great deal about the evolution of machines.

Professor Hughes, F.R.S., thought all must agree in the main points of the theory which Professor Shaw had put forward, because he had shown the evolution of these machines, but he had not shown the failures, and he could not say that these stepping stones showed exactly the law. It was certainly a great thing to know what had been done, but great things had been done by men who really knew nothing of what had been done, and which never would have been done had they known it. Take for instance the telephone, all scientific men would have pronounced it to be perfectly impossible; and if Professor Bell had passed his life in studying the science of telegraphy, he never would have invented it. There was, therefore, a good deal to be said on both sides of the question.

Mr. Hiram Maxim then described at some length the construction, and showed the mode of working his patent gun. He said the complication to which the reader of the paper had alluded was not a nece-sary part of the gun; it might have been made to load and fire itself without so much complication; but these complications were introduced in order to allow of the magazine for the cartridges being placed under the gun instead of over it, where it was more exposed, and of its continuing to fire automatically with no attention beyond that of one man who directed the fire. Some other guns required two men to put the cartridges in at the top, and one to turn the crank for firing, and another to turn the gun about, which made the motion very slow, the cartridges falling into their place by the action of gravity. In this gun they were arranged in a belt from which they were taken one by one, and a belt might be made to hold 2,000 if necessary. The speed was adjustable by the trigger, and could be made as high as 600 per minute. The gun could be adjusted so as to have a horizontal fixed range between two points, and thus, if works destroyed in

the day were repaired by the enemy in the night, the bearings and levels could be taken in the day time, and fixed, and at night the gun could be kept firing between these two points all night, by simply a boy to move it slowly from side to side; and he should not be surprised to find that the boy, like the one they had heard of, had devised some plan for making the gun do this automatically. There was such beautiful adjustment in every direction, that you could easily write your name on a screen with it. Having described the means by which the recoil from each shot was utilized to extract the empty cartridge case, Mr. Maxim concluded by saying that when once put into work, the gun would go on firing, if desired, until the man who paid for the cartridges was in a hopeless state of bankruptcy.

The Chairman, Sir Frederick Bramwell, said he gathered from the paper that Mr. Hele Shaw's notion was that the evolution of machinery tended to complexity, and that did not seem very unnatural. As time went on, persons found that devices which they thought would not work, and would get out of order, would work, and could be trusted; they then thought if they could trust one, they could trust two, and so they used two instead of one, with the result that certain functions which had formerly been performed by humanity might be performed by apparatus. Therefore, owing to improvements not only in material, but also in workmanship, machinery became more trustworthy, and, as a consequence, more complicated, and greater benefits were derived from it. He had been much interested in the two early lathes which had been shown, one where the operator held the tool down with his toe, as he had often seen done in Cairo, and the other, the old pole-lathe, the implement on which he first learned to turn. He was sorry to hear Mr. Shaw treat the attempt to make a rotary steam-engine as a retrograde movement. In his judgment it was a reproach to mechanics that they had to begin with a reciprocating movement when they wanted to end with a rotative one; and he could not help thinking that those who, time after time, attempted to solve this problem were deserving of their thanks. He agreed very much with Professor Perry, that there was a good deal to be said on both sides of the question whether inventors ought to know everything which had gone before; and with Professor Hughes, that no one who had been familiar with the science of electricity would ever have dared to think about the telephone. Again, to call to mind the wonderful boot-sewing machine of Blake, which it was agreed on all hands the inventor never would have invented had he known anything about boot-making. No doubt there was considerable danger, when a man studied all that had gone before, that his mind would get into a groove, and thus he would not make really substantive inventions. When speaking on the Patent laws, he had often pointed out that a very favorite suggestion of the opponents of such laws, that there was no need for them because the inventor would be rewarded by making the thing he invented and selling it, was a fallacy, inasmuch as very often the man who had made substantive inventions had been wholly unconnected with the trade to which they referred, and was, therefore, incapable of competing commercially with those in the trade. This being so, he was inclined to doubt the soundness of the suggestion that all persons, before beginning an invention, should know everything that had gone before. In the conclusion of the paper, comment was made on the large number of patents applied for, in comparison with those which were completed. He, for one, was very glad to see that difference, because it represented the value which they had contended so much for in all these patent questions—the time of the provisional protection giving the patentee time to experiment and make inquiry. By that means he found out that he was wrong, that the thing would not do, that it was not novel, or that for some other reason the thing could not be gone on with, and in that way the full benefit was secured, both to the inventor and the public. He looked on the number of abandoned patents as by no means a thing to be deplored, but as an evidence of the care and forethought exercised by inventors before going on to complete their patents.

Professor Hele Shaw said the first three speakers had almost entirely cor-

roborated, at any rate, the tenor of his remarks. He had not been able to develop his ideas completely, for when he came to write the paper, he found that the subject really required a book, and he had to leave off where he should have liked to begin. He believed the general theory of evolution did hold good with regard to machines, and when Professor Perry said he did at last understand the machinery of a large ship, what more striking proof of the truth of his views could be desired. Again, the same thing occurred when Mr. Maxim explained why he had been led to complicate his gun, namely, to make it a more efficient machine. When he used the term complexity, it did not necessarily mean the addition of parts which would get out of order, but that with the progress and development of the manufacturer's arts, increased complexity would be a synonym for the increased certainty of action, as had been the case with many machines. With reference to Professor Hughes' remarks, he might say that he had already quoted Herbert Spencer's words, showing that progress was the differential result of a periodicity of action and reaction, of backward and forward motion. With regard to the rotary engine, he believed that there was now a successful one at work, the Tower spherical engine, which was a marvelous piece of mechanism. He was referring not to the effort to make a rotary engine, but to the way in which those efforts had been made, through ignorance of the principles applicable, one after another having gone on in the same hopeless way, until Mr. Tower had struck out a new line. This had resulted in a motor which though externally it looked very simple was really anything but this, and required elaborate arrangements, and no less than thirty-two figures to completely explain its action and various parts. There was such a thing as degeneration in machines; such a thing as machines made for a higher purpose having for certain reasons to submit to the process of degeneration. The locomotive, vast though its influence on civilization had been, as a type of an efficient motor, and regard simply being had to its maximum efficiency, had certainly degenerated from the condensing engine of Watt. He had not been driving a pet theory, but simply calling attention to what he believed a general law of progress in machines. He had not been quite understood with reference to the line on the curve of application for patents, which showed not only how many never reached completion, but also the immense increase in inventions, and this he had endeavored to show was clearly tending to the absolute necessity for a more scientific classification than at present adopted. A great many men spent hundreds or thousands of pounds for want of knowledge: it was the knowledge of that kind he was advocating; not the knowledge which would prevent them going further, which was simply that little knowledge which was a dangerous thing.

CHIMNEY CONSTRUCTION.

By R. M. BANCROFT and F. J. BANCROFT.

Papers of the Civil and Mechanical Engineers' Society.

Since the reading of the paper on the same subject by Mr. R. M. Bancroft, read in January, 1878, numerous inquiries have been received on the subject from all parts of the United Kingdom and America, owing to the interest and importance of the subject, and in consequence of these inquiries, they had been led to continue their investigations in this branch of architectural and engineering construction.

Chimneys are constructed principally for two purposes,—firstly, to create the necessary draught for the combustion of fuel; secondly, to convey the noxious gases to such a height that they shall be so intermingled with the atmosphere as not to be injurious to the health of the neighborhood.

A chimney-shaft when in work contains a tall column of heated air, which, being lighter than the outside atmosphere, is

forced up by a corresponding column of atmospheric air pressing into the entrance of the furnace; thus a displacement of hot air is constantly being effected, and its place filled by normal air forcing itself through the furnace of the boiler, which in its turn is heated and displaced. The column of atmospheric air and the column of rarefied air in the chimney are somewhat like a pair of scales, or the two ends of a lever of which the boiler is the fulcrum.

Foundations.—In building large chimneys one of the most important points is the construction of the foundation. Very much will depend, of course, upon the nature of the ground. When we are on solid rock, it is only necessary to excavate to such a depth that the heat of the gases will not materially affect the natural stone, and to a depth sufficient to allow the necessary spreading of the base. In many instances, however, chimney stacks have to be built near rivers and on sites where the upper strata are of alluvial clay or made ground, and it is necessary to carry the foundation deep down until a stiff clay, hard sand, or rock bottom is reached. This frequently entails excavation 25 feet or 30 feet deep, or even more, and it is not only requisite that the foundation should be large enough to carry the superincumbent weight, but also that it should be of such an area that it will not allow the base to be forced into the yielding ground. These deep foundations are usually constructed of concrete. In some cases piles are driven in to form the foundation, as, among others, in a brick chimney erected at Boston, and in an iron chimney constructed at Ohio. This is a measure on which the engineers must decide upon the advisability of using it, so as to economize material without risking unequal subsidence, which cannot be too carefully guarded against; and, in fact, it is the practice in the erection of tall stacks to construct the foundation and pedestal, if any, and allow them to stand some considerable time before proceeding with the shaft proper, in order that the work may set and any slight settling take place before a great weight is built upon it. As a remarkable instance of the general settlement of the foundation of a shaft, we may mention a chimney which was built by Mr. Clegg, at Fulham, over a quicksand in which an iron rod sank to a depth of 15 feet with little more than its own weight as pressure. During the erection, the concrete foundation sank bodily 1 foot 4½ inches without cracking the shaft or causing it to deviate from the perpendicular. From this it will naturally follow that in all cases the ground at the foundation should be equally resistant, or unequal settling will take place, as in the disastrous case of the Newlands Mill chimney, Bradford. Some of the pressures exerted upon the foundation are given under the respective descriptions of the chimneys.

Copings and Cornices.—The stone coping or cornice of a chimney will seldom require more to hold it together than two good cramps across each joint; they should be of copper, or double-dovetailed slate dowels. On no account should iron cramps be used, as they will oxidize and burst the stone. Heavy and large caps are often the source of great danger, inconvenience, and expense, as the cap at top in a gale of wind acts upon the shaft as a weight at the end of a long lever. The cap when finished should be a complete whole, or so bound together that the joints cannot open, and be so proportioned that its center of gravity is within the outer circle of the shaft on which it rests, and it should be designed so that the wind striking against it is deflected upwards.

Bond.—In large factory chimney-shafts the longitudinal tenacity which resists any force tending to split the chimney is of more importance than the transverse tenacity, therefore, in these structures, it is advisable to have, say, three or four courses of stretchers to one course of headers. In some circular stacks, a uniform header-bond for the outside courses of brickwork is adopted. This is a practice condemned by some authorities.

Wind Pressure.—It is usual in this country to estimate, as the maximum pressure, 55 lbs. per square foot, but, as in 1868 the pressure of wind at Liverpool was registered at nearly 80 lbs. per square foot, it is advisable to take a higher factor.

If the wind-pressure on a square chimney be taken as	1
that on an hexagonal chimney may be taken as	0.75
that on an octagonal chimney may be taken as	0.65
that on a circular chimney may be taken as	.5

Wrought-Iron Chimneys.—Wrought-iron shafts have found great favor in America and Russia, but in England and the continent generally, as far as we have been able to ascertain, they are an exception. In addition to the wrought-iron shafts detailed in this paper, we have been informed of the following:—Messrs. Witherow & Gordon, of Pittsburgh, Pennsylvania, have since 1876 built upwards of thirty wrought-iron shafts, varying in height from 100 feet to 190 feet, and from 5 feet to 9 feet in diameter. The firm write us that these shafts answer admirably the purpose for which they were built. Mr. L. S. Bent, Superintendent of the Pennsylvania Steel Company, Steelton, Pennsylvania, states that his company have the following eight wrought-iron shafts in use, and have found them both durable any economical:

No. 1, 170 ft. high,	6 ft. 6 in. diameter,	built	'81
No. 1, 165 "	6 ft. 6 in.	"	'77
No. 1, 135 "	7 ft. 0 in.	"	'80
No. 1, 112 "	6 ft. 0 in.	"	'81
No. 4, 110 "	7 ft. 0 in.	"	69'74-5-6

They are lined for 30 feet with 9-inch fire-brick, and the remainder of height with 4 inch red brick. The Ravensdale Iron-works Chimney-shaft, Tunstall (Messrs. Robt. Heath & Sons), is a circular wrought-iron shaft, not spread at its base. Its height from ground-line to top is 75 feet; outside measurement at ground surface, diameter 6 feet; ditto at top, diameter 6 feet. Seventy-five wrought-iron plates were used in the construction of this shaft, the thickness being ¼ inch. The plates have a lap of 2¼ inches, and are riveted together with ⅝-inch cup-headed rivets. The shaft is lined its entire height with fire-brick. The shaft carries off the fumes from three boilers. The wrought-iron chimney of Messrs. Francis & Co., of the Nine Elms Cement Works, Cliffe Creek, Rochester, was erected in 1878. It was designed by Mr. V. de Michelle, C. E., and constructed by Messrs. Fielding & Platt, Gloucester. The shaft is circular, and parallel throughout, and is constructed of wrought-iron plates. The plates vary in thickness downwards from ¼ to ⅜ inch. Its height from ground-level to top is 160 feet; external diameter throughout, 5 feet; internal diameter throughout, 4 feet 6 inches. It is lined with 3-inch firebrick its entire height. The chimney is stayed against the wind by four 3¾-inch steel guy ropes. This chimney was erected over the center one of a row of nine cement kilns, which are all connected to shaft by a wrought-iron horizontal flue 4 feet in diameter. Two additional ones have since been added, and the chimney now carries off the gases from eleven cement kilns. Round the outside of center kiln on the ground level is fixed a cast-iron curb or base-plate. On this base stand four cast-iron standards or supports, having their lower ends butting on to and secured to base-plate. The standards incline inwards until their upper ends meet to support a cast-iron circular chimney-base which forms the top of the center kiln. The wrought iron chimney proper commences from top of this circular cast-iron base, directly over which the 4 feet horizontal flue is connected to shaft. For the construction of this chimney a timber stage was erected at the level of the kiln tops, and upon this stood the rivet fires. Four winches were worked on this stage, and to them were led guy ropes, after passing round blocks at convenient distances. A hydraulic press, with a 4-foot stroke, was then fixed over the center kiln, and the top length of 20 feet, which had previously been riveted-up on the ground, and raised to the stage level, was placed upon the ram. The ram was then pumped up, and the 20-foot length raised a height of 4 feet, the guy-ropes being slackened out to the quired extent, as the 20 foot length gradually rose. A 4-foot ring of plating was then riveted on with ⅝-inch snap-head rivets and the usual lap, the ram was then again pumped up, and the now 24-foot length raised the necessary height; another ring of plates was then riveted on, and the operation repeated, until the chimney had reached its required altitude. The cost of this chimney was about £1,000, including long wrought-iron flues.

Messrs. Wesenfield & Co.'s Chimney, Chemical Factory, Barmen Prussia.—This has a square brick pedestal and an octagonal brick shaft. Its total height from foundation to top is 345 ft.; height from ground line to top, 331 ft. The pedestal is 20 ft. sq. by 40 ft. high by 7 bricks (equal to 5 ft. 3 in.) thick. The octagonal shaft is 291 ft. high, 17 ft. outside diameter at the base by 5 bricks (equal to 3 ft. 9 in.) thick; 11 ft. outside

diameter at the top by 2 bricks (equal to 1 ft. 6 in.) thick. The shaft diminishes $2\frac{1}{2}$ in. every 10 ft. in height, or 1 in 48. The internal octagonal clearance is 8 ft. throughout. The foundation is on a bed of hard and coarse gravel, and made of large flat quarry stones bedded with "terrass" mortar in the proportions of 1 lime, 1 river sand, and 1 "terrass" (a kind of pozzuolana). The pressure on the lowest part of chimney proper is equal to 21,335 lbs., or $9\frac{1}{2}$ tons, per square ft. The pedestal and shaft were built with bricks and ordinary mortar, composed of 1 of lime to 2 of river sand, prepared every morning by the masons themselves. On rainy days cement mortar was used in the proportion of 1 cement to 2 river sand. The courses of brick were flushed up with cement as construction proceeded. The crown of the shaft was built with cement exclusively. The foundation and pedestal were built in the summer of 1867, and the chimney was successfully completed in October of the same year. According to the original design, it was intended to build to a height of 260 ft., but, as the erection was proceeding in a very satisfactory manner, it was considered safe to increase the height without altering the dimensions of the base. But before doing so, a comparison was made between the pressure on the foundations of this chimney and the pressure on the foundations of a chimney erected at Bochum, Prussia. These were found to be as follows:—

	Lowest part of chimney proper. Press. per sq. ft. lbs.	Press. per sq. in. lbs.
Chimney at Barmen, Prussia..	21,335	149
Chimney at Bochum, Prussia..	18,429	128
Excess of pressure on Barmen chimney foundation......	2,906	21

The three masons who constructed the chimney daily changed their positions, so as to equalize any unevenness in their respective laying. Every fifty feet a course of brickwork was painted *black* so as to indicate the height of any point of the chimney above ground. The chimney was built from the inside. The materials were hoisted by a steam engine erected temporarily near the place of construction. The frame which supported the upper drum over which the chain worked was moved higher after the completion of every three or four courses, and was at the same time turned horizontally from one side of the octagon to the next one, so as to equalize the pressure of the frame on the masonry. The holes made into the masonry to support the frame were filled up with bricks and mortar immediately after the removal of the frame to a higher level. The chimney, when completed (Oct. 1867), was vertical. In the Spring of 1868, remarkable for storms and long-continued gales, this stalk inclined towards the north-east by the action of the south-west wind, probably aided by the softness of the mortar and the large size and shape of the ornamented chimney crown, which caught the wind and acted as on a long lever. The deflection was considerable at the end of May, and apparently increased. As before mentioned, layers of bricks in the shaft at distances of fifty feet from each other were painted black. The height of these black lines above the pedestal being known, they were, by means of a theodolite, projected on a board which was fixed on the pedestal, and these projections showed that at

251 ft.	high	the chimney	was out	of plumb	45 in.
210	"	"	"	"	30 "
160	"	"	"	"	16 "
110	"	"	"	"	5 "

The pedestal stood perpendicular. As the canting of the shaft was still increasing, immediate action had to be taken. The ordinary method of straightening chimneys was at first resorted to. A hole was made through the whole thickness of the masonry on the side of the chimney which required a lowering, at a distance of 4 ft. above the top of the pedestal; into this hole a saw was passed, and an attempt was made to cut through one-half of the shaft, but owing to the thickness of the wall and the hardness of the bricks, the saw could only be worked from one end, and the effect of sawing after two hours' work was almost *nil.* The hole through the stalk having been made with little trouble, and the difficulty experienced in sawing, led to the idea of removing a course of bricks and replacing it by a thinner one. Before the work was proceeded with, an experiment was made on an old inclined shaft 120 ft. high. This proving successful, it was determined to treat the new chimney in the same way. A layer of bricks was broken out by means of pointed

cast-steel bars, varying from 1 ft. 6 in. to 5 ft. in length. The first division removed was in the middle of one of the octagonal faces. When division 1 was broken out it was replaced by thinner bricks covered with "terrass" mortar. After this, two divisions, one on either side of the first, were broken out and replaced by thinner bricks; then the two divisions on either side of the open space, and so on until one-half of the whole course had been exchanged. Purposely-made flat shovels with long handles were used to lay the bricks, which had to be placed near the inside of the chimney. A side-space of 5 in. was left between the newly-laid bricks and the old ones of the next division, so as to enable the workmen to break out the latter with greater facility. The width of each single division was 2 ft. to 2 ft. 6 in.. The masonry directly above was sufficiently dry not to give way when a course of that width was removed from below it. The replaced bricks were thicker near the points (at 90° distant from the first division cut out), so that the difference was greater in the middle and gradually less towards the extremities. As soon as the slit reached these points the chimney began to move, and by slight oscillations slowly settled down on the new layer of bricks. The time occupied in settling by oscillation at each substituted course varied from 18 to 36 hours, according to the width of the slits, which were different in the various cuts performed. The oscillations were greater the higher the cut. At the highest cut, 100 ft. from the top, the oscillations frightened the masons, and they left the place. The slit became alternately wider and narrower by ¾ in.. This seems to prove the elasticity of the structure. Four cuts were made,

1st	4 ft. above	pedestal,	greatest	width	⅜
2d	100	"	"	"	1¼
3d	140	"	"	"	1½
4th	191	"	"	"	1¾

After the completion of this work the chimney continued during several weeks to settle slightly in the direction opposite to its former inclination. This circumstance had to be carefully considered beforehand, or else the slits would have been made too wide, and so have produced an inclination in the opposite direction. A severe storm on the 6th and 7th December, 1868, which overthrew several chimneys in the neighborhood, did not affect this one. The result of the straightening operation described above was quite satisfactory. The heights of the upper cuts were reached as follows: Standing on a platform, the masons made a number of holes into the exterior wall of the chimney, 4 ft. above the platform on which they stood. Into these hole the ends of iron bars were fixed, and boards secured to them so as form another platform. Standing, then, on the latter, they fixed another platform 4 ft. higher, in the same way. Every second platform was removed, so that the remaining ones were 8 ft. apart; they were then joined by ladders for the workmen to ascend. This method of straightening is only practicable when the chimney has a considerable diameter, and when the mortar is sufficiently dry as not to give way under pressure of the bars and platforms.

Chimneys at Duisburg.—In December, 1868, a chimney was straightened at this place by the method just described, but as the diameter was not so great as that at Barmen, and as the mortar was soft, a wooden scaffold was erected round the shaft to reach the upper points which required cutting. The breaking out and replacing of bricks could not be done in divisions wider than 5 in. to 10 in., as the upper masonry, not being dry, would have settled down. When the chimney was straight, a further settling down towards the side of the cuts was prevented by driving iron wedges, covered with mortar, into the slits.

Edinburgh Gas-Works Chimney.—This chimney was designed by Mr. Mark Taylor, engineer to the company. Messrs. Geo. Buchanan, C. E., and Prof. Gordon, of Glasgow, were consulted. The builders were, for the stonework, Mr. James Gowan, of Edinburgh, and for the brickwork, Mr. James Bow, of Pollocksfields, near Glasgow. The pedestal is square, and of stone, surmounted by a circular brick shaft. Its dimensions are:

Stone foundation under ground.......	6½	ft.
Part of base..........................	6	"
Stone pedestal above................	65	"
Brick shaft...........................	264	"
Total height from foundation to top	341½	"

Height from ground line to top, 329 ft.

The foundation stone is 40 ft. 6 in. square, by 6 ft. 6 in. deep. The pressure on the bottom of foundation per square foot is nearly $2\frac{1}{2}$ tons. The stone pedestal is 30 feet square at ground line, and 27 ft. 9 in. square at top. The internal diameter of the pedestal is 22 ft. 6 in. at bottom, and 20 ft. 4 in. at top. This was built during one summer, at the end of which the works were suspended until the following year. The brickwork of the shaft was commenced and finished in the summer following the erection of the pedestal. The following dimensions relate to the outer brick shaft (circular):

Outside diameter at bottom		....	26 ft. 3 in.
Internal	" "	....	20 ft. 5 in.
Outside	" at top	....	15 ft.
Internal	" "	....	12 ft.

This was built up in five steps, as follows:

1st section,	35 ft. high,	$3\frac{1}{2}$	bricks,
2d "	40 ft. "	3	"
3d "	48 ft. "	$2\frac{1}{2}$	"
4th "	58 ft. "	2	"
5th "	83 ft. "	$1\frac{1}{2}$	"
	264 ft. total.		

The greatest pressure on any part of the work comes at the lowest section, where it amounts to about 8 tons 2 cwt. per square foot. The inner brick shaft (circular) is distinct from the outer shaft, and is 90 ft. high, with 13 ft. internal diameter throughout, and was built in four steps, viz.:

1st section,	14 ft. high,	35	in. thick,
2d "	6 ft. "	30	"
3d "	30 ft. "	25	"
4th "	40 ft. "	20	"
Total	90 ft.		

The thicknesses include a lining of fire-brick 10 in. thick for 20 ft., and 5 in. thick for the remaining 70 ft. The weight of the materials used is about 3,700 tons. The total cost was £4,637. The lightning conductor is a solid copper rod, $\frac{5}{8}$ in. diameter. The stones used in the foundation are Cragleith, Humbie, and Hailes, which, before use, were tested by Mr. Buchanan and Mr. James Gowan. The tests were made in the most careful way by crushing cubes of 1 in. square. The following were the results of tests:

Craigleith crushed at	315	tons per square foot.
Humbie "	240	"
Hailes "	225	"

A second test of Craigleith showed that before being crushed to powder it sustained a pressure of 440 tons per square foot. The appearance after fracture of the different cubes was that of a pyramid or wedge, and this led Mr. Gowan to assert that if the cubes were enlarged a greater relative increase of strength would be gained, and further that if the pressure were *vertical* to the line of cleavage a greater resistance would be obtained, so that such a stone as Hailes, which is a laminated stone, would increase in strength according to its surface more in proportion than that of a rock-stone such as Craigleith. This led to discussion and further tests, the result being that with a 4 in. cube from Hailes quarry the resistance was equal to 567 tons per square foot. The bricks were supplied by Mr. Livingstone, of Portobello Brick-works, and were tested with the following results:

Description of Specimen.	Length.	Breadth.	Thickness.	Weight.	Crushing weight on each brick.	Crushing weight per sq. foot.
	Ins.	Ins.	Ins.	Lbs.	Tons.	Tons.
Extra size and quality.....	10	5	3	$10\frac{1}{4}$	153	440
" "	$9\frac{1}{4}$	$4\frac{3}{4}$	$2\frac{3}{4}$	$9\frac{5}{16}$	140	448

Repairs of Chimney-Shafts.—In the *Builder* for January 8th, 1875, there is an interesting account of the repairing of chimney-shafts damaged during the siege of Paris by the fire of the German artillery.

REPORTS OF ENGINEERING SOCIETIES.

Engineers' Club of Philadelphia—Record of Special Business Meeting, June 6th, 1885.—The Secretary presented a communication from Mr. Wm. T. Blunt, secretary of a committee appointed by the Civil Engineers' Club of Cleveland to investigate the relation of army and civil engineers in the Government service, and report a line of action in the premises. The communication requested that a similar committee be appointed by this club.

On motion, the consideration of the subject was postponed for the present.

On behalf of Prof. Louis H. Barnard, M. Dana C. Barber presented an illustrated article upon Stereographic Projection, which being entirely mathematical, does not admit of presentation in abstract.

Mr. Percy T. Osborne presented an illustrated description of the Sand-Bag Embankment to close Little Inlet on Brigantine Beach, N. J., which was constructed by his father, Mr. Richard B. Osborne, C. E.

The closing of this inlet, the width of which was 831 feet, with a rapid current at ebb and flow of tide, never was deemed possible with the very fine sand which was the only material at hand.

The changing character of this shore, under the action of the north-easterly winds, the laws of nature and the results they had in years past produced on this beach, the possibility of making these co-workers in producing like results in a shorter period of time, supplied the only expectation that, with the means possessed, this water way could be obliterated, and a large area of land redeemed from the sea.

By closing this stream for a *short period*, it was expected that the sea would form rapidly what it would require perhaps a century otherwise to accomplish, and that the accretion from the sea would quickly protect and cover up the works of construction, without which it was utterly useless to attempt to build a barrier against the ocean, out of the material available. The expectation has been proved to have been founded on correct principles. The sea has performed its part most fully. The operations of nature's laws have been quickened. and the inlet to-day is a thing of the past. The sand was packed in ordinary salt bags, costing 5½ cents a-piece, sewed up and placed in the embankment, at a cost of 66 cents per cubic yard for bags, and 34 cents per cubic yard for filling, sewing and placing, making a total cost per cubic yard of one dollar. The working hours were between half ebb and half flow of tides. The embankment was made from each shore simultaneously, to within a foot of low water, by placing the bags by hand. On these bags, frames, 6 feet in height, 12 feet broad on top, and 8 feet apart, were placed, and connected by planks spiked on the sloping sides. The interior spaces were filled with sand bags, 80,000 of which were used in the construction of the embankment. About 1,050 were placed in a day of 10 hours, 12 of them equaling 1 cubic yard. At low tide the water in the main channel was 13 feet deep when the bank was begun, on June 17th, 1881. On the 5th August following, the embankment was closed and the sea shut out—in 42 working days—and where the deep channel had been, there was then less than 2 feet of water; the deposit of sand from the sea having commenced with the embankment and increased as it progressed. For 9 days after closing, the embankment withstood the heavy seas and south-easterly gales. On Aug. 12th the sea broke over the banks, near the north end, where the water was 2 feet deep. Having some bags on the ground, this breach was closed on the next ebb tide. On the 14th another breach occurred, when there was nothing at hand for its repair, the bags ordered on August 6th not having been supplied for 18 days. When they did arrive the company had discharged the contractor, and the breach had only to be left to itself. It continued to widen up to 16th September, when it was 132 feet, with 16 feet depth of water below the original surface, the old 13 feet channel remained closed, and three bars from the bank seaward had been created. It was expected then that the accretion would still continue, and that finally, in spite of the breech, the closing of the original channel and the alteration of the course of the tide would continue the deposit and finish the undertaking without further efforts. Something was attempted afterwards by one of the company—by driving poles and stretching light wires along them—but these petite attempts conduced in nowise to the ultimate result. The increased action of the sea finally obliterated the inlet, and covered the whole locality, thus proving that nature had fully performed what had been expected of her—70 acres seaward and 30 acres inside were reclaimed.

June 20th, 1885.—The Secretary presented, from Prof. J. A. L. Waddell, a communication proposing that the Club organize a system of Review of Engineering Literature, and suggesting methods therefor.

The Secretary presented, for Mr. James Beatty, Jr., a paper upon the Relative Costs of Fluid and Solid Fuels. After giving the relative advantages in economy of labor in use, reduction of weight and bulk, ease of manipulation of fire, perfection of combustion, and cleanliness, the principal substances, experiments and processes are noted.

Notes and tables are given as to the compositions of different fuels, their heat units and evaporative capacities, efficiencies in furnace, prices per unit, and lbs. of fuel for $1.00 and lbs. of water evaporated from 212° F. for $1.00, in various localities.

The paper concludes with the following table, of which the author says: "These figures are very much against the fluid fuels, but there may be circumstances in which the benefits to be derived from their use will exceed the additional cost. It is difficult to make a comparison without considering particular cases, but for intermittent heating, petroleum would probably be more economical, though for a steady fire, coal holds its own."

RELATIVE COSTS OF FLUID AND SOLID FUELS.

	Anthracite.	Bituminous.	Petroleum.	Coal Gas.	Generator Gas.	Water Gas.
New York.....	1.00	1.08	1.71	14.92	22.90	8.70
Chicago.......	1.00	.71	1 50	8.72	18.30	7.00
New Orleans...	1.00	.59	1.56	17 90	15.30	5 80
San Francisco..	1.00	.64	1.50	8.75	9.40	3.50
London........	1.00	.61	2.05	7.16	17.70	6.30
Port Natal	1.00	.90	1 21			
Sydney........	1.00	.34	1 39			
Valparaiso.....	1.00	.44	1.03			

Prof. L. M. Haupt announced, by title, a paper upon the Repairs to the Conduit of the Philadelphia Traction Company, which will probably be ready for publication during the summer.

The Secretary presented, for Mr. C. W. Buchholz, a Memorial of the late William Lorenz, reviewing his studies, his early experience, his career on the Philadelphia and Reading Railroad from topographer to Chief Engineer, his great scientific and practical ability, and last, but far from least, the extraordinary completeness of his manhood.

ENGINEERING NOTES.

Summary of Report on Preservation of Timber to American Society of Engineers.—After a brief statement of the labors of the Committee and of the evident necessity for the introduction of preserving processes on account of rapidly diminishing supplies of timber, a short history of the progress of the art is given, showing three principal methods of working, viz.:

1. Steeping.
2. Vital suction or hydraulic pressure.
3. Treatment in close vessels by steaming, vacuum, pressure, &c.

The experience in the United States is given in five tables, comprising the results more or less conclusive of 142 authenticated trials or experiments. In each case these are referred to at more or less length in the text, sufficiently to give the reasons for success or failure, and the lesson taught. The five heads corresponding to the tables are:

1. Kyanizing, or use of corrosive sublimate.
2. Burnettizing, or use of chloride of zinc.
3. Creosoting, or use of creosote oil,
4. Boucherie, or use of sulphate of copper.
5. Miscellaneous, or use of various substances.

Of the first, *Kyanizing*, it is stated that an absorption of four or five pounds of corrosive sublimate per thousand feet, b m , is considered sufficient, and it would now cost about $6.00 per 1,000 feet, b. m. It is not recommended except in situations where the air can circulate freely about the wood, as in bridges and trestles, but in very damp locations (as for ties when in wet soil and pavements), its success is doubtful. Its cost when first used led to cheating, which for a time brought discredit upon it.

Burnettizing the committee do not consider the best adapted to use where the timber is exposed to the washing action of water (as this removes the preservative); but, on account of its cheapness, it is probably to be preferred at the present time to any other process for the preservation ef railroad ties. The Wellhouse, Thilmany and other modifications of the process aim at making the chloride insoluble, but are yet on trial. This process has been largely and successfully introduced in Germany. Experience shows the life of soft wood ties to be doubled and trebled by its use. Its cost in this country is about $5 per 1,000 feet, b. m., or 20 to 25 cents per tie, and for the latter purpose the committee particularly recommend it. The work must be well done; but some of the failures were from doing it *too* well, that is, from using solutions of too great strength, thus making the timber brittle.

A solution of 2 per cent., by weight, of chloride of zinc in water is recommended.

Creosoting, or the injection of timber with hot creosote oil in a cylinder under pressure, is considered to be the very best process which has been fully tested, where *expense* is not considered. It is as yet the only one known which is sure to prevent the destructive attacks of the teredo or other marine animals, and to give absolute protection against decay in very wet situations. It is a somewhat expensive process, requiring for protection against the teredo from 10 to 20 pounds per cubic foot of timber, and costing from $12 to $20 per 1,000 feet, b.m. For resisting decay alone a cost of $10 to $14 is sufficient.

The *Boucherie* process, in which green timber is impregnated with sulphate of copper either by *vital suction*, *hydraulic pressure* or a *vacuum*, when well done, using a solution of 1 pound of sulphate to 100 of water, has proved fairly successful.

Under the head of "miscellaneous," are classed 41 experiments with almost as many substances, sulphate and pyrolignite of iron, lime, resin oil, tar, &c., but with as yet no commercial success.

The general principles laid down are, to select the process with reference to the subsequent exposure. Use *open-grained*, *porous* timber, for that reason in *general* the cheaper woods.

Extract the sap and water to make room for the material to be injected, natural seasoning, except for the Boucherie process, being very desirable. Steaming takes the place of seasoning.

Use enough of the antiseptic to insure a good result, and then let the timber dry before using, as its durability will thus be increased. Do not hasten the work if it is to be well done. Protect ties or timber in the track as far as may be from water by drainage.

Contract only with reliable parties of establish reputation, under a skilled inspector, who must be in constant attendance when the magnitude of the order warrants.

There is at the close a discussion of the question, Will any preserving process pay? This is answered in the affirmative. The chairman of the committee gives a careful estimate in one of the appendices in an actual case in this country; another general estimate is given based on European experience, and three other separate appendices give different methods of examining the question of economy and comparing values.

Other appendices (to the number of twenty in all) treat of the general question of destruction and conservation of forests, and give reports of the personal experience of a number of engineers, with methods pursued, apparatus used, &c.

Amount of Water Required for Irrigation in Salt Lake City.—The amount of water, which Salt Lake City requires for irrigation during the period of about 120 days, from

the middle of May to the middle of September, is so enormous, that the amounts required for domestic and other purposes, sink into insignificance by comparison. Fast becoming a city, filled with lawns and shrubbery, in place of gardens and ordinary crops, with abnormally wide streets, the requirements are simply immense. Long experience has demonstrated that watering in our climate for city lots, should be as often, as at least once a week, and such is the custom here. During the "season," from 17 to 20 waterings are had; and we find from the tables of rain fall, that only 3.31 inches of rain can be depended on during this period. Experience has also demonstrated, that the *best results* are obtained, when, at each watering, as much water is used as would be equal to covering the ground one inch deep. At *least*, this amount is generally required.

This, therefore, means, that, during the "season," from 20 to 23 inches are flowed over the land, the rainfall being counted in; and practically amounts to the same as though that many inches of rain had fallen, between the middle of May and middle of September. To those not familiar with the needs of irrigation, this seems an unusually large allowance, and very few appreciate how large an amount of moisture they put on an acre of ground, when they cover it an inch deep. It means .63 gals., or 5¼ lbs. per sq. ft., or 113 tons per acre at each watering, or 2,260 tons of water in a season. Yet, large as this looks, it is not as great as the original founders of the city deemed necessary to provide.

In a conversation had with Mr. Orson Pratt, some fifteen or sixteen years ago, on the subject of amount of water required for irrigation here, that gentleman said to the writer. "It was our intention that one cubic foot of water per second should suffice for 100 acres of land for about as many days, and at the outset that or more perhaps was used: since we have got the ground saturated our crops don't probably require, in ordinary seasons, more than half that."

This one cubic foot per second of water duty, allows nearly 24 inches for a season of 100 days; but gardens, lawns, etc., require water during at least 120 days. At each watering we find required not less than 3,630 cubic feet, or 27,225 gallons per acre. During parts of July and August, the least flow of City Creek is given as being 7,200,000 gallons, or 960,000 cubic feet per day, sufficient for 1850 acres during the "season." The total number of acres outside of the 1st, 10th and 11th Wards, lying above City Canal level, including the cemeteries, is 812, of which 295 are already supplied. Should the balance of this 812 acres be also watered from City Creek it would divert 2,010,757 gallons of that water, now flowing into the lower wards. The result being that *all* the water from City Creek not required for lands above canal level, or 4,041,901 gallons could pass through the pipes; instead of the bulk of it as now, going through the ditches. Water through the pipes means an income therefrom, and at 20 gallons per head, this 4,000,000 gallons gives a supply for 200,000 people. By storing one-fourth of the flow of City Creek this four millions can be more than doubled.

Thus using City Creek water for the areas *above* City Canal level, leaves 2,840 acres in the city *below* its level. Should this canal be drawn upon for its full capacity, it would admit of flooding this entire area of 2,840 acres, 1⅝ inches deep every day, or 11⅜ inches at each watering, or 16 feet deep during the season.

It follows, therefore, that the canal, for the *necessary irrigation only*, of the lands within city limits below its level, need furnish but 11,045,571 gallons daily Two-thirds of the whole area of the city would thus seem to be pretty well provided with water.

This apportionment of the waters, if made, would only be carrying out in detail, the ideas suggested by Mr. Ellerbeck last summer, or like that now made in alloting the canal water in lieu of Emigration Creek in the 1st, 10th and 11th Wards, and will give to those making the exchange, more water, should it be required, than they could ever secure from City Creek alone.

It has been suggested, that owing to its leaky condition, the canal cannot be made *at present* to flow to its capacity. Neither will it at present be required to. The amount proposed to be taken from City Creek, for the entire 517 acres unsupplied, including Cemeteries, only diverts 2,010,757 gallons, from that creek, and not that amount until the whole of these 517 acres shall have come under irrigation. As a matter of fact *at present* and with full supply given, to the lands of the living and to the Cemetery, less than 1,500,000 gallons per day will be diverted for several years, leaving some 20,500,000 gallons for pipe supply, more than now flows through them.

Repairs would certainly remedy the leakage of the canal, and repairs like repentance are always in order, and an extra four millions of flow would require but five inches more depth. Whatever may be said about the faulty construction of this great waterway or the damage from muskrats, yearly, the wisdom displayed in its adoption was of the highest order. Nor has it been built any too soon to meet our rapidly increasing wants. The waters of Utah Lake, which it brings us, for irrigation and manufacturing purposes, are superior in quality to those from all other sources. We are, however, frequently told that the construction of a canal at all was a terrible mistake, that a pipe should have been laid to prevent any chance for damage to lands along its route, etc. Well! a pipe, or its practicable equivalent, a masonry conduit, would be a good thing to have, but to bring the same amount of water from the same starting point on the Jordan, would have required that conduit to be eleven (11) feet in diameter, or if iron pipes were used, 14 of them 3 feet in diameter, would have been required, and carried the cost away up into the imposing millions. Surely the $225,000 the canal has cost was a judicious expenditure.

With a city like Salt Lake, which has no large manufactories a supply of twenty gallons per head, exclusive of hose service, is generally conceded by all hydraulicians ample for domestic purposes. This means 7,300 gallons

per annum to an individual. Now, an acre of land requiring irrigation during the same period, though distributed generally during 120 days, requires not less than 490,000 gallons, or nearly 70 times as much. During the 120 days, however, the acre consumes 204 times as much as the individual. Following out the calculation we find that the water absolutely required for our irrigation, with no allowance for entailed or other waste, would suffice for a population of 852,000 people. I gave these figures to show the cause which necessitates such immense volumes of water. The probabilities are, that during the dry season, 50,000.000 or more gallons daily will be used and rightfully wasted. By rightful waste I mean that quantity of water left to flow in our streets not only for irrigation therein, but that which cannot well be controlled, as also that quantity, which so flowing will always be an important factor from a hygienic point of view.*

In all the foregoing estimates the quantities given are for a supply to the *entire* acreage of the city; while the least capacity of the streams have been taken as the basis from which such supply is to be had. This is not entirely a just way of estimating, since, it is fairly presumable that whenever required in the future, one-fourth of the flow of both City and Emigration Creeks could be stored and utilized.

As will also be noticed no deduction has been made for buildings, the whole area having been computed as requiring irrigation. The requirements of the streets, will, however, quite offset this amount, and it is sincerely to be hoped that no diminution of the flow along them will ever be required, and below the canal there would seem to be no excuse for any such diminution.

* The streets of Salt Lake City are 132 feet wide, and it is the intention to keep a continuous flow in the side ditches. With the exception of about six weeks in winter this flow of water is kept up. There is not a sewer in the city.

IRON AND STEEL NOTES.

MAGNETIZATION OF IRON, NICKEL, AND STEEL.—Mr. Shellford Bidwell has communicated a paper to the Royal Society on the changes produced by magnetization in the lengths of rods of iron, steel, and nickel The most novel result of these observations is the fact that if the magnetization of iron rods be carried beyond a certain critical point, the consequent elongation, instead of remaining stationary at a maximum, becomes diminished, the diminution increasing with the magnetizing force. If the force is sufficiently increased, a point is arrived at where the original length of the rod is totally unaffected by magnetization; and if the magnetization be carried still further, the original length of the rod will be reduced. It also appeared from Mr. Bidwell's researches, that the position of the critical point in steel depends in a very remarkable manner on the hardness or temper of the metal. Considerable light is thus thrown on the anomalous results obtained by Joule and Mayer. His experiments also disclosed reasons for believing that the value of the critical magnetizing force in a thin iron rod is greatly reduced by stretching. This explains the fact that Joule obtained opposite effects with stretched and unstretched wires. It is known that the length of a nickel bar is diminished by magnetization, and Mr. Bidwell shows that it continues to retract with magnetizing forces far exceeding those which produce the maximum elongation in iron. The greatest observed retraction in the case of nickel was more than three times the maximum observed elongation of iron, and the limit was not reached in the experiments. A nickel wire stretched by a weight undergoes retraction when magnetized. The critical value of the magnetizing force for a steel rod diminishes with increasing hardness up to a certain point corresponding to a yellow temper; after which it increases, and with very hard steel becomes very high. There is, therefore, a critical degree of hardness for which the critical magnetizing force is a minimum. In steel of a yellow temper the value of the critical magnetizing force is lower than in steel which is either softer or harder. A temporary elongation once produced in soft steel may be maintained by a magnetizing force which is itself too small to originate any perceptible elongation.

SAFETY OF IRON PILLARS IN CASES OF FIRE.—We stated sometime since that owing to the upper stories of buildings in Berlin falling in during a fire, by the giving way of cast-iron pillars, the Prussian police authorities had issued an edict forbidding the use of cast-iron pillars in any inhabited building, but permitting the use of wrought-iron pillars. Cast-iron may only be used provided that each pillar is surrounded by a fixed casing of sheet iron, in such a manner that there is a good air-space between the two. This edict has provoked much criticism and opposition, and several authorities have reasoned against it, as well as made experiments to disprove the assumption on which it is based. Prof. Bauschinger of Munich, recently made a long series of actual trials with pillars of both cast and wrought-iron. He loaded them with the weights that they are usually allowed to bear in buildings, and heated them first to 300 deg. Cent., then to 600 deg. Cent., and finally to a red heat, and let a stream of cold water play on them, exactly as would be the case in a fire being extinguished by fire engines. The cast-iron pillars were much damaged and cracked by this treatment, but continued to carry their loads quite safely, while those of wrought-iron were much bent before redness was reached, and so twisted when cold water was squirted on them that they could not carry their loads. The conclusion is that cast-iron is really far safer for buildings than wrought. Pillars of other materials were also experimented with, viz., natural stone, brick, and concrete. The latter stood the test best, resisting a fire of three hours' duration. Also pillars of ordinary brick stood very well, but granite, sandstone, and other natural stones did not show as much resistance. If the obnoxious edict of the Berlin police has done no other good, it seems at least to have set a good many people to work on this important subject.

RAILWAY NOTES.

It is said that the construction of a ship railway to connect the Bay of Fundy with the Gulf of St. Lawrence has been finally decided on. Ships of 1,000 tons and under will thus be able to reach St. John from Montreal, Quebec and other ports on the St. Lawrence, without having to encircle the dangerous Nova Scotian coast, a saving of 600 miles. The ship railway, which is to be seventeen miles long, will, it is expected, be supported by a subsidy of £60,000 per year for twenty years from the Canadian Government.

There is still running on the Western and Atlantic Road, in Georgia, hauling a passenger train, the old locomotive General, which —the *Railroad Gazette* says—was the pursued party in one of the most exciting chases on record. The locomotive was carried off by a small party of Federal scouts during the war, while the engineer and firemen were at dinner, and the train was stopping at Big Shanty. The pursuit was kept up for over 100 miles before the engine was finally recaptured, and she was only abandoned when entirely out of fuel and water, and the journal bearings had been almost entirely melted out, the supply of oil having also run out. In the chase, this General and the pursuing engine probably made the fastest time ever run on a southern road, although all parties were too much engaged in the business on hand to keep any record of the actual speed.

ORDNANCE AND NAVAL.

An addition to the German navy was made on the 18th of May, by the launch, at Dantzic, of the fast cruiser corvette, Arcona, which took place in the presence of General von Caprivi—the chief of the German Admiralty—Admiral Jachmann naming the vessel. The Arcona is a sister ship to the Alexandrine, launched in February last at Kiel, and is of the following dimensions: Length between perpendiculars, 72 meters (237 ft.); breadth of beam, 13 meters (42 ft.); displacement, 2370 tons. She is built of iron and steel throughout, and has a double planking of teak, sheathed with copper. Her draught of water when completely fitted up and fully armed will be 4.60 meters (somewhat over 15 ft.) forward and 5 meters (16 ft. 6 in.) aft. The vessel is divided into eight watertight compartments by cross bulkheads, the two largest ones containing engines and boilers. She will have two compound engines, working independent of each other, placed side by side in the direction of the keel, and developing together 2,400 horse-power. Steam will be supplied by eight cylindrical boilers, four to each engine, placed into two separate boiler rooms. The estimated speed of the Arcona is between 14 and 15 knots (16 to 17 miles) an hour. She will be armed with twelve 15 centimeter (5.85 in.) and two 8.7 centimeter (3.39 in.) guns, one light gun, and four Hotchkiss guns. She will also be fitted with a launching apparatus for Whitehead torpedoes.

BOOK NOTICES.

Publications Received.

Minutes of the Proceedings of the Institution of Civil Engineers.

Paper No. 1983.—Modern Practice in Construction of Steam Boilers. By David Salmond Smart.

No. 2024.—Comparison of British and Metric Measures for Engineering Purposes. By Arthur Hamilton Smythe, B. A.

No. 2047.—Maximum Flood-Discharge from Catchment Areas. By James Craig, M. Inst. C. E.

No. 2051.—Experiments on Friction of Disks Rotated in Fluid. By William Cawthorne Unwin, M. Inst., C. E.

No. 2056.—Method of Removing Rock under Water. By Charles James, Assoc. M. Inst. C. E.

No. 2065.—Cost of Dredging at Calais and Boulogne. By F. Guillain.

No. 2073.—Guns as Thermodynamic Machines. By James Atkinson Longvige, M. Inst. C. E.

No. 2074.—Standard Engine Shed. By Francis William Webb, M. Inst. C. E.

No. 2079.—Brauer's Dynamometric Brake. By H. Walthier Messnier.

No 181.—Students' Paper—Trigonometrical Surveying.

No. 185.—Students' Paper—Secondary Batteries.

No. 188.—Students' Paper—Gauging Flowing Water.

Professional Papers of the Signal Service. No. XVI. Tornado Studies for 1884. By John P. Finlay. Washington: Signal Office.

Monographs of the United States Geological Survey. Vol. VI. The Older Mesozoic Flora of Virginia. By William Morris Fontaine. Washington: Government Printing Office.

Monthly Weather Review for April, 1885. Washington: Signal Office.

Report on Water Supply for Salt Lake City. By Charles L Stevenson.

A Manual of the Theory and Practice of Topographical Surveying. By J. B. Robinson, C.E. New York: John Wiley & Sons.

This treatise deals with the use of the transit and stadia.

The writer explains in the preface the plan of the work as follows:

"The writer has three objects in view in the preparation of this work, viz.:

"*First*—To prepare a manual that could profitably be put into the hands of students in surveying, and then be of further use to them in the field.

"*Second*—To so clearly and minutely explain the theory and methods of field work, that an engineer in practice could, without other instruction, prepare his instruments, and do the work in good shape.

"*Third*—To furnish such means of reducing the field notes, with such methods of plotting as have resulted from many years' experience of many engineers engaged in the business."

The Winslow tables, with which our readers are doubtless familiar, are appended to the text.

An Introduction to Practical Organic Analysis. By George E. R. Ellis. London: Longmans, Green & Co.

This is a convenient little manual for chemical students. It is particularly good in specifying the characteristics of the leading organic acids and alkaloids, and in the directions for general examination.

The work is well printed, and is made to have the appearance of being illustrated by the ingenious process of repeating a simple diagram three or four times. This is a harmless practice when no illustrations are called for nor referred to.

The Elements of Railroading. By Charles Paine. New York: Railroad Gazette.

This is a series of essays reprinted from the *Railroad Gazette*. How extensive the variety of topics presented may be seen by the following list of chapter headings: Surveying and Construction, Real Estate and Records, Drainage, Main Track, Trackmen and Sidings, Stations, Shops and Engine Houses, Telegraph Lines and Fences, Locomotives, Cars, The Movement of Freight, The Movement of Passengers, Employes.

Of course each of these important subjects is treated only in a cursory manner, as the thirteen chapters cover only 154 small pages. Many good suggestions are afforded young engineers in the way of caution about common errors in railroad construction and maintenance.

The Technology of Bacteria. By Chas. S. Dolley, M. D. Boston: S. E. Cassino & Co.

This work is designed to stimulate students to microscopical research. It is divided into three parts. Part I. is devoted to the directions for obtaining organisms from various sources and their preparation for the microscope. Experimental culture of Bacteria receives a fair share of attention in this part.

Part II. deals with the special methods employed by Pasteur, Koch, and many others, to propagate Bacteria of particular maladies.

Part III. describes the use of the preservative and coloring fluids used in connection with the preparation of minute organisms for the microscope.

The absence of illustrations will render a microscope manual a necessity to a beginner.

Friction and Lost Work in Machinery and Mill Work. By Robt. H. Thurston.

The author is so well known that we need not devote space to showing that the book is of a highly practical and thoroughly scientific character.

Nothing more than a statement of the range of topics seems called for.

Chapter I. treats of the Theory of Machinery and deals with precise definitions of scientific terms.

Chapter II. on Nature, Laws and Theory of Friction, is probably the most important in the book, as the author has made this a special subject of research.

Chapters III., IV. and V. treat of Lubricants, their application, and the chemical and physical tests to which they should be subjected before application in important cases.

Chapters VI and VII. deal with Experiments, Testing Machines and the Coefficients of Friction.

Chapter VIII. treats of the financial aspect of the question, and exhibits calculations of cost of oil, loss of useful effect, etc.

It is a worthy addition to the list of useful books by this author.

MISCELLANEOUS.

The South Foreland Lighthouse Experiments.—The long series of experiments made during last summer and autumn at the South Foreland to test the respective merits of oil, gas, and electricity for lighthouse illumination, have been brought to close, and the reports will now be drafted and published. Pending their appearance, which will necessitate some delay owing to the voluminous character of the observations, we may mention that the evidence is in favor of the electric light on the whole, because of its far-penetrative power. While the rays of oil and gas lights were lost sight of at a distance of eight miles. the electric light could be seen for fourteen miles; and with gas or oil at its maximum power, and visible for 10 miles, the electric light could be seen for 14½ miles. Although the arc is very much reduced in fogs, it still appears to be visible farther than gas and oil. By direct testimony of observers, the electric light became visible in dense fogs at a distance of 1500 feet to 1900 feet, whereas gas and oil were only discerned at distances of from 1250 feet to 1500 feet. This is an important point, because it was thought gas and oil might have the advantage over electricity in dense fogs. There is no doubt that fog does quench the direct rays of the arc light in a remarkable manner; but how much this would diminish its efficacy in a practical sense was not well known hitherto. Probably the white haze surrounding the lamp is detected for a considerable distance, although the arc itself in a fog has lost its brilliance. So long as this haze is seen, mariners will learn to distinguish it as the lighthouse. The heating due to the use of gas was found to be very considerable. With the original 88-jet burners the temperature rose to 200 deg. Fahr. but the subsequent use of 108-jet burners brought it to 300 deg. and 350 deg. Fahr. These burners gave an illuminating power of 2400 candles. The electric arc lamps used gave a light of 12,000 candles each. Four of the gas burners were superposed in a quadriform arrangement, and three arc lights were sometimes used. For the oil lights the Trinity House six-wick burners were employed, each giving a light of 800 candles. We may add that although the experimental trials are for the present over, the photometric observatory and testing towers will be allowed to remain for further use and testing purposes. This is a sensible decision, since whatever the results of the trials at present, they cannot be regarded as conclusive while the subject of illumination is engaging so much attention.

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THE RELATIVE VALUE OF TIDAL AND UPLAND WATERS IN MAINTAINING RIVERS, ESTUARIES, AND HARBORS.

By WALTER RALEIGH BROWNE, M.A., M. Inst. C. E.

Proceedings of the Institution of Civil Engineers.

I.

The two natural agencies which tend to keep clear the channels of tidal rivers and estuaries are: the tidal flow, *i. e.*, the run of the tide, passing up and down such channels twice in about every twenty-four hours; and the low-water flow, *i. e.*, the current, mainly of fresh water, passing through such channels at the end of each ebb tide, and up to the beginning of the next flood. The object of this paper is to investigate the relative value of these two agencies, in preserving the full depth and section of such channels.

The determination of this relative value is a matter of very great moment in many practical questions of marine engineering, and much discussion has taken place upon it. The reports for 1845, 1846 and 1847 of the Commissioners on Tidal Harbors in particular, contain many opinions, and a considerable amount of information, bearing on the subject, but they cannot be said to furnish a solution of the problem.

One preliminary remark must be made, viz., that no universal rule can be laid down, and that each case must be judged on its own merits. This remark is so much an accepted dictum in matters of engineering, especially marine engineering, that it may appear superfluous. But in this particular instance a universal rule has been laid down and supported by engineers of eminence, that no encroachment on tidal waters should ever be permitted; in other words, that nothing should ever be done to diminish the amount of tidal water flowing up and down a channel. The object of this rule is to prevent the silting up of the channel, and its ground can only be the assumption that the main, if not the only, cause which prevents this silting up is the tidal flow. In the author's opinion, this assumption cannot be maintained, and grounds quite as solid might even be found for exactly the opposite rule, viz., that tidal water should be wholly excluded from a channel, wherever possible. As already stated, the author declines to lay down any universal rule; but his study of the subject has led him to accept as a principle, that the main and essential agent which keeps clear the channels of tidal rivers is not, in general, the tidal, but the low-water flow.

This principle may be supported by the following line of argument:

(I.) The silt, which tends to choke up tidal channels, is almost wholly due to the tidal water, and not to the fresh water.

(II.) The tidal water brings up more silt on the flow than it takes down on the ebb; *i. e.*, on the whole, it tends to choke the channel, not to scour it.

(III.) The low-water flow, if left to itself, scours away the deposit and keeps the channel open.

(IV.) Therefore, where the two act together, the scour must be due mainly, if not entirely, to the low-water flow, and not to the tidal flow.

(I.) The silt, which tends to choke up tidal channels, is almost wholly due to the tidal water, and not to the fresh water. In some quarters the idea still seems to prevail that the mud of tidal rivers is all brought down by those rivers themselves. But if this were true, then rivers, above the tidal area, would carry as much silt as below; and since they are there much smaller, the amount of silt per cubic foot, or the muddiness of the water, would be greater. The contrary is notoriously the case. Let any one compare the current of the Severn at Worcester with the immense mass of turbid water which occupies the Bristol Channel; or even the Thames at Richmond with the Thames at Erith. A good example on this head is furnished by the river Parret, at Bridgewater. This river is formed by several streams, which spring from the sandstone and limestone hills of Somerset, and unite in a great level plain many miles above Bridgewater. Any small amount of silt which they might wash down in their earlier course, would be deposited in this plain long before it could reach Bridgewater. But above and below that town the banks are regularly cut into level benches, for the sake of collecting the silt, which is eventually made into what is known as "Bath brick." The silt is deposited on these benches with great rapidity, requiring to be excavated every two or three months, and many thousands of tons are thus abstracted from the river annually, an amount which would be utterly beyond the capacity of the river itself to supply in that limited area.

To complete the proof, it is only necessary to answer the question, Where does this silt come from? In the case last described, this is an easy matter. At Stert Point, close to where the Parret debouches into the Severn, the river can be seen washing and undermining, for a considerable distance, a vertical bank of sandy clay, exactly resembling the silt which it deposits higher up. In other cases, such as the Severn, the silt is due to the constant motion of the waves beating upon the wide muddy flats which lie between high and low water. The mud on those flats, originally derived from the eating away of alluvial lands near the estuary, combined with a small portion of mud brought down by the river from inland, has been rising and settling alternately under the variations of weather, probably for centuries.

The effect of waves, even of small ripples, in stirring up mud, has been well illustrated in a paper by Mr. Charles Richardson, M. Inst. C. E., read before the British Association, at Bristol, in 1875. Speaking of the Avon between Bristol and Avonmouth, he says: "At the ferry from Pill to Lamplighters, the Lamplighters shore is entirely alluvial, and it will be found that during a quiet season the mud on that shore will rise to a level of 5 or 6 feet, on both sides, above the level of the ferry path. If, after this, a continuance of strong westerly winds sets in, the ripple on the water will gradually wash away the accumulated mud again, until it becomes almost level with the ferry path. This fact proves that the tidal waters do of themselves have a tendency to deposit mud, but that the ripple, caused by the wind, washes it away again in exposed situations near the mouth of the river."

(II.) The tidal water brings up more silt on the flow than it takes down on the ebb; *i. e.*, on the whole, it tends to choke the channel, not to scour it.—That tidal waters do bring up the silt from the large estuaries, where it is raised by wave action, into river channels, is made clear by section (I). That this silt is rapidly deposited at the bottom, during the period of slack water at the top of the tide, is an unquestioned fact, and follows from the laws of the mechanical suspension of solids in water. That a certain quantity of the silt so deposited is removed again during the run of the ebb, and carried out to sea, may also be granted. The question is, whether the whole, or only part, of the silt is so removed. If the latter, then a certain quantity is left behind after each tide, and unless some other agency super-

venes, the channel will gradually but surely silt up. Now, that only part and not the whole of the silt is so removed may be proved, in the author's opinion, as follows:

(1) To ascertain the effect of any cause, the best course is to examine cases where that cause alone is operative. This, in the matter of tidal scour can fortunately be done. The banks of any tidal and muddy river, between high and low water, are fully exposed to the action of the tidal scour, and to that alone. It is well known that these banks are generally covered, from top to bottom with a thick deposit of silt. This deposit has certainly been brought up by the flood tides, and as certainly has not been swept away by the ebb. Here, therefore, is a proof that tidal waters cannot sweep away as much mud as they deposit.

(2) It might be argued that this only applies to the upper portions of these banks, which, from the rapid fall of the tide, are not so long exposed to its influence. But, apart from other considerations to be subsequently mentioned, not only does the deposit retain about the same thickness from the top to the bottom of the slope, but in muddy waters the rise and fall of a single tide is sufficient to leave a distinct film of silt over the whole surface of that slope. For instance, during tidal work at or near low water in the Avon near Bristol, the planks, wheelbarrows, &c., had always to be washed before commencing operations at each ebb tide.

(3) It may be asked how it happens, if this deposit is always going on, that the channel is not finally choked up. The answer is that the deposit does accumulate, until the angle at which it lies becomes greater than the angle of stability of this soft and wet material; a portion then slides down into mid-channel, to be carried away by the low-water flow, and the accumulation commences afresh. This is well described in Mr. Richardson's paper already alluded to, as follows:

"Now, if the river channel be carefully observed at low water of a spring tide, flakes of mud may be observed to slip down into the low-water stream here and there as the tide recedes; the hollows these mud-slips leave are characteristic, and are plainly observable all along the river at low water. These patches of mud slip into the low-water channel as they lose the support of the tidal water and are then carried out into the Severn by the stream of fresh water. This process is constantly going on, and is evidently the way in which the channel is kept at its regular width, notwithstanding the increasing deposit of mud by the tidal waters at every tide."

(4) It is known (see Bouniceau, "Etude sur la navigation des rivières à marées"), that the amount of deposit is always less in winter than in summer. Since the tidal flow is the same in winter and in summer, this can only be due to the deposit being swept away by the winter freshes; and it therefore shows that the deposit diminishes as the scouring power of the low-water flow increases.

(5) Hitherto the question has perhaps been complicated by the low water and the tidal flow being both present in the same river channels. But it is easy to find examples where the former is entirely absent. In all such, so far as the author is aware, the result is the same, viz., that complete silting up does actually occur, unless artificially prevented. Every half-tide basin is a case in point, and it is well known that in muddy waters these have always to be kept to the proper depth by artificial means. Two special cases of a more general character may be given as further illustrations.

The first refers to the mode in which the Avon debouches into the deep water of the Severn at King Road. In former years the fresh water was discharged through two channels, embracing between them Dumball Island, and named respectively the Swash and the North Channel. The latter was the larger, and was chiefly used by the shipping. When the Bristol Port and Pier railway was made from Bristol to Avonmouth, two timber jetties, with landing-places and pontoons, were constructed in the North Channel, to enable goods and passengers to be landed for conveyance by the railway. About twenty years ago, however, the tugs and other steamers plying between Bristol and places down the Bristol Channel found that they saved time, as well as distance, by taking the Southern or Swash Channel, whenever the tide was sufficiently high to admit

of it. The passage of these steamers, often almost scraping the bottom, gradually deepened the Swash Channel, until the whole of the low-water flow was diverted down it. From that moment the North Channel, though still exposed to the run of the ebb-tide down the Avon and the Severn, began to slit up; and so rapidly has this process gone on that no vessels can now use this entrance, the landing-places and jetties have been completely abandoned; and Dumball Island bids fair, in a few years, to be connected with the mainland by an expanse of mud only overflowed at high tides.

The second case is that of the Chesel Pill, near New Passage on the Severn. A "pill" in this district means a tidal creek, into which are discharged, through a sluice, the waters of an inland water-course or "rhine." A plan of the Chesel Pill is given in Fig. 3. The sluice originally stood at Redwick, about a mile inland, and the channel below this was tidal. Some years ago it was resolved, for the benefit of the level, to dam the stream nearer to its mouth. For this purpose a new sluice D was built within the horse-shoe bend ACB, and a straight cut was made from A to B. The original course was then dammed at B, and the new cut opened, causing the water to pass through the new sluice D. The results have been as follows: (1) Above the sluice D the high banks, which were formerly covered by the tidal waters only, have been overgrown with grass, the fresh-water channel remaining the same; (2) Below the sluice D the whole channel remains as deep as ever, in spite of its having lost the scour of all the tidal water which formerly ran up to the old sluice; (3) The horse-shoe ACB has completely silted up, although it has had the daily scour of the tide running up to B.

These two cases have been dwelt upon as peculiarly instructive; but ordinary experience and observation in such matters are enough to teach that a creek, or any other space, left exposed simply to the flux and reflux of the tide, will in time silt up to something like high-water level; and this can only be because tidal waters tend on the whole to choke and not to scour.

(III.)—That the low-water flow, if left to itself, scours away the deposit and keeps the channel open, is evident from the remarks already made as to the Chesel Pill, which forms a salient instance of the truth of this statement. Further, ordinary rivers, in parts above tidal influences, keep, with slight oscillations, a constant regimen, and require no artificial means to maintain their channels. The only exceptions are where rivers, previously charged with silt, descend into wide and level plains, such as those of Lombardy. But these cases rarely occur in tidal waters, where the fall practically coincides with the slope of the land, towards the sea, and is seldom very small. As confirmatory of this it may be mentioned that even in tidal waters the bottom is kept clean by the low-water flow. This has been proved in the Avon, where, in the course of the improvements, it was desired to deepen the channel for some distance below the Clifton Suspension Bridge. Although the tidal banks are here covered with mud to the depth of some feet, and the water is always thickly charged with sediment, the spoil, brought up by the dredger, proved to be entirely composed of gravel and stones, so clean that it only needed the breaking up of the larger pebbles, and the addition of lime or cement, to form excellent concrete. An exactly similar layer of clean gravel, mixed with bones, trees, &c., is found over the whole of the alluvial lands of the Avon, covered by about 25 feet of tidal silt. This must be the old bottom of the river, silted over as the stream gradually worked its way from one side of the valley to the other. What can be the cause of this phenomenon, unless the silt, which is certainly deposited after each tide (and most thickly in the deepest water), is regularly swept away by the low-water flow, in the period before the next tide begins?

The author has now, he believes, succeeded in establishing the three propositions stated at the outset, as constituting the premises of his argument. If so, there appears to be no means of avoiding the conclusion, (IV.) that where the tidal and the low-water flow act together, the scour of the channel must be mainly due to the latter, and not to the former. A few subsidiary arguments may be added. The scour of a low-water flow is in its nature self-regulating. The velocity, and therefore the scouring power, of the water, de-

Fig. 1

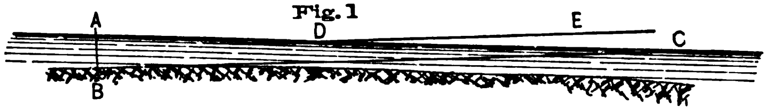
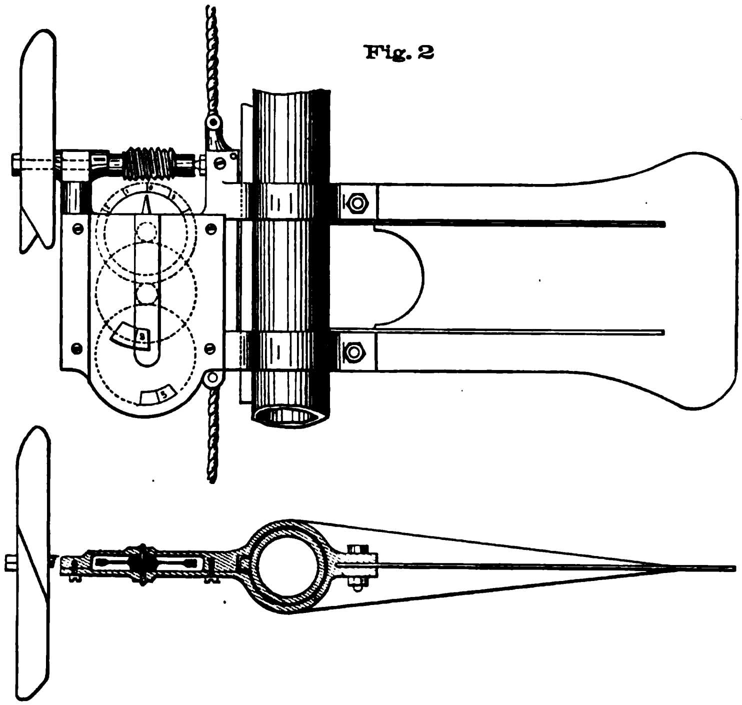
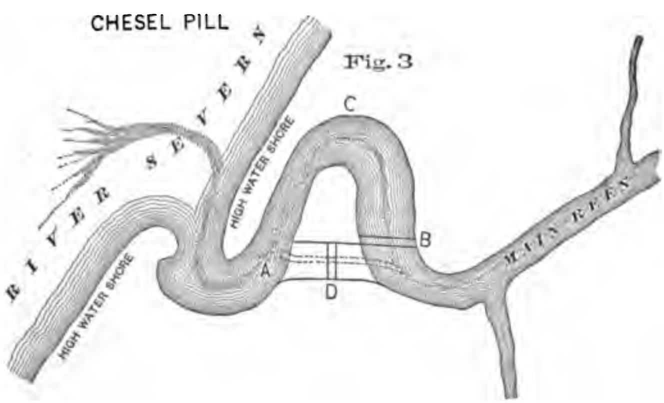


Fig. 2



Current Meter. General View and Plan. ¼ full size.

Fig. 3

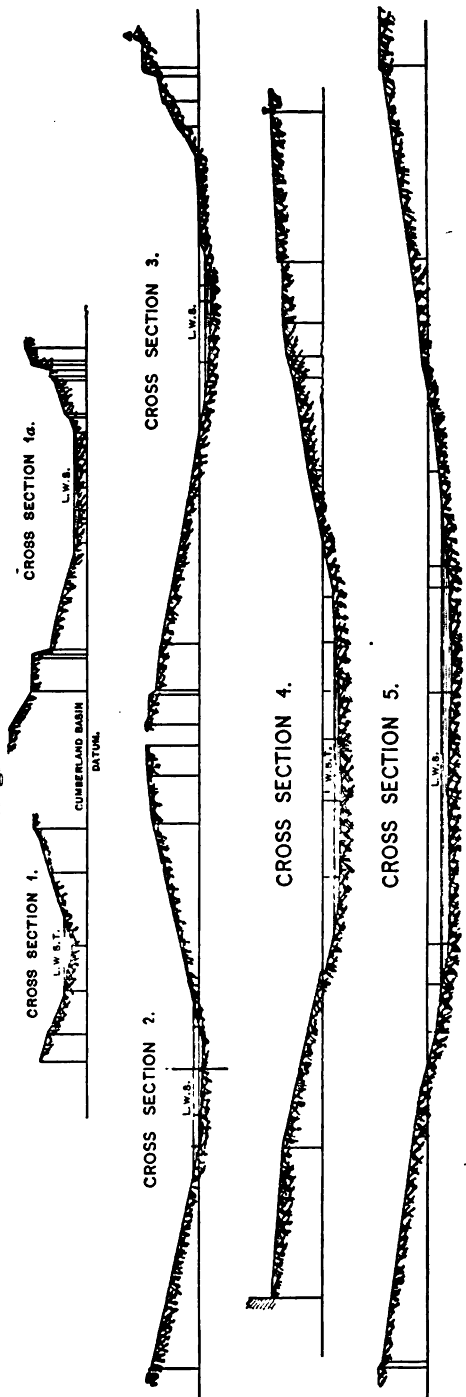


pend on the slope of the bottom. If the bottom be anywhere too deeply cut into, this slope, from thence downward will be lessened; the velocity will thereby fall, and the bottom will silt up again. But the scour of tidal flow would seem to be of an opposite character. It is due to the flowing down of a mass of water, following the fall in the level of the ocean outside; and in proportion to the size of this mass, so apparently would be its scouring effect. Hence a tidal channel,

in which the amount scoured out once becomes larger than the amount deposited, should go on enlarging indefinitely. It is needless to say that such a case is unknown. On the contrary, there is good reason for the belief that, in alluvial soils, the section of a channel always bears a fixed relation to the amount of fresh water coming down. This is illustrated by the accompanying cross sections of the Avon, Fig. 4, Sections 1—5, token at different points between Bristol and King Road. It will be seen that the low-water stream occupies a similar position at the bottom of each section (becoming somewhat larger in the lower sections), and that the mud banks slope up from this at a regular angle, which is less at the sections near the mouth, because the mud there is softer and wetter. In this view the author is supported by Mr. Richardson, who has been a close observer for many years of such phenomena in the Bristol Channel, and who says: "If any one will look carefully at the different streams in the neighborhood, he will see little streams with little estuaries, medium streams with middle-sized estuaries, and big streams with large estuaries; the channel formed being always in proportion to the amount of fresh water. This cannot be accounted for by any other supposition than that the volume of the fresh water determines the size of the estuary, and that the tidal waters have nothing at all to do with it."*

But whatever may be the worth of the above argument as to the slight value of tidal, compared with low-water scour, it is desirable to check it, if possible, by direct experiments. Now, obviously, the scouring action of any current is due solely to the layers of water in immediate proximity to the sides and bottom; and the main body, flowing above, has no direct effect. This has been frequently ignored, and perhaps not unnaturally. If an observer stands on the brink of such a river as the Avon, and sees the ebb-tide rushing past, at a speed of sev-

Fig. 4.



* It is obvious that another factor enter directly into the determination of the size of an estuary, viz., the size of the valley of which the estuary forms part; but as the size of the valley will as a rule govern the size of the stream which flows down it, the effects of the two factors are generally the same. Of course this does not hold when the valley has been excavated by ice or otherwise in solid rock, like the glens of Scotland or of Norway.—W. R B.

TABLE I.—GRAEVE'S AND SCHLICHTING'S OBSERVATIONS.

Reference No. of Experiment.	Depth.	Surface or Maximum Velocity.	Bottom Velocity.	Ratio of Bottom to Surface Velocity.	Mean.
	Meters.	Meter per second	Meter per second		
S. I.	1.4	0.636	0.4926	0.77	—
S. V.	1.8	0.551	0.3296	0.60	—
S. II.	1.9	0.714	0.4752	0.69	
20	2.0	0.79	0.50	0.64	
23	2.0	0.82	0.48	0.59	
22	2.0	0.75	0.43	0.56	
24	2.0	0.78	0.50	0.64	
29	2.0	0.83	0.46	0.55	0.61
33	2.1	0.786	0.46	0.585	
49	2.1	0.78	0.47	0.60	
21	2.1	0.79	0.52	0.66	
53	2.1	0.78	0.45	0.58	
28	2.1	0.84	0.53	0.63	
15	2.2	0.83	0.45	0.54	—
17	2.2	0.78	0.49	0.63	—
14	2.3	0.82	0.50	0.61	—
32	2.4	0.805	0.48	0.596	—
48	2.4	0.81	0.48	0.59	—
52	2.4	0.82	0.47	0.57	—
9	2.5	0.82	0.52	0.64	—
11	2.5	0.82	0.56	0.68	—
10	2.6	0.81	0.44	0.54	—
7	2.7	0.82	0.52	0.64	—
6	2.8	0.87	0.55	0.63	—
31	2.8	0.855	0.55	0.64	—
39	2.8	1.12	0.55	0.49	—
47	2.8	0.83	0.55	0.64	—
40	2.8	1.19	0.55	0.45	—
51	2.8	0.86	0.55	0.64	—
42	2.9	1.13	0.55	0.49	
1	2.9	0.88	0.55	0.57	
41	2.9	1.08	0.57	0.53	
44	2.9	1.19	0.67	0.56	
46	3.0	1.10	0.60	0.545	
36	3.0	1.18	0.70	0.59	
50	3.0	1.10	0.60	0.55	
43	3.0	1.16	0.60	0.52	0.55
54	3.0	1.10	0.60	0.54	
45	3.0	1.07	0.65	0.61	
35	3.1	1.14	0.62	0.54	
37	3.1	1.13	0.60	0.53	
2	3.1	0.89	0.50	0.56	
38	3.1	1.18	0.70	0.59	
35	3.1	1.14	0.57	0.50	

eral feet per second, he is easily led to the idea that an immense work of scouring must inevitably be going on. But a moment's reflection will show that it is only the velocity at the surface which he sees; and that unless he knows the ratio of this surface velocity to that at the bottom, he really knows nothing as to the intensity of the scour.

It is thus a matter of great importance to determine the ratio between the surface and the bottom velocity in tidal currents; and this point the author set himself to investigate. The data which he was able to discover were at first extremely meagre. While a multitude of experiments are recorded on the surface and middle velocities of rivers, yet, owing perhaps to the difficulties of measurement, the bottom velocities have been generally omitted, or at least have not been recorded with exactness.

Mr. W. Bald states that the bottom velocity of the tidal flow in the Clyde is about $\frac{3}{5}$ that at the surface, but unfortunately the depths are not mentioned. Rankine gives the ratio as $\frac{3}{5}$ for ordinary, or $\frac{1}{2}$ for very slow currents.

The best observations the author was at first able to meet with were those of

Graeve in the Oder and the Warthe, published in the "Civil Ingenieur." But these experiments were only in depths varying from about 2 to 3 meters (6 feet 7 inches to 9 feet 10 inches) while even in small rivers like the Avon the tidal current has a depth of more than 30 feet. The author, however, prepared the following Table I., showing, for gradually increasing depths, the surface velocity (or the maximum velocity where this was not exactly at the surface), the bottom velocity, and the ratio of the two, deduced from Graeve's experiments. The figures added for the depths of 1.4 meter, 1.8 meter, and 1.9 meter, are from Schlichting's observations on the Memel.

These results present considerable anomalies, especially in the figures for the depth of 2.8 meters. Taking, however, a mean of the observations from 1.9 meter to 2.1 meters, and also of the observations from 2.9 meters to 3.1 meters, it appears roughly that the ratio of the bottom to the surface velocity has diminished, between the depths of 2 meters and 3 meters (6 feet 7 inches and 9 feet 10 inches), from 0.61 to 0.55, or by about 10 per cent. It is clear that if anything like this rate of diminution goes on as the depth increases, the velocity at such depths as 20 to 30 feet must always be a mere fraction of that at the surface. This conclusion is confirmed by two experiments at such depths, given in Révy's "Hydraulics of Great Rivers." These, reduced to French measures for comparison, are as under:

Depth.	Surface Velocity	Bottom Velocity.	Ratio of Bottom to Surface Velocity.
Meters.	Meter per sec.	Meter per second	
6.9 (22 ft. 8 in.)	0.593	0.256	0.43
7.4 (24 ft. 4 in.)	0 549	0.178	0.32

It is to be observed, moreover, that such observations cannot be taken exactly at the bottom (Mr. Révy's were 1 foot from it), and that the diminution in velocity close to the bottom is very rapid, as is shown by Graeve's curves of velocities, at different sections, four of which are reproduced in Figs. 5 and 6.

Fig. 6

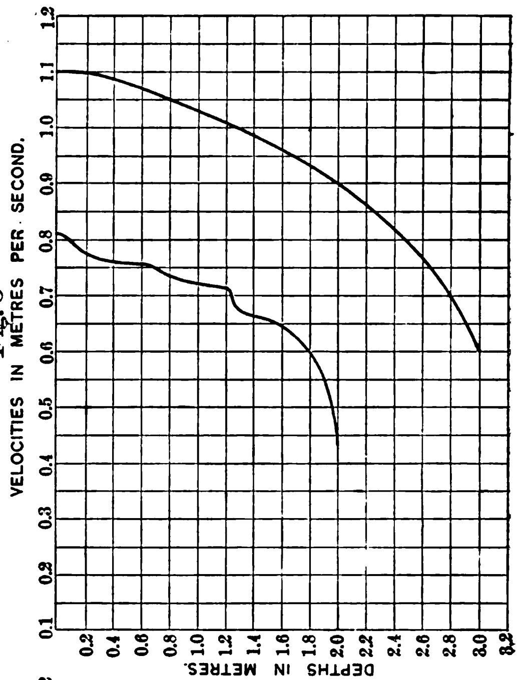
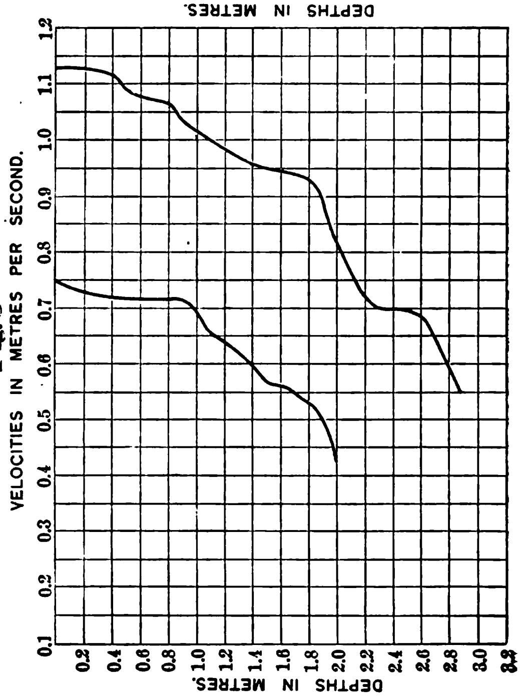


Fig. 5



The author's attention was subsequently directed, by Mr. Robert Gordon, M. Inst. C. E., to the Report of the Chief Engineer of the U. S. Army for 1878, which contains a valuable record of ex-

periments made by General Ellis in the Connecticut river, at a part where the width varied from 1,400 to 1,000 feet and the depth exceeded 20 feet. The velocities at the surface, and at various depths from thence to the bottom, were measured, partly by floats but chiefly by meter. With the meter, observations could be got as near to the bottom as 0.4 foot in some cases; with the floats they could not be nearly so close, and with these the results as to bottom velocities are so irregular that the author decided to reject them. The meter observations also presented several anomalous results, some in one direction, some in another, and these also have been rejected. The remainder are grouped, in Table II., into two classes, according as the surface velocity was above or below 2 feet per second; a division practically suggested by General Ellis, and justified by the result.

From this table it appears that for low velocities the results are too irregular to allow any rule to be formulated, but that the ratio of bottom to surface velocity is always small, varying on the average from 0.40 to 0.20, and in some cases falling as low as 0.03. In the higher velocities the results are far more regular; the average ratio for the observations at 7 feet and 8 feet is 0.61, and for those at 20 feet and 21 feet is 0.44, showing a diminution between those depths of 28 per cent. The latter ratio, 0.44, agrees closely with one of Mr. Révy's, given above, and the results as a whole certainly go to confirm the conclusion already formed.

Another series of experiments, to which the author was also directed by Mr. Gordon, are contained in the Report to the King of Holland on Dutch Public Works, for the year 1873. These experiments were carried out at several places in the various channels of the Rhine and Meuse. These channels, though not deep, are about 500 yards wide, and the results appear to indicate an intermediate position between first-class rivers, such as the Mississippi and the Irrawaddy, on the one hand, and ordinary rivers on the other. There is a considerable difference, generally ranging from 40 to 50 per cent., between the records of the surface and bottom velocities; but the ratio is irregular, and it does not decrease with increase of depth to the same extent shown by the experiments given in Tables I. and II. The detail of some of these experiments is given in Table III.

The above observations were all made in ordinary fresh-water rivers, and, in the author's opinion, would not fully represent the circumstances of the ebb in a tidal channel a short distance from the sea. An ordinary river in a condition of steady flow, may be taken for theoretical purposes as being of unlimited length, and a horizontal line, B C (Fig. 1), drawn through the bottom of any section A B, will cut the surface at some point, C, determined by the average fall of the river. The triangle of water, A B C may be considered as acted on by the full force due to the pressure on A B, and will move with a velocity such that the work done by this pressure is just sufficient to overcome the various resistances to the flow. At every point between B and C the conditions will be the same, and the water will be moving with the same steady motion. But the case of a tidal channel is different. There the water at some intermediate point, D, falls into an estuary or ocean, whose waters are either at rest, or at any rate are moving in a direction different from that of the current in the channel. The waters of the channel, below the level D E, can only make their way into this estuary by displacing the waters already there; and to do this must require some expenditure of energy, especially as the estuary waters will, in general, have the higher specific gravity of the two. The result must be a back pressure on the water at B, additional to the resistance of friction, &c., and, in consequence, a retardation of its velocity. This retardation or loss of energy will apparently be greater as the depth increases; and, therefore, near the bottom, where the speed is in all cases low, it would seem probable that, in tidal channels of this character, the velocity may be reduced to a very small amount, or may even disappear.

Influenced by these considerations the author became anxious to obtain some actual observations on the velocity at the bottom of such channels during an ebb tide. Some time elapsed before such an opportunity presented itself, but at length, in March, 1880, Mr. Thos. How-

TABLE II.—OBSERVATIONS ON THE CONNECTICUT RIVER.

Velocity above 2 feet per second.						Velocity below 2 feet per second.					
Depth.	No. of Experiment.	Surface velocity.	Bottom velocity.	Ratio.	Mean Ratio.	Depth.	No. of Experiment.	Surface velocity.	Bottom velocity.	Ratio.	Mean Ratio.
Ft.						Ft.					
7	34 35 33 28 32	3.88 3 39 3.99 3.96 3.75	2.22 2.29 2.81 2.18 2.86	0.57 0.67 0.70 0.55 0.76	0.65	7	—	—	—	—	—
8	26 27 26	3.68 4.07 3.10	1.83 2.09 1.91	0.50 0.51 0.62	0.54	8	48 70	1.03 1.38	0.50 0.41	0.49 0.40	0.44
9	—	—	—	—	—	9	81 88	1.48 1.38	0.30 0 28	0.20 0.21	0.20
10	29 11	4.05 2 54	2.00 1.48	0.49 0.58	0.53	10	23	1.61	0.53	0 33	0 33
11	—	—	—	—	—	11	—	—	—	—	—
12	10	2.65	1.34	0.51	0.51	12	54 80	1.06 1.70	0.46 0.46	0.43 0.27	0.35
13	—	—	—	—	—	13	39 47	1.27 1.54	C 04 0.61	0.03 0.40	0.21
14	79	2.10	1.34	0.59	0.59	14	14 22	1.29 1.83	0.25 0.72	0.19 0.39	0.29
15	21	2.14	1.09	0.51	0.51	15	59	1.23	0.64	0.52	0.52
16	2 30 20 78	3.12 4 30 2.17 2.18	1.43 1.89 1.14 1.09	0.46 0.44 0.52 0.50	0.48	16	46 53	1.66 1.30	0.56 0.29	0.34 0.22	0.28
17	8	3.36	1.58	0.47	0.47	17	18	1.57	0.73	0.47	0.47
18	77	2.49	0.97	0.39	0.39	18	17	1.60	0.46	0.29	0.29
19	—	—	—	—	—	19	15 19 73 75	1.49 1.62 1.37 1.91	0 36 0.64 0.43 1.24	0.24 0.39 0.31 0 C4	0.39
20	44	2.54	1.07	0.42	0.42	20	—	—	—	—	—
21	42 3 41	3.03 2.31 2.80	1.43 0 96 1.30	0.47 0.42 0.46	0 45	21	—	—	—	—	—

ard, M. Inst. C. E., Docks Engineer to the corporation of Bristol, kindly placed men and materials at the author's disposal, and the necessary experiments were undertaken and carried out with much zeal and ability by Mr. H. S. Hele Shaw, Assoc. M. Inst. C. E., Whitworth and Miller scholar, and assistant to the Professor of Engineering, University College, Bristol. The observations were taken at a point, shown in Fig. 5 (Plate 1), on the "New Cut," at Bristol, be-

TABLE III.—EXPERIMENTS ON LOWER RHINE.

Width of river 460 meters; distance from bank to nearest section, 70 meters.

Date of Experiment.	Number of Section.	Depth.	Surface or Maximum Velocity.	Bottom Velocity.	Ratio of Bottom to Surface Velocity.	Mean.
		Meters.	Meter.	Meter.		
July 11	V.	2.6	1.095	0.70	0.64	0.63
" 6	V.	2 85	1.085	0.65	0.61	
July 22	IV.	3.4	1.395	0.825	0.59	
June 10	IV.	4.0	1.27	0.85	0.67	
July 15	V.	4.7	1.34	0.93	0.69	—
" 6	IV.	4.9	1.265	0.65	0.51	—
July 6	II.	5.4	1.71	1.20	0.70	—
July 22	III.	6.5	1.515	0.875	0 58	0.575
" 6	III.	6.6	1.535	0.85	0.55	
June 30	III.	6 6	1.475	0.77	0.52	
July 15	III.	7.0	1.62	1.025	0.63	
" 15	III.	7.2	1.725	0.95	0.55	
" 12	II.	7.5	1.775	1.10	0.62	

tween the Bathurst and Cumberland basins. This cut is nearly straight, and is well leveled at the bottom, being excavated in the New Red Sandstone rock; it is, therefore, eminently suited for such observations, as it is free from errors due to bends in the river, or to inequalities in the bottom. The observations were made by means of a current meter, kindly lent by Mr. Baldwin Latham, M. Inst. C. E. The observed velocities at the surface were checked by floats, and in almost all cases the meter gave the higher value, so that its error, if any, was one of excess. The bottom velocities were taken by means of a strong rod, let down perpendicularly from the boat to the bottom, and having a piece of sheet iron, 8 inches square, on the end to prevent its sinking in the mud.* The meter was passed down this rod as far as it would go, and was then allowed to run for a fixed time (five or three minutes). It was then stopped, pulled up to the surface, and the reading taken. The middle velocities were determined in the same manner. The results are contained in Table IV., and although, like all current observations, they present some anomalies, they will be found very instructive. The small difference, varying from 1 foot 1 inch to 2 feet 1 inch, between the surface level at Avonmouth (where the Avon debouches into the Bristol Channel, 7¼ miles lower down) and that at the place of observation, brings out strongly the difference already pointed out between this case and that of an ordinary river. It is only in the last series, G, that the ordinary conditions may be said to appear. Unfortunately, no levels were taken on the tide gauges after 1 P. M.; but, from the rapid increase in the difference between the Bristol and Avonmouth levels from 12.40 to 1 P. M., it may be conjectured that at 1.20 P. M. this difference would have amounted to at least 4 feet, while at the same time the depth at the place of observation had diminished to 7 feet. The conditions thus approximated to those of an ordinary river, and it will be seen that here and here alone, the bottom velocity becomes considerable, and its ratio to the surface velocity approaches the ratios given by Graeve for similar depths.

The bottom velocities shown in Table IV. are very small, and not in themselves sufficient to move even the finest silt. That in E3 was 2 feet from the bottom, and it will be seen from F3 and F4 that the ve-

* This was found practically to be needless, as the bottom was clean rock; a fact which confirms the previous assertion as to the power of low-water flow to keep the bottom clear.

TABLE IV.—EXPERIMENTS ON VELOCITIES OF THE EBB TIDE IN THE RIVER AVON, MARCH 31, 1880.

Ebb commenced about 10.25 A.M. Depth at high-water, about 23 feet 6 inches.

Series and Number.	Total Depth at Time of Experiment.		Position of Meter.	Time of commencing Experiment.		Duration of Experiment.	Distance run by Meter.	Velocity by Meter.	Surface Velocity by Surface Floats.	Level of Surface at Avonmouth below Surface.	
	Ft.	In.		H.	M.	Minutes.	Feet.	Ft. per Second.	Ft. per Second.	Ft.	Ins.
A 1	22	11	At surface	10	55	5	627	2.09	1.9	1	5
2			11 feet below	11	10	5	312	1.04	2.6		
3			1 foot from bottom	11	3	5	55	0.18	2.6		
B 1	21	5	At surface	11	17	5	902	3.01	3.0	2	1
2			9½ feet below	11	32	5	309	1.03	—		
3			1 foot from bottom	11	24	5	38	0.13	—		
C 1	18	4	At surface	11	40	5	932	3.11	3.1	1	1
2			8 feet below	11	55	5	905	3.02	—		
3			1 foot from bottom	11	47	5	39	0.13	—		
D 1	15	2	At surface	12	2	5	905	3.02	—	1	2
2			7 feet below	12	19	5	901	3.00	3.3		
3			1 foot from bottom	12	12	5	26	0.09	3.0		
E 1	12	9	At surface	12	25	3	699	3.89	3.3	1	10
2			6 feet below	12	35	3	564	3.09	3.3		
3			2 feet from bottom	12	38	3	244	1.36	3.3		
F 1	10	10	At surface	12	40	3	524*	2.92	3.3	1	11
2			5 feet below	12	56	3	614	3.41	3.3		
3			2 feet from bottom	12	45	3	300	1.67	3.3		
4			9 in. "	12	51	3	19	0.11	No observation.		
G 1	9	0	At surface	1	0	3	689	3.83	Do.	3	3
2			7 feet from bottom	1	5	3	528	2.94	Do.		
3			5 " "	1	10	3	524	2.91	Do.		
4			3 " "	1	16	3	514	2.86	Do.		
5	7	0	1 foot "	1	21	3	417	2.32	Do.		

locity at 2 feet may be considerable when that at 1 foot is practically zero. These velocities, however, are given only because they were actually recorded on the meter, and not as being absolutely correct. Preliminary experiments had shown that it was not possible satisfactorily to put the meter into gear while it was at the bottom, and, therefore, during these experiments, it was put into gear before being lowered into the water. The effect of course was that the meter was running during the time it took to descend through the current, and that the distance thus run must properly be subtracted from the distance indicated at the end of the experiment.† If this subtraction were made there could be little doubt but that the bottom velocity, already very small, would turn out to be absolutely nothing. This was confirmed by those who were conducting the experiments noticing that the small plate which has been mentioned as attached to the bottom of the rod, whenever it was lifted to the top of the water, was found to have a deposit of fine silt on its upper surface. This seems to prove that, at the very moment when an ebb tide is running down at a speed of 3 to 4 feet per second, not only may there be

* Probably there is an error in this reading.

† It will be noticed that in the later experiments, where the depth was less, the distance thus run was also less.

TABLE V.—EXPERIMENTS ON CURRENT VELOCITY IN THE AVON, SEPTEMBER 21, 1880.

Ebb commenced about 8.40 A.M. Depth at H.W. about 26 feet 5 inches.

Position of Meter.	Time.		Depth of Water.		Surface Velocity by Floats.	Velocity by Meter.
	H.	M.	Feet.	Inches.	Feet per Second.	Ft. per Second.
Surface	9	40	22	8	—	3.57
Bottom	9	50	21	0	4.1	0.00
6 feet from bottom	9	55	20	0	4.3	2.70
Bottom	10	10	17	4	4.5	0.00
Surface	10	15	16	10	4.6	4.60
Middle	10	21	16	0	—	3.40
2 feet from bottom	10	25	15	9	4.3	0.05
Middle	10	30	15	2	—	3.44
Surface	10	33	14	10	—	4.60
2 feet from bottom	10	36	14	6	—	0.00
4 feet from bottom	10	38½	14	0	—	1.00
Bottom	10	44	13	6	—	0.00
Middle	10	49	13	0	—	3.65
Surface	10	52	12	6	—	4.60
1 foot 6 inches from bottom	11	2	11	4	—	2.40
Middle	11	6	11	0	4.4	3.80
Surface	11	10	10	8	—	4.08
Bottom	11	13	10	4	—	3.30
Surface	11	16	10	0	4.6	4.04
Bottom	11	20	9	6	—	3.30
Middle	11	24	9	2	—	3.40
¼ depth	11	27	Not taken.		—	3.50
¾ depth	11	30	Do.		—	3.40
Surface	11	33	8	4	4.1	4.04
Bottom	11	37	8	0	—	3.35
Middle	11	40	7	8	—	3.50
¼ depth	11	43	7	4	—	3.87
¾ depth	11	46	7	2	—	3.40
Surface	11	50	7	0	3.6	3.90
Bottom	11	55	6	9	—	2.87
Middle	11	58	6	8	—	3.20
Surface	12	1	6	6	—	3.60
Bottom	12	8	6	4	3.4	2.70
Middle	12	11	6	0	—	3.00
Surface	12	18	5	10	—	3.20
Bottom	12	22	5	9	—	2.35
Middle	12	24	5	9	—	3.00
Surface	12	25	5	8	3.5	3.07
Bottom	12	34	5	7	—	1.91
Middle	12	37	5	6	—	2.60

no scour whatever going on at the bottom, but an actual deposit of silt may be taking place. It is obvious that the theoretical views already stated have met with the fullest confirmation.

In order, however, that this point might be put beyond all doubt, the author arranged with Mr. Shaw to make a fresh set of experiments with improved apparatus, and these were successfully carried out on the 21st of September, 1880. In this case, following an ingenious suggestion of Mr. Shaw, the rod on which the meter was mounted (Fig. 2) was made with a feather on it, which fitted into a recess in the socket of the meter. By this means the meter, which was specially constructed for the purpose by Mr. Shaw, could be turned in any direction with regard to the current by simply turning the rod. When being lowered through the water the meter was thus held in such a position that the screw had no tendency to revolve at all, a position which was found to be nearly, but not quite, at right angles to the direction of the current. When it reached the required depth (which was marked

upon the rod), the meter was shifted, by turning the rod, into the position parallel to the current, in which it was maintained by the vane; and it then began immediately to revolve. Its scale of measurement was carefully ascertained by preliminary trials, in comparison with floats—a ship's log, which was first used for the purpose, not proving sufficiently accurate; and it was so carefully adjusted and lubricated that the slowest current was sufficient to cause it to rotate. Under these conditions the experiments were carried out, and their results are given in Table V.

On this occasion the depth of high water at the place of observation was about 26 feet 5 inches, and the ebb tide began at about 8.40 A. M. The first observation was taken at 9.40 A. M.; the ebb had then been running for about one hour, and had lowered the surface level by about 3 feet 9 inches, or to a depth of 22 feet 8 inches. The first observation showed that the conclusion drawn from the former set of experiments, viz., that there was absolute stillness at the bottom, was correct, for while the velocity at the surface was 3.57 feet per second, that at the bottom was zero. The observations were repeated with the same result, till about 11 A. M., when the ebb had been running about two and a-half hours, and the depth had fallen to about 11 feet, or 15 feet below high water level. The bottom layers of water then appeared to start into activity, and to assume a velocity which from that period continued to bear a tolerably uniform ratio of about 0.7 to the surface velocity. The experiments were ended at 12.37 P. M., when the whole of the ebb was over, four hours after it had commenced, and one hour and a-half after the bottom velocity had become apparent. The depth was then 5 feet 6 inches, and the velocity, 1 foot from the bottom, about two-thirds that at the surface, a result which agrees very fairly with the conditions for an ordinary river, as determined by Graeve's and Ellis's observations.

The foregoing experiments, combined with others, appear to justify the laying down of the following rules as to the relation between bottom and surface velocities in various cases.

A. In the largest rivers the bottom velocity may for the present purpose be taken as the same as the surface velocity, varying within small limits only. This seems to be fully established by the observations of Humphreys and Abbott on the Mississippi, and more recently by the numerous and careful observations of Mr. Robert Gordon on the Irrawaddy, some of the MS. records of which he has most kindly placed at the author's disposal. In such cases the whole mass of water appears to move almost like a solid body, independently of the resistances of the sides and bottom. It is possible that the same may hold of great tidal estuaries, such as that of the Mersey, but on this the author offers no opinion, as he is unable to find any observations of bottom velocities made under such circumstances.

B. In rivers of ordinary size, such as the Connecticut and Memel, the bottom velocity bears to the surface velocity a ratio which diminishes as the depth increases, but with too much irregularity to allow of a formula being constructed. Roughly, the ratio may be said to be about three-fourths at a depth of 5 feet, one half at 15 feet, and probably one-third at 25 feet.

C. In ordinary tidal channels, such as the Avon below Bristol, the course of events during an ebb seems to be as follows: At first the slope of the surface is exceedingly small (in the Avon it was about 1½ foot in 7½ miles), and, while the velocity at the surface is considerable, it diminishes rapidly from thence downwards, and at some distance from the bottom becomes *nil*. This continues for about two-thirds of the ebb, the surface velocity increasing up to a certain point, and then becoming nearly constant. During all this time, not only is no scour going on at the bottom, but, if the water be muddy, an actual deposition of silt is taking place. At this time, after about two-thirds of the ebb, the water has fallen about three-quarters of its total height, the slope of the surface has considerably increased, and the conditions approximate to those of an ordinary river. The bottom layers of the water then spring suddenly into motion, and the ratio of bottom to surface velocity is from thenceforward approximately as in the last paragraph, the surface velocity diminishing steadily as the tidal waters dis-

appear, until it assumes the normal rate of the low-water flow. During this period a scour of the bottom is of course going on; but as the velocity is not much higher than in the subsequent period of low-water flow, the rate of scour will not be much greater, and the actual scour will be insufficient to compensate for the amount of deposit from the tidal waters which has taken place, not only during the period of high water, but also during the first two-thirds of the ebb. It must follow, therefore, that the scouring effect of the ebb tide is little or nothing, and the observed incapacity of tidal flows to sweep away the silt they have deposited is amply and satisfactorily explained.

The general investigation as to the relative value of tidal and low-water scour on river channels may now be considered as concluded. It was begun by adducing numerous facts to establish the proposition that the effect of tidal flow was to cause channels to silt up, while the effect of low-water flow was to keep them open. It was then pointed out that the former circumstance could only be explained by the hypothesis that the current at the bottom of such tidal channels (the only current which has any scouring effect) was very slow, even when the surface current might be rapid. Observations were quoted to show that in ordinary rivers this diminution of velocity does exist to a large extent, and that it increases rapidly with increased depth. Theoretical reasons were then given for believing that in tidal channels this reduction of velocity would again be much larger than in fresh-water rivers, otherwise similar. Lastly, it was shown that this view was amply borne out by direct experiments, which have demonstrated that in some cases, at least, the bottom velocity is not only reduced, but destroyed, and that silt may be settling on the bottom when the speed of the surface current is at its highest.

To complete the subject, however, it must be observed that there are certain circumstances in which the passage of tidal waters through a channel may have some effect in keeping it open. These are, briefly, any circumstances in which the tidal waters are so far kept back on the ebb, that a portion of them remain to increase the velocity of the low-water flow. Thus a large tidal basin may be supposed to be connected with the sea by a very narrow sluice. Then the flow through the sluice would not be able either to fill or to empty the basin with sufficient rapidity to keep pace with the variation of level outside; hence, on the ebb, it would be low water outside for some time before the current would cease to run through the sluice; and the waters thus impounded would go practically to increase the volume of the low-water flow, and no doubt to add to its scouring effect. This is probably a very rare case. A common one is that of a harbor having a narrow entrance, but expanding inside into a wide extent of mud flats. A large quantity of water soaks into these flats during flood tide, when they are covered with the sea; and when the tide has ebbed this water oozes out again, not immediately, but gradually, and, winding through innumerable depressions and channels, finds its way to the main entrance. It thus forms, as it were, a spurious low-water flow, which, if the inland waters entering the harbor are small, may have a decided effect in preserving the depth of this main entrance. In the author's belief, it is in a great measure from the observation of such cases, which are common in some localities but rare in others, that the strong predilection in favor of tidal scour has taken its rise.

It remains to inquire how far these views agree with the facts and opinions adduced by various authorities on the question, and also to consider briefly their practical effect on some of the problems of marine engineering.

It has been already mentioned that the chief discussion of the subject, as far as English literature goes, is to be found in the reports of the Tidal Harbor Commissioners for 1845, 1846 and 1847. From these reports may be quoted the evidence of Mr. James Walker, Captain Washington, Sir John Rennie, Mr. D. Stevenson, Mr. Scott Russell, and others, as strongly insisting on the value of tidal scour, and deprecating any interference with tidal areas under any circumstances. But the value of these opinions is somewhat lessened on account of the very meagre substratum of facts on which they appear to be based. Thus special reference is made to Southwold as a case where great injury was done to a harbor by embank-

ing lands inside, and so diminishing the amount of the tidal flow. But from Mr. Walker's evidence the true nature of the injury appears to be this: The action of the tides on the East coast, as is well known, is to keep a bank of shingle continually traveling parallel to the shore, at a certain depth below high water. So long as the harbor in question was kept open to its full extent, the stream issuing from thence on the ebb was sufficient to sweep away the encroaching shingle as fast as it advanced, and so to preserve a clear entrance. After the embanking this was no longer the case; the shingle (aided, according to Mr. Ellis, by a strong easterly gale) pushed forward in spite of the current, formed a bar across the tide-way of the basin, and thus made it comparatively inaccessible. This statement indicates a most peculiar case, which must be treated on its own merits, but must not for a moment be allowed to have any weight on the general question of the relative value of tidal and low-water flow.* Again, in the case of Rye, Sir John Rennie states, that the harbor was greatly deepened by the blowing up of a sluice, which enabled the tide to run further up the country. But it appears that the failure of the sluice was due to a heavy land fresh; and it seems pertinent to ask, whether the deepening of the harbor might not possibly be due to the land fresh also. In its general history Rye appears almost a typical case of a harbor gradually silting up from the deposit of tidal mud, the fresh-water flow being insufficient to keep it open. Beyond the evidence, such as it is, of these two cases, the author has been able to find little but simple expressions of opinion, and general axioms, of a more or less doubtful character.

Nor is the weight of opinion entirely on one side. Mr. William Cubitt, while disposed to attach considerable value to tidal scour, is most emphatic in pointing out that there are no principles of universal application to harbor engineering, and that there may be many cases where to embank lands would not be injurious, and to open them to the sea would not be beneficial. Mr. John Murray and Mr. W. C. Mylne, in their reports recommending the "dockizing" of the River Wear, speak of the excellent results achieved in France by sluicing from back-water reservoirs, or, in other words, by forming an artificial low-water flow. This is confirmed by Sir John Macneill in his report on Hartlepool Harbor, where he points out that the channel was kept open by sluicing, while it had silted up when left to the operation of the tide. To this testimony may be added that of perhaps the earliest of English marine engineers.

Smeaton's report on Wells harbor is an admirable account of what may be considered almost a typical case of the effects of scour. This harbor receives practically no inland water, but has a narrow entrance, and a considerable extent of marshy flats inside, through which the tide used formerly to find its way by small creeks and channels. Smeaton's report shows that the harbor has always been in a state of slow decay from the deposit of silt; but that the entrance was kept partially clear by the tidal water, which, oozing among these creeks and channels, gradually found its way back to the ocean; that, to improve this scour, a sluice, or rather a dam with one narrow opening, was erected to form a back-water reservoir; that this, for the whole time it lasted, proved efficient in keeping the harbor open; that, when it fell into decay, the authorities, instead of repairing it, chose to expend their money on a lawsuit with certain parties who had embank a portion of the marsh lands; and that the natural consequence of this policy was the destruction of the harbor. In commenting upon these circumstances, he makes the following remarks, which in the main coincide with the author's views, and are well worth careful consideration:

"The reason why the waters passing the sluice have a greater effect in scouring than those which return to sea without passing the sluice, is, because by the contracted opening of the passage of the sluice, the waters that lie in the creeks behind it are detained from ebbing so quickly as they otherwise would have done; that is, their numerous mouths when always open, reduced the level of the water contained therein, to nearly the

* It appears that there was always shoal water outside the harbor entrance, and that the shingle bar formed across the top of this shoal. If so, this would bring the advancing shingle within the range of the surface-current of the ebb, and enable that current to have its proper scouring effect.—W. R. B.

same level as that of the water in the main channel of the harbor, being stopped by dams made across and united by cross passages into one, and the mouth of this being contracted by the work called the sluice, a body of water is held back in these creeks, as reservoirs, which not being able to escape so fast as the tide ebbs in the main channel, it follows, that a body of water by these means is vended upon, and after the half ebb, which discharging itself into the harbor's creek, forms a scour when the depth is so much lessened as to operate with power in grinding the bottom, which otherwise would have been so languid as not to have stirred a grain of sand or mud, in which case its effect would be little or nothing. This artificial scour thus procured, in some degree imitates the effect of a fresh-water river, which in these situations is very greatly beneficial, not from any virtue there is in fresh water preferable to salt in these cases (if anything rather less on account of its less specific weight), but from its having a fall from the land, and proceeding therefrom continually it not only strengthens the ebb, but running to sea at low water when the fall being greatest, and the sandy bottom exposed to its action, it continues to work at a time when it can operate to the best advantage; and when the ordinary current of a river is assisted by extraordinary land floods and freshes from downfalls of rain and snow, and this operating at low water, when, as just remarked, the fall is the greatest, in such cases it is capable of producing extraordinary effects, and of keeping a harbor continually open with a channel of a given magnitude, though loaded with sands in any possible degree; for a fresh-water river has this peculiar advantage, that at the same time that it strengthens the scouring power of the ebb, it operates most forcibly at low water when there is the least to obstruct its operation; it opposes the tide of flood from the sea, and thereby prevents its bringing so much sand and silt into the harbor as otherwise it would.

"The defect therefore of this sluice of Wells is, that though it retains the waters so as to be behind the general ebb, and thereby strengthens the latter part of it considerably; yet being at low water all spent, when the greatest good might otherwise be obtained, it loses that good effect which would be had from a fresh-water river, or from a proper sluice; that is, one that will retain the water wholly till a proper time of tide, and then being let go in one collected body, is capable in a short space of time of producing marvelous effects."

Lastly, the author may venture to cite, in support of his views, the following passage from the important work of Bouniceau ("Etude sur la navigation des rivières à marées," 1845, p. 68). After speaking of the way in which estuaries become charged with silt, especially after storms, he continues: "Deposits are thus formed throughout almost the whole tidal course of the river; the ebb carries back only a part of these deposits, and the next tide, passing over the remainder, takes up a part, and deposits it higher up the river; so that the finest silt is borne to a great distance from the mouth. Each tide thus augments the quantity of the deposits thus formed, until a fresh in the river scours away the whole of this alluvium, bears it to the sea, and deposits it outside the mouth. It follows that, when the river is low, the depositing power of the flood tide exceeds the scouring power of the ebb, aided by the fresh water, and by the natural slope of the channel; during freshes, on the contrary, the latter power exceeds the former. It will be seen, therefore, that the ebb would not alone be able to keep open the entrance of a river, did not considerable freshes occur from time to time to increase its effect. On our coasts the ebb only serves partially to maintain the channel, until the winter comes and re-establishes it, scouring the bed, and enabling it to form in the next summer a vast reservoir for scouring purposes. If the fresh waters, and mainly the freshes, did not thus lend their aid to the ebb, if, from any cause, their flow was interrupted, rivers whose estuaries are charged with mud would be soon silted up by the action of the sea, and before long would be turned into cultivable land. A striking example of this effect has been given by the Somme. A lateral canal had been constructed beside this river from Abbeville to St. Valery. As this was large enough to convey the whole of the waters, it was decided to dam the channel near Abbe-

ville, and thus direct the fresh waters from their accustomed channel. The silting up of the bed of the Somme was the almost immediate consequence of this alteration."

Finally there has to be considered briefly the practical application of the principles brought out in the course of this inquiry. These principles may be stated as follows: The scouring power of any current depends wholly on its velocity at the bottom. This bottom velocity, except in rivers of the largest size, is much less than that at the surface, and in a ratio which decreases rapidly as the depth increases. In the case of a deep tidal channel not far from the mouth, it appears to be absolutely *nil* for at least the greater part of the ebb. Such channels, if muddy, are never kept open by the operation of tidal scour alone. Hence, in general, excluding the largest rivers and estuaries, the maintenance of the channel is mainly, if not entirely, owing to the low-water flow.

This flow is, of course, chiefly due to the fresh waters of the rivers; but the rivers are swollen slightly, perhaps, in all cases, and considerably in some, by the salt waters which continue to ooze out from the muddy banks or flats when not covered by the tide. This addition to the fresh waters will, of course, make the depth at any point, and therefore the slope and the velocity, greater than it would be if the fresh water acted alone. This greater velocity will no doubt give a certain advantage in scouring power. But the extent of this advantage cannot be estimated offhand. Theoretically, however, it may be roughly gauged as follows: Take the section whose bottom is exactly at the same level as the surface of the water outside the mouth—a level which will obviously be the same whatever the discharge of the river may be. Then the depth at this section will be proportional to the slope. Let this depth be H, and the mean velocity V. Then, by the ordinary principles of hydraulics, $V^2 = CH$, where C is a constant. Suppose that it be ascertained by measurements that half of this depth H is due to salt water, and now consider the effect of diminishing the area covered by the tide to such an extent that half of the salt water, which at present goes to swell the low-water flow, is subtracted. Believers in tidal scour would no doubt assert that the scour, in other words, the bottom velocity, would be reduced by one-half also. To see how far this would be the case, let h be the new depth, v the new velocity. Then, since the discharge is only $\frac{3}{4}$ of what it was before,

$$v\,h = 0.75\ V\,H.$$

But $h = \frac{v^2}{C}$, and $H = \frac{V^2}{C}$; hence, $v^3 = 0.75\ V^3$, $v = 0.91\ V$; in other words, the mean velocity will be diminished, not by 50 per cent, but by 9 per cent. only. The bottom velocity will be diminished, if at all, by a much smaller percentage, since it has been shown that the ratio of bottom to surface velocity increases rapidly as the depth decreases.

The above, which is merely an illustration, will suffice to indicate the course which it seems desirable to adopt in advising on the question, whether a given diminution should be allowed to be made in the tidal area of any particular river. The low-water discharge at some point below the proposed embankment should be carefully measured, and the total discharge of the inland or fresh waters should be measured at their entrances into the tidal area. The difference between the two will show the extent to which the low-water flow is increased by tidal waters.

The effect of the proposed operations may then be judged of by ascertaining how far they will reduce, not the cubic content of water passing up and down on each tide, but the area of mud, which is submerged by the tide, and thence contributes to the low-water flow. It should be noticed, however, whether this mud lies nearly horizontal, as inside a harbor, or inclined at a considerable angle, as on the banks of a river; since, in the former case, it will clearly retain the water longer, and therefore be a more effectual feeder, than in the latter.

This process is neither difficult nor costly; and the author submits that it would, at least, give a more satisfactory result than the course, at present common, of condemning embankments altogether. It would seem that embankments are only to be feared where the fresh-water flow is exceedingly small, or its current exceedingly sluggish. Many rivers have been embanked, especially in

early times, with favorable, rather than unfavorable, results. That the results should be favorable may, at first sight, seem impossible, but the author's experiments suggest a ready explanation. It will be remembered that the bottom velocity of the Avon remained at zero until the surface had sunk by about three-quarters of its total fall. This sinking is, of course, due to the passing away of the waters sent up by the flood tide. Now, if the Avon were embanked say, by building quay walls on each side from low-water mark, the content of these tidal waters would be reduced, but the slope of the surface and the velocity of the ebb would remain as before. Hence, the level would fall more rapidly, and the point at which the water at the bottom began to move would be at an earlier period of the ebb. The result of this would be to diminish the time during which sediment was being deposited, and lengthen the time during which it was being scoured away; and, therefore, it would be favorable, not unfavorable, to the maintenance of the channel.

Among recent instances of such embanking may be mentioned those at Aberdeen, at Lynn, and on the the Forth; and among early cases of the kind, it is impossible to forget that the three largest rivers of England, the Thames, the Severn, and the Trent, were all embanked in the course of the middle ages, with results the reverse of disastrous. Had the modern school of engineering existed in those times, it is at least probable that all those embankments would have been condemned and forbidden, and, in consequence, England would have failed to acquire many hundreds of square miles of her most fertile lands.

There is another operation which may be applied to tidal rivers, besides that of embanking their shores, *i. e.*, the process of "dockizing," or of excluding the tide altogether by a dam at or near the mouth, so as to turn the river, above this point, into a floating harbor or canal. Any individual case of this kind will have many peculiar circumstances, which must be carefully considered before a decision is arrived at, and no attempt will be made here to discuss the general question. But if there is any truth in the foregoing investigation, it at least shows the groundlessness of an objection frequently made to such proposals, namely, the fear of some unknown injury to the river thus treated, or to the estuary into which it flows. It appears that wherever silting takes place in a channel it is due to the tidal waters laying down a greater amount of deposit than they can sweep away; and that from this fact all tidal rivers, at least of ordinary size, would gradually silt up, if it were not for the corrective influence of the fresh-water flow. Thus, looked at from the point of view of maintaining the channel, the entrance of tidal water into an ordinary river can be nothing but an evil; whilst any deposit that may result in the estuary outside can be easily removed by judicious scouring at low water, to the effect of which the author can bear witness from his own experience. It would seem, therefore, that this mode of turning dangerous rivers into canals deserves fuller consideration than of late years it has received. It is well known that Smeaton recommended the "dockizing" of the Clyde, and it has since been remarked how happy the Corporation of Glasgow may esteem themselves in that they rejected his advice. The author ventures to think otherwise, and the labor of preparing this paper will have been sufficiently recompensed if it help to vindicate the wisdom of the first, and perhaps the greatest, of English marine engineers.

At a meeting of the Meteorological Society in June, a paper was read on "The Mean Direction of Cirrus Clouds over Europe," by Dr. H. H. Hildebrandsson, Hon. Mem. R. Met. Soc. The author has collected a number of observations on the movements of cirrus clouds over various parts of Europe, and after discussing them, has arrived at the following results:—(1) The mean direction at all stations lies between south-west and north west; (2) in winter the cirri come from a more northerly direction, and in summer from a more southerly; in winter the northerly component is greater on the Baltic and the north coast of the Mediterranean; (4) the mean directions of the upper currents nearly coincide with the mean tracks of storm centers; (5) the upper currents of the atmosphere tend in general to flow away from those areas in which a barometrical depression exists at the earth's surface towards those in which there is an elevation of pressure.

A SKETCH OF ROMAN BUILDING CONSTRUCTION.*

By W. T. OLDRIEVE.

From "The Building News."

To disentangle the architectural conceptions of a people from that which is borrowed or inherited by tradition, leaving that which can justly be called their own, is a task by no means easy. Architectural writers have, in attempting it, entered upon an arena of strife, which to the amateur and the professional student, is not a little perplexing. It is only necessary in this "sketch" to assume that we may legitimately call that Roman which the Romans first made a principle of construction or design, although the primary forms adopted may have been previously used by other nations. It is proposed to consider the subject under—I. Principles and forms of Roman construction. II. Processes and methods. III. Materials and workmanship.

I.—PRINCIPLES AND FORMS.

Although the Romans used the three "orders," it was in some important respects in a different manner to that in which the Greeks made use of them. The Romans did not rest satisfied with them, nor did they enter wholly into the spirit of an epistyle construction as the Greeks, whose purity of form and delicacy of line were never appeciated by the Roman architects. While the architecture of the Greeks is expressive of an artistic and refined simplicity, that of the Romans is expressive of vast resources, indomitable energy, and great constructive skill. The Greeks so designed their buildings that no decoration or adornment was needed as a necessity to make them artistic, for the artistic spirit is in the construction. Not so is it, as a principle, with the architecture of the Romans; the form which covers the construction is often independent of it. This may be stated as a first principle in Roman construction. Undoubtedly the most characteristic feature in the buildings of the Roman period is the arch. The Romans first used it in a true sense architecturally and consistently, although its use can be traced back, in a more or less perfect form, to almost all the ancient nations. In adopting the arch as an architectural feature, the column and the entablature were not discarded by the Romans, though they show by their great vaults and domes that they could constructively have been dispensed with. It is generally assumed that in the combination of arch and epistyle in the Roman façade, the arch and wall are Roman, and the column, &c., are borrowed from the Greeks. Semper has entered a protest against this assumption, and asks why the column should be borrowed by the Romans from the Greeks when both nations belonged to the same family and started with the same traditions, and when the column is found as early in Italy as in Greece. It is probable that the column and entablature were used as the natural construction, and the wall and arch introduced in the buildings of several orders in height, as the Colosseum, to support the upper stories as well as to form an outer screen. A feature was thus added which gave increase of strength, while preserving the columnar principle. Viewed in this way the Roman combination of column and arch is not an architectural scandal, though this is frequently taught. It is considered not improbable that this combination was introduced from some of the Hellenistic cities, though its introduction does not seem to have been clearly traced. The first Roman example is believed to be the Tabularium at Rome, B. C., 78. Another combination of the same nature, though less defendable, is the use of piers or mullions in an arch-headed aperture. This can only be mentioned here. The Pantheon at Rome exhibits, in its front external elevation, the curious effect produced by the pedimented portico placed in front of a domed circular structure. Fergusson and others have held that the portico is a later addition, but authorities are pretty generally agreed that this is not so. It is in the use of the arch principle as a vault that the Romans have gained pre-eminence. Vaulting was no

* Being the Cousin Prize Essay in the Class of Fine Art, Edinburgh University, Session 1884-85.

doubt used earlier in underground tunnels, &c., and the Hellenistic city of Mitylene in Asia Minor is supposed to have furnished the model for the first Roman stone-constructed theater on flat ground, B. C. 50, where arches supported the rows of seats; while a writer of about B. C. 40, referring to the city of Alexandria, says that the buildings were without boarding or timber of any kind, so that vaulting seems to have been in common use there. In no part of the architecture of the Romans can we get a better knowledge of the principles which guided them in their treatment of the arch. Although the vast extent of their undertakings seems at first sight to indicate that they were lavish in their expenditure of labor and materials, a more careful examination shows that there was really a feeling of economy, and that there is a correspondence between the extent of the building and the means adopted. By their scientific and constructive skill in the use of the vault, the Romans produced in their structures perfect solidity and incomparable grandeur with a minimum of expenditure. In following out our next division of the subject under consideration, we shall be able to trace in detail the carefully considered economy of parts and labor which was practiced in Roman buildings, as, for instance, the brick rib and concrete filling principle of vaulting, and the hidden buttresses in the thickness of the walls of the Pantheon, whereby greater resistance to the pressure of the vaulting was obtained than if the walls, 20 ft. thick, had been solid, though only half the quantity of materials was used.

II.—PROCESS AND METHODS.

It will be necessary, in order to keep within the limit of a short class-essay, to confine our inquiry to Roman building construction in its most limited sense, not giving much attention to the great engineering works of the Romans in the shape of viaducts, bridges, &c., which they have left in so many parts of the world. Again, of the processes and methods of build construction adopted by the Roman, only those which are peculiarly characteristic need be referred to. The arch and vault were constructed both of stone and of brick and concrete; the round arch, the barrel and the domed vault being almost the only forms used. When of stonework, the arch stones, or "voussoirs," were carefully hewn to the proper form and built in alternate bands, thus economizing the centering. The old Roman baths at Nimes (now called the Temple of Diana) offer a good example of this construction. Another form of stone barrel vaulting was constructed with stone ribs and slabs between, and is interesting as bearing a resemblance to what is found in some early Christian churches in Syria. It seems a little astonishing that the Romans with all their constructive skill, avoided groining in their stone cross-vaults. Wherever square stone-vaulting met, the stone construction would be stopped and the vault worked in brick and concrete. In sloping vaults, as the entrance passages to the amphitheaters, the arching was sometimes executed in stepped rings, each separate arch standing vertically, and thus exerting no pressure upon the lower part of the vault. The inside of the stones would, in this construction be dressed off to the slope, probably after the stones were set in position. In the passages with converging sides the voussoirs were sometimes arranged thus, with the line of keystone and the lines of springing stones of parallel widths, the filling-in stone being wedge-shaped on plan. At other times the side and key stones were wedge-shaped on plan and the filling-in stones were parallel. In the construction of the oblique arch, the abutments of the stone rings were built at right angles to the faces of the rings, the stones being cut to the required obliquity. The brick and concrete vault is most characteristic of Roman construction, although it does not date further back than the time of Augustus. It is thought probable that this too came from the Hellenistic cities. Had the great Roman vaults been "cast" entirely of concrete, the timber centering would have shrunk during the execution of so extensive a work and caused cracks in the concrete which might have led to an entire collapse. Viollet-le-Duc calculated that the centering for the dome over the Pantheon would have sunk about 20 inches in from three to six months if constructed over the whole area at once, like an inverted basin. To avoid this, and to give elasticity to the whole covering, the Romans constructed light ribs of brick and filled in between them with concrete. The

brick ribs, unlike those in the Gothic periods, were not intended to be decorative features and so were not permanently exposed, The concrete filling was economical, only easy straightforward labor being necessary. I may also mention a building on the Palatine at Rome. Here the ribs are made up of cells formed by large flat bricks, 2 ft. square, and smaller bricks 2 ft. by 6 in., their thickness being 2 in. The cellular construction was sometimes applied to the whole vault, as at the Palatine, and as here illustrated. The cells gave support to the concrete, both by adhesion and by their wedge shape. The concrete filling between the brick ribs appears to have been sometimes arched on the upper surface; but it is not very easy to see how this could have added much to the strength of the vault. The great example of Roman barrel vaulting is that over the Basilica of Constantine, near the Colosseum at Rome, the span being no less than 80 ft. The casettes here, as in the Pantheon, and other examples, greatly reduce the weight of concrete, while not causing any decrease of strength, for where the casette is most deeply sunk, an approximation to the form of an arch is formed, as shown by dotted lines on sketch. The wood moulds fastened to the centering before the concrete was filled in would greatly assist in securing the ribs in their position laterally. Exception might perhaps be taken to the smaller sinkings in the face of the ribs, as in the Pantheon. If the ribs were intended to be displayed, these sinkings would certainly be most objectionable, but the objection is at least answerable when we recognize that the principle of the Roman architects was not to display the parts of construction, but to construct a framework and apply a covering, thus following the principle of overlaying, which is observed to govern many styles of architecture. There is still another method of constructing the vault which was used by the Romans, namely—that by which the vault was formed by layers of tiles or flat bricks over the centering. The arching was reduced, as much as possible, by carrying up the wall considerably above the line of springing. This system has the advantage that skeleton centering would serve the purpose of supporting the tiles at their joints. This construction is still in use in Italy. In dealing with cross vaults the Romans preferred to have two barrel vaults of the same span, but they sometimes stilted one vault to have both crowns on a level. Oblong vaults were occasionally used, but the square were preferred. The groins in cross vaulting of the brick and concrete construction were formed in a similar manner to the ribs already described; the lower edges of the bricks, however, had to be either specially molded to the proper angle before the bricks were burnt, or cut to the exact line after being set in position. Where the groin ribs meet at the crown of vault no attempt was made to make a finished junction or miter, one rib was carried over unbroken, and the other simply butted up against it on either side. A sketch of the cross vaulting over the Therme of Diocletian will illustrate what has been said. Domical vaulting was executed by the Romans in a very similar manner to the brick rib and concrete barrel vaulting. At the Pantheon the ribs are probably of brick, and carried up of the same width to the top, where they abut against the curb of the hypæthral opening. Viollet-le-Duc thus vividly describes this master-piece of spherical vaulting:—"What, in the Pantheon at Rome, is it that produces the most lively impression? It is that immense vault which derives all its decoration from its very structure; it is that single opening for light, 26 ft. in diameter, perforated in its summit, through which the zenith is seen, and which throws upon the pavement of porphyry and granite a large circle of light. It is there that the genius of the Roman appears in full strength. So great is the elevation of this orifice above the floor, that its enormous opening scarcely affects the internal temperature. The most violent storms scarcely send down a breath of air on the head of a person standing beneath its orbit; and when it rains, the drops are seen falling perpendicularly down upon the pavement of the Rotunda, on which they describe a circle of wet. The cylinder of rain drops, falling from that height through the space of the building, renders sensible the immensity of that space. It is in conceptions like this that the Roman is really grand, because they are the outcome of his own genius, and because for their execution he borrows from no one, nor asks

the aid of any artist whose nature is foreign to his." The diameter of this building is 143 ft., the height from floor to top of dome is the same, and the springing of the vault is half way up. The spaces between the ribs are filled in with concrete and casseted or coffered, with four sinkings so arranged that a spectator in the center of floor sees all the margins of sinkings of equal width. A rosette of gilded bronze was originally placed in the center of each coffer, and bronze ornaments decorated the borders. An old writer states that he saw the manner of the construction of the dome during some repairs, and that there are series of arches. If this is the case, this outer system of arched reticulation must be regarded as an additional means of bracing the cupola. Nothing need be said of the portico, except that its roof was originally supported by bronze girders of the section of an inverted rectangular trough, which fact is interesting as pointing to the existence of so early an example of tubular metal girders. Prof. Adler has described the scientific construction of this building, and points out that the cupola is really supported by eight pillars and strengthened by the upper buttresses, the remainder of wall being merely filling in between the constructive parts. Prof. Adler's restoration of the upper story of the Pantheon is now generally acknowledged by authorities. Roman domes and vaults were not protected by outer roofs as in more modern examples, the interior only being considered by the architects.

III.—MATERIALS AND WORKMANSHIP.

The ordinary mason's work of the Romans was not executed in large blocks of stone, as the smaller were more economically worked. It is true that large blocks of stone are found, of Roman work, in the temple at Jerusalem ("Opus Quadratum"), but they are evidently in imitation of the older work of Solomon's temple. Foundations and first courses are often of stone when the other parts of walls are of brick strengthened by timber. Two kinds of exterior facing to stone walls are mentioned by Vitruvius as commonly used by the Romans, "Reticulatum" (net-like), and "Incertum," (irregular), Vitruvius says that the former was the method in general use in his day, and the latter the ancient method. A limestone called Travertine was much used in Rome; the Colosseum and other buildings being built of it. Travertine, Saxum Tiburtinum, is a calcareous deposit found along the course of the Anio and Aqua Albula, by whose waters it is deposited, but most plentifully in the neighborhood of Tivoli, the ancient Tibur, whence its name is derived. When first taken from the rock it is soft and easily cut, but the longer it is exposed to the air the harder it becomes. This may be ascertained by comparing its state in the native rocks near Tivoli with the exterior walls of the Colosseum. For less exposed work, as the interior of walls, and where covered by plaster, soft Tufa was used, and a hard volcanic substance called Pepperine, which could be exposed to the air with more freedom. Very many buildings in Rome are composed of it. Basalt was principally used in paving roads and streets; but it is also to be found employed in consolidating and packing other stones, as in the interior of the wall of Servius Tullius, &c. Pumice was only used to lighten a building, as in the vault of the Pantheon and in the Colosseum. Marble was imported for monolithic columns, and was much used in slabs for lining interior of walls, &c. It was the boast of Augustus that he had found Rome brick and left it marble. The chief supply of white marble was derived from Luna, called "Marmor Lunense," and the same quarries are still worked, the marble being well known by its modern name of Carrara. The white marbles of Greece were also occasionally employed. Of colored marbles the most esteemed are the Rosso, Verde, and Giallo Antico, and Cipollino, Africano, and Pavonazetto. Roman bricks were of different forms and sizes; flat tile-like bricks were generally used, some found 2 ft. square and 2 in. thick, others 1½ ft. by 1 ft. and 1½ in. thick. A triangular form for "opus latinitium," was also used in facing. Vitruvius devotes a chapter to a description of bricks. About the time of Constantine a practice was introduced of using large jars of baked clay (Testæ) to diminish the weight of a dome or the upper part of a wall. The Romans are famous for their mortar and cement, the excellence being due to the use of Puzzolana earth, which was used along with the lime. The Puzzolana is a volcanic substance and very

abundant in every part of the Campagna di Roma, and the extent to which it was used is shown by the vast caverns which form the catacombs. The name Puzzolana is said to be derived from the Pulvis Puteolana, which was a similar substance found near Puteoli, now Puzzuoli. Concrete was most used, as we have already seen in considering the processes and methods of construction. It was always covered with a facing of finer material. The method of laying concrete was by an even bed of mortar 6 in. to 8 in. thick, first put down, and then the stones pressed into it while soft, until the desired solidity was attained. This process was, of course, only applicable where there was resistance to lateral pressure. Plaster work (intonacum) was brought to great perfection, particularly for coating the specus of aqueducts, the interior of piscinæ, and those walls of houses which were adorned by fresco painting. Metals were not much used. The bronze beams of the portico of the Pantheon have already been alluded to. The doors of this building were of the same material. Iron was used for clamps in stonework. Lead pipes for water service are supposed to have been used. Glass was sparingly used for lighting, and as early as the time of Agrippa, was employed in mosaic work. Mosaic pavement was introduced into Rome in the time of Sylla and was largely used. Tiles appear to have been the only covering made and used for roofs. Roman works were carried out by either, 1, paid labor; 2, forced labor; or, 3, soldier labor. The materials were obtained either, 1, bought; 2, taxed; 3, procured from the quarries, &c., by forced labor. The regular workman had considerable political power. Trade guilds were formed and recognized by most Emperors, after it was found impossible to put them down. Immunities and privileges were granted on certain conditions, as that Government works should be carried out at fixed rates, &c. Time will not now permit of further notice of workmanship, and so this paper must close with a word of admiration for the constructive and organizing genius of Roman architects.

EXPERIENCE IN THE USE OF THICK STEEL BOILER PLATES.*

By W. PARKER.

From "Iron."

An ordinary cylindrical boiler of 13 feet diameter and 16 feet long, designed for a pressure of 150 lbs. per square inch, for which the scantlings were amply sufficient, burst under the hydraulic test. The pressure was applied very carefully, and when it had reached 240 lbs. the fracture occurred, extending completely across one of the shell plates, and to a slight extent, also, into the adjoining plate, as shown in diagram. The boiler was constructed entirely of steel made by the Siemens-Martin process, by a firm who enjoy the reputation of producing a material second to none in the country. The plates were all tested at the steel works and fulfilled the requirements of both Lloyd's Register and the Board of Trade. I find from our surveyor's report that the sample from the particular plate which failed—which was 1¼ inch thick—stood a tensile strain of 29.6 tons per square inch with an elongation of 20 per cent. in a length of 8 inches, whilst strips cut from it were bent almost double, cold. In fact the material appeared, from the mechanical tests applied before it left the steel works, to be in every respect suitable for the purpose for which it was intended. One remark, however, may here be made, namely, that the plate in question was exceptionally large and heavy—viz., 20 feet long, 5 feet 6 inches wide, and 1¼ inch thick, weighing about 2 tons 16 cwt.

This material was built up into a boiler by a company who have had an unusually extensive experience in the manipulation of steel, having turned out no fewer than 175 boilers of this material. The plates were treated precisely as other steel plates have been treated in the same works, and

* Paper read before the Institution of Naval Architects.

with all the appliances which experience has shown to be necessary, all the holes were drilled, and the plate was then heated in a furnace and bent to the required curvature in a pair of powerful vertical rolls in the usual manner. Under these circumstances it appeared at first sight astounding to find the material tearing under a pressure which represents a strain of 6.7 tons per square inch only, or less than one-fourth of the strain which the original sample withstood. In addition to this the appearance of the fracture indicates that the plate did not possess any ductility, stretch, or elongation whatever. Neither the steel makers nor the boiler maker have as yet afforded any satisfactory explanation of the occurrence. It is without doubt a most serious affair, especially in view of the high pressures which have now become so common. On hearing of this accident the committee of Lloyd's Register instructed me to investigate the matter, endeavor to ascertain the cause of the accident, and, if possible, recommend some measure to prevent such an occurrence in the future. My investigations were only completed last Tuesday, and as such a serious matter as this, which bears upon the probable safety of life and property at sea, must naturally give rise to no little speculation amongst engineers and steel makers, and has already produced great consternation in many quarters, I have taken this opportunity of laying before you a short statement of the facts as they have come before me, the results of my investigations, and the conclusion which I have arrived at, with a view to eliciting from the various steel makers and steel users here, the benefit of their views and experience.

Upon my visit to the boiler-making works I was fortunate enough to find a sister boiler to the one which had burst, ready for testing. This boiler was tested in my presence to 300 lbs. per square inch, and was carefully measured and gauged, and found to show no signs of deflection or yielding. I also ascertained from an examination of the testing appliances that an abnormal pressure could not possibly have been exerted at the time of the testing of the first boiler. Seeing that the plates that broke had stood all the mechanical tests required before leaving the steel works, and that when worked into the form of a boiler shell it gave way at less than one-fourth of its original strength, it appeared at first sight that the plates had been in some way injured, or had undergone some material change from the time they left the steel works until they were riveted into the form of a boiler shell; therefore it became necessary to look carefully into the mode of manipulation of the plates in the boiler-shop, and especially the heating and bending of them. One of the plates was bent in my presence. It was heated in an ordinary plate furnace, but when taken out was far from being of a uniform heat; the end of the plate near the door of the furnace was at a black heat, which gradually increased toward the other end to a dark red heat. Then the plate was turned end for end and again placed in the furnace with a view to heating it, as far as possible, uniformly; but when again drawn out of the furnace it was seen that the heat was not at all uniform, one end being of a dark red or nearly black heat, which gradually cooled down to a blue heat at the other end.

In this condition it was passed through a set of powerful vertical rolls, and bent to the required curvature. The plate passed through these rolls six times, and by the time the operation was completed, one end of the plate was quite cold, while the other end remained at a blue heat. It was thought that this unequal heating of the plate may have set up in the body of the plate excessive strains of a dangerous character, and that these strains were aggravated by rolling the plate at a dangerous heat, it being well known that the ductility of all steel becomes lessened when worked at a blue heat, and it is, I think, generally admitted that it is far safer to work steel cold, or red hot, than at any heat between these two points. Steel plates, and especially large ones, must be injured by such treatment, but as to the intensity of the strains set up, or their exact locality, nothing definite can be said. To ascertain the nature of the material as it stood, test pieces were cut from the fractured plate, both close to the fracture and apart from it, and subjected to tensile test at one of Lloyd's proving houses, with the following results, which the engineers have kindly communicated to me:

Samples.	Breadth.	Thickness.	Area.	Total Tensile Strain.	Strain per Sq. In.	Extension In 8 Ins.	Extension.	Contracted Area.
	Inch.	Inch.	Sq. In.	Tons.	Tons.	Per cent.	Inches.	Inch.
S. I. X......	1	$1\frac{13}{50}$	1.26	40.5	32.14	27.34	$2\frac{3}{16}$	$\frac{11}{16}\times\frac{7}{8}$ & $\frac{1}{32}$
S. C. H. I....	1	$1\frac{13}{50}$	1.26	41.75	33.1	26.59	$2\frac{1}{8}$	$\frac{29}{32}\times\frac{23}{32}$
S. 2.....	1	$1\frac{13}{50}$	1.26	41.5	.32.93	21.27	$1\frac{1}{16}$	$1\frac{1}{32}\times\frac{15}{16}$
S. C. H. 2 X.	1	$1\frac{13}{50}$	1.26	39.5	31.35	23.4	$1\frac{7}{8}$	$1\ \times\frac{3}{4}$
S. XX......	1	$1\frac{13}{50}$	1.26	37.5	29.7	21.8	$1\frac{3}{4}$	$\frac{13}{16}\times 1$
S. IXX.....	1	$1\frac{13}{54}$	1.26	37.25	29.56	26.6	$2\frac{1}{8}$	$\frac{31}{32}\times\frac{25}{32}$
S. XXX.....	1	$1\frac{13}{50}$	1 26	38.5	30.5	28 1	$2\frac{1}{4}$	$\frac{11}{16}$ & $\frac{1}{32}\times\frac{15}{16}$
S. IXXX....	1	$1\frac{13}{50}$	1.26	38.25	30.3	27.34	$2\frac{3}{16}$	$\frac{29}{32}\times\frac{3}{4}$

From these tests it appears that the proved tenacity of the plate ranges from 29.5 tons to 33.1 tons, while the elongation ranges from 21.8 per cent. to 28.1 per cent. in a length of 8 inches. I may say that I corroborated these tests by others made from the same plate for my own information in London, and they were also corroborated by other tests made for the information of the steel makers. This range of about 4 tons in the tensile strength of a plate of homogeneous metal like mild steel is very unsatisfactory. I obtained samples of the plate, and submitted them to five eminent and independent metallurgists, who have kindly furnished me with the results of their chemical analyses, which are as follows:

Carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese
0.36	0.015	0.055	0.087	1.05
0.27	0.016	0.044	0.076	0.641
0.33	0.010	0.038	0.065	0.612
0.30	0.018	0.044	0.063	0.648
0.26	0.005	0.038	0.067	0.650

The most striking feature in these analyses is the large proportion of carbon shown to exist in the plate. It is particularly high for boiler plates. Material used for thin plates, say, from $\frac{1}{2}$ inch to $\frac{5}{8}$ inch thick, to stand the same mechanical tests as these thick plates did, would not contain more than from 0.15 to 0.18 of carbon; and these facts led us to further experiments. In view of the great difference in the amount of carbon required in steel for a thick plate and a thin one to stand the same mechanical tests, it was deemed desirable to make an experiment which would determine to what extent work in the shape of rolling—and especially rolling thin plates, which, during the latter part of the operation, must of necessity be rolled, comparatively speaking, cold—affected the tenacity and ductility of the material. A slab of steel containing about the same amount of carbon as the plate that ruptured, viz., 33, was obtained at the steel works where the plate was made, and rolled at one heat down to $\frac{1}{2}$ inch in thickness. This material, had it been rolled down to $1\frac{1}{4}$ inch plate, judging from the carbon it contained and the tests of the broken plate, as well as the opinion of the steel makers, would have had a tenacity of from 30 to 34 tons per square inch. It was found, however, that when rolled down to $\frac{1}{2}$ inch thick, its tenacity was increased to from 35 to 41 tons per square inch, with an elongation of from 21 to 24 per cent. in a length of 8 inches. Other pieces were made hot and quenched in water. These, when tested, broke at a tenacity of from 44 to 45 tons, and had, practically speaking, no stretch at all.

Pieces were cut from the fractured edge of the plate, and subjected to tensile, bending, and temper tests. They showed a tenacity of 33.5 to 34.2 tons per square inch, but they stretched only 13 and 16 per cent., and broke with a crystalline fracture, as will be seen by the specimens produced. They bent cold to a considerable degree, but when made red hot and quenched in water, instead of bending, as pieces of a thin plate of similar tenacity and ductility would do, they broke under the first blow of a hammer without any bending whatever. The material was so high in carbon as to take a temper and become quite hard and brittle. Further cold bending tests were made from pieces of the broken plate, both before and after being annealed; those which were tested before annealing bent fairly well, strips $1\frac{1}{4}$ inch square bent to an angle of 49 and 61 degrees, the fracture showing a considerable amount of alteration in form; while those pieces which were tested after annealing, bent much better, in fact, al-

most double. Strips, however, that were heated and quenched in water, broke short without any bend whatever at the first blow of a hammer, and thus corroborated the previous experiments made in London. These experiments point to the fact that the plate which gave way must have become partially tempered by the heating and cooling to which it was subjected for the purpose of rolling it into its cylindrical form. The heating not having been uniform, the tempering could not have been uniform, and the variations in the temper, no doubt, have caused the variations in the strength and ductility shown by the different parts of the plate. The hardest part of the plate yielding less than the rest became naturally more strained, and hence the plate tore at its hardest part at a pressure only a small fraction of that which it would have borne if its yielding had been uniform.

Having thus placed before you the nature of this accident, and the steps taken with the view of unraveling the supposed mystery, I now venture to state what inferences may, in my opinion, be drawn from the results of the investigation. I think it will be acknowledged that a material which is so high in carbon as to take a temper and break short as described, even if it possesses high qualities of tenacity and ductility before being tempered, must be looked upon as unreliable and altogether unsuitable for use in marine boilers. It would appear that the desire to obtain high steam pressures, and to use steel of a higher tenacity consistent with a large amount of ductility, has caused the marine engineering world to unknowingly drift into using a material of an unreliable and unsuitable character for the shells of marine boilers, more especially when the usage which such plates receive in heating and bending is considered, for, except among steel makers, it does not appear to have been generally known, that the thicker a plate is, the more brittle and erratic in its behavior it must become, as compared with a thin plate made to stand the same mechanical tests as far as tenacity and ductility are concerned, as, otherwise, I feel convinced that the increase in tenacity from 29 to 32 tons for thick boiler shells would not have been advocated.

So far as I am concerned, and the society which I represent, I may say that it has always been our endeavor to discourage the use of steel of high strength. The rules of Lloyd's Register require boiler-plates to have a tensile strength of from 26 to 30 tons, and have done this from the commencement of the use of steel, because we felt that the higher the tenacity arrived at, the more likelihood there would be of the plates giving trouble, and our whole desire has been to keep the material mild. We have, however, had considerable pressure brought upon us by manufacturers and engineers to allow a strength of 32 tons per square inch for thick boiler shell plates. This accident and the investigations which have followed, clearly point out that engineers have been drifting toward the use of an unreliable material, or at all events a material which is too near the verge of danger to be pleasant, a state of things that should not exist with steam boilers.

I would therefore urge, in order to remedy this growing evil, that the tenacity of steel plates for boiler shells—which are becoming thicker every day, should in no case exceed 30 tons; and that a temper test should be insisted on from every thick plate, and the practice of using enormously large plates should be discouraged, while more care should be exercised in uniformly heating and bending these plates. I have conferred with the principal steel makers in the kingdom on this subject, and am able to say that they agree with me, and are decidedly of opinion that steel plates over an inch in thickness, and having a tenacity of more than 30 tons, must contain so much carbon as to render them unsuitable for boiler-making purposes, although they may possess the necessary tenacity and ductility to withstand the usual tensile and cold-bending tests. I venture to hope that this paper will be made the subject of discussion, with a view to obtaining further opinions respecting the important points in question.

Nature says it is contemplated to use the electric light in Algiers for night work during harvest time, in order to escape the heat, which is just too much for Europeans, and is an obstacle to their carrying on agricultural work.

ON THE ANTISEPTIC TREATMENT OF TIMBER.

By SAMUEL BAGSTER BOULTON, Assoc. Inst. C. E.

From Papers of the Institution of Civil Engineers.

III.

Mr. Boulton had been obliged to be very brief in his verbal reply at the close of the discussion, and as some of the points then raised involved matters of considerable detail, which had also been alluded to in the correspondence, he thought that unnecessary repetition would be avoided if he were to connect his replies to both series of communications in a continuous form. He was gratified at the valuable support which his main propositions had received.

The remarks made by Dr. Tidy, and the views expressed by that gentleman in his recent report to the Gaslight and Coke Co. were in principle in accordance with the views expressed in the paper. The author, however, believed with Dr. Armstrong that Dr. Tidy, who had been somewhat conservative on the subject of tar acids, would be led by the logic of facts to accept a much lower proportion than 8 per cent. The "London creosotes" as they came from the still, honest creosotes which had done excellent work, and which constituted probably about one half of the total supply of this kingdom, did not contain so large a percentage. Some misapprehension still existed on this subject, which the statement of a few facts might remove. In July, 1863, the author sent to Dr. Letheby a sample of the usual London creosotes, which he was then largely using. Dr. Letheby found it contained only 4.37 per cent. of tar acids. Later on, and during one period of seven years especially, nearly the whole of the tar of the great London Gas Companies, as well as tar from other sources, was contracted for and distilled by the author's firm. The quantity was probably larger than had ever been treated up to that time by any one firm or corporation, and it therefore formed a sufficiently broad basis for estimation. He would give the quantities during three consecutive years—

1877	Gallons of tar distilled....	14,785,404
1878	" " "	15,839,819
1879	" " "	12,690,029

or an average of between fourteen and fourteen and a half million of gallons per annum. He had found, as stated in the paper, that the heavy oils distilled from this mass of tar contained on an average from 4 to 7 per cent. of total tar acids. More recent experiments which he had made upon a large number of London tars—one series in May, 1882, another in August, 1882, and a third since this paper had been read—gave similar results. Latterly, the largest of the English gas companies, the Gaslight and Coke Co., had erected works at Beckton, at which they distilled their own tar. It had been assumed that the list of analyses appended to Dr. Tidy's printed report represented the percentage of tar acids which the London creosotes in their natural condition contained. This, however, was not the case. The samples analyzed by Dr. Tidy contained from 8.2 to 10.2 per cent. of tar acids, but they had been specially treated to "meet the market," created by the modern type of specification by removing from the creosote some of its least volatile parts, those parts containing little or none of the volatile tar acids. The Gaslight Co.'s creosotes as they came from the still contained on an average 6 per cent. of total tar acids by the ordinary caustic alkali test. The author had been enabled to clear up this matter, of which experts would readily detect the importance, owing to the courtesy of the Board and Secretary of the Gaslight and Coke Co.

He agreed with Dr. Armstrong in the importance of M. Pasteur's experiment upon sawdust, which was recorded in the Comptes Rendus of the Académie des Sciences for 1863. It is remarkable as an early demonstration of the application of the germ theory to the phenomena accompanying the decay of woody fiber. Dr. Armstrong had alluded to the distinction between wood creosote and tar creosote. Both contained tar acids, some of which might be identical, or if not identical, isomeric. But tar creosote, if

it could be so called, was a complex body; some of the tar acids it contained differed essentially from either carbolic or cresylic acid, being less volatile, and less soluble in water than either phenol or cresol. There is evidently room for much further investigation in this connection; also for a more complete comparison between the "tar acids of the coal-tar oils and similar bodies contained in other oils."

In relation to the remarks of Mr. W. Foster, the author must express the hope that that gentlemen would continue the very interesting researches of which he had so recently given an account to the Institution in his valuable paper on "The Composition of Coal." Authentic Tables as to the varying products derived from different kinds of coal, and at different temperatures, were becoming matters of the first necessity in various branches of industry. Mr. Foster had referred to the experiment of Pettigrew, alluded to in the paper. Pettigrew had removed the embalming material from the heart of the mummy by steeping it in alcohol; after which, upon exposure to the atmosphere, putrefaction took place. What the author desired to point out was that the previous immunity from decay had not been the result of any chemical combination between the antiseptic and the tissue.

A jarring note had been struck by Mr. Bamber, who had represented "the whole secret of the paper" to consist in "the author's idea..... that nothing should be left in the creosote which it would pay him better to take out;" an object foreign to the declared aim and intention of the paper. The author had not approached the subject from the commercial point of view—a fact which the President had so gracefully recognized. It might, however, be opportune to state that he was not at present commercially interested in any manufacture which caused carbolic acid to be "taken out" of the creosote oils, although he was largely interested in the success of prepared timber as an engineering material, and therefore in the choice of the best antiseptics for that purpose, whether obtained from the creosote oils or from other sources. Mr. Bamber's figures as to the comparative commercial values of creosote oils and carbolic acid, recalled to memory the well-known comparison between the value per ton of iron ore and of steel watch springs. The manufacture of pure carbolic acid was a long and costly process, of which the first cost of the crude material formed an altogether insignificant item. Nor was so low a price as 2*d.* per gallon for creosote either "proverbial" or usual. But it would be found in the long run that the consumer had to pay the commercial value for everything which the creosote contained, and it was therefore best to discuss upon scientific and practical grounds the substances which the engineer should require it to contain. It was one of the main objects of the paper openly to point out by diagrams and detailed descriptions the principal substances contained in the coal-tar oils, to draw attention to their properties, and to state their uses for various manufactures, so that for the purposes of timber-preserving, engineers might be in a position to "prove all things, hold fast that which is good." Mr. Bamber was mistaken as to facts in his allusion to Dr. Letheby's specification, and that of Dr. Tidy. Dr. Letheby's specification, drawn up under instructions from Mr. Meadows Rendel, M. Inst. C.E., in 1865, for the use of the East Indian Railway Company, stipulated that the creosote was to yield to a solution of caustic potash, not less than 5 per cent. of crude carbolic, and other tar acids. Dr. Letheby never increased that quantity. Dr. Tidy had increased, and not as Mr. Bamber supposed, diminished the percentage of tar acids mentioned by Dr. Letheby.

Mr. Bamber complained that no facts or data had been given respecting Dr. Tidy's experiments on naphthaline. But the paper contained a reference to a printed report of Dr. Tidy, deposited in the library of the Institution, wherein was a full account of these experiments. They were also recorded and approved of by Dr. Lunge, of Zurich, in his learned work upon "The Distillation of Coal Tar." Amongst other authorities who after investigation differed from Mr. Bamber in admitting naphthaline as an ingredient in the timber-preserving oils, were the late Mr. Bethell, Mr. Burt, Prof. Sir Frederick Abel, Mr. Forestier, for the French Government, Mr. Coisne, for the Belgian Government, &c. Mr. Bamber had once stated to an eminent engineer, in a report upon a creosote highly charged with naphthaline, that timber impregnated with such

an oil would, "within a very short time of the timber being in India, lose 5 lbs. out of every 10 lbs. put into the timber here merely by escape of naphthaline." Dr. Tidy's experiments with timber injected wholly with naphthaline, and subjected to a temperature of 130° Fahrenheit, proved that these apprehensions were unfounded. But it was now related by Mr. Bamber that in his own experiment a piece of wood impregnated with a creosote of the type which he preferred, and containing 20 per cent. of tar acids, lost in four weeks 42.33 per cent. of the oil taken up. Mr. Bamber's record of his own experiment was very instructive. He tried two kinds of creosote against each other. One, which might be called specimen A, was "full of naphthaline," but the percentage of that body was not stated. It contained 10 per cent. of tar acids. Specific gravity not named. With this oil a piece of deal 3 inches by 3 inches by 8 inches was impregnated. The other, which might be called specimen B, was a "country oil," specific gravity 1.045, containing 20 per cent. of tar acids. With this oil a piece of deal 3 inches by 3 inches by 6 inches was impregnated. Specimen A was alluded to as "Mr. Boulton's own oil" and "the author's London creosote;" but to these appellations he demurred, as he never used a 10 per cent. creosote unless required to do so by specification, and the London oils did not in their natural state contain 10 per cent. of tar acids. Therefore A, although it might come from his works, would be a mixture of London and Country oils. But, although in the author's judgment too volatile, yet the 10 per cent. specimen would be less volatile than the 20 per cent. Therefore, the author preferred A to B. Where a large issue was staked upon a single minute experiment, accuracy of result should be ensured by the most minute precautions. It was not explained why the two pieces of deal were not cut to the same size, a circumstance which affected the conditions both of absorption and of evaporation. Nor were the specific gravities of the two pieces of wood stated. Of two pieces cut from the same log, one piece of wood would frequently absorb, under the same conditions, a very much larger quantity of fluid than the other. However, the results as stated might be calculated as follows:—

A. Piece of wood, capacity 72 cubic inches, absorbed 1,020 grains of creosote =3.49 lbs. per cubic foot.

B. Piece of wood, capacity 54 cubic inches, absorbed 1,785 grains of creosote =8.17 lbs. per cubic foot.

But no pressure was used, and engineers would recognize that the experiment failed to reproduce the conditions of the ordinary creosoting cylinder. It was well known that without pressure, light oils penetrated timber more easily than heavy oils. In like manner the adulterating substance, bone oil, penetrated more readily than creosote; solutions of metallic salts more readily still; and water more readily than all. But it was "light come, light go;" those which penetrated most readily were generally the least permanent. The main object of the engineer was not to select the fluid which gave the contractor the least trouble to inject. He desired to select the antiseptic which was likely to be the most efficacious and the most permanent, and he required the contractor to provide efficient apparatus, and to inject under pressure a stipulated quantity by weight. Sleepers and large logs of timber were injected without difficulty with creosotes of a heavier type than either of Mr. Bamber's samples, and to the extent of 10 lbs. and 12 lbs. per cubic foot. Small pieces of wood could be easily gorged with creosotes. The author had recently injected some fir paving blocks 6 inches by 6 inches by 3 inches, with 22 lbs. per cubic foot of ordinary heavy London creosote, containing about 5 per cent. of tar acids. Mr. Bamber exposed his specimens to evaporation on a mantelshelf at a temperature never above 70° Fahrenheit, and generally between 40° Fahrenheit and 50° Fahrenheit. In four months A had lost 47.75 per cent., and B had lost 42.33 per cent. of the creosote put in. If this could be taken as a normal result, engineers would hesitate as to employing either type of creosote. No doubt both were too volatile. But it should also be borne in mind that the injection was imperfect; to use Mr. Bamber's "expression, it was only "skin deep." As regarded the comparative evaporation of the two specimens, however, the result was extremely valuable. It is well-known that the evaporation of fluids (except when in a state of ebullition) was in pro-

portion to the surface exposed, and not to the bulk of the fluid. This point Mr. Bamber appeared to have forgotten; he had exposed A, the creosote he disliked, to a wider evaporating surface than that to which he had exposed B, the creosote which he preferred. The position on the mantelshelf in which the pieces of wood were placed was not stated. But supposing them to have been suspended, say by a thread, so that all the surfaces were exposed to evaporation equally, the results might thus be calculated:

A. Piece of wood, the sum of whose superfices was 114 square inches, lost 487 grains=4.29 grains per square inch of exposed surface.

B. Piece of wood, the sum of whose superfices was 90 square inches, lost 575 grains=6.39 grains per square inch of exposed surface.

If, however, each piece of wood had been placed with one of its sides in contact with the mantelshelf, so that one surface was protected from evaporation, the calculation became slightly modified, so that A would have lost 5.41 grains, and B 7.98 grains per square inch exposed. If the specimens had been placed on end, then A showed a loss of 4.64 grains and B of 7.09 grains per square inch. Mr. Bamber had therefore been mistaken as to the comparative volatilities of naphthaline and the tar acids, as proved by his own experiment. B, the creosote with 20 per cent. of tar acids, had lost about 50 per cent. more than A, the creosote with 10 per cent. of tar acids and "full of naphthaline." Had it been otherwise, every chemical treatise describing the properties of these bodies, would have to be re-written. The statement that part of the loss of specimen B was due to the fact that some of the oil drained out of it, which it was said "was not fair" to that specimen, gave rise to the rejoinder, was it quite fair to a timber-preserving process that a type of antiseptic should be recommended which "drained out" with so little provocation? This part of the discussion might almost appear trivial, were it not for the fact, confirmed by many special instances in the author's experience, that whenever these light oils had been used exclusively, whether for marine work or for railways, complaints invariably arrived, sooner or later. Oils of so light and volatile a nature lost a large portion of their bulk, which evaporated or drained out in the creosoting yard, on the export ship, and on the permanent way in India and elsewhere. An experiment, easy to carry out without any laboratory apparatus, may be tried by any one interested in this subject. Take three saucers or shallow dishes; place in one saucer 200 grains of pure carbolic acid (crystallized), in the second 200 grains of pure cresylic acid, and in the third 200 grains of pure naphthaline. Expose them side by side in any room, and at any ordinary temperature. The crystals of carbolic acid would liquefy in a few minutes, owing to the avidity with which that body absorbed moisture from the atmosphere. In a few weeks' time (varying with the temperature) the carbolic acid would have entirely disappeared by evaporation. By that time the cresylic acid would have lost about half its bulk. When the whole of the cresylic acid had also evaporated. The naphthaline in considerable bulk, at least one-half of the original weight would still remain, an easy victor in the trial of endurance.* The evaporation was greatly retarded by the incorporation of those bodies with the less volatile oils, and by their being driven into the cells of the timber. But the evaporation must necessarily take place in proportion to the respective and recognized volatilities.

Allusion had been made by Mr. Bamber to "charred oil," and he presumed that it was a residue of anthracene manufacture. The author in the course of his experience had never met with "charred creosote," except indeed as a result of over-heating in a laboratory experiment; nor was he acquainted with any ordinary process of manufacture by which it could be produced. Creosote oils were distillates; whatever the heat in the still, the residuum might become carbonized, but not the substances which came over in the form of vapor. Anthracene or paranaphthaline had been denounced by the creosote specifications of the theorists at a time when it was considered worthless for any purpose; it was taken out of the creosote by every tar-distiller in England, whether in London or country, and was now of value for the manufacture of ali-

* This experiment was carried out on a mantelshelf at the Institution of Civil Engineers in August, 1884, with the result indicated by Mr. Boulton.

zarine. The removal was effected by a simple process of filtration; the resulting oils were the green oils, the best part of creosote for timber-preserving, fluent and rich in alkaloids. How could they become "charred oils?"

In the illustration, drawn from a fire-engine, it was forgotten that a fire might break out a second time, and that if a fresh supply of water were not available, the building would be consumed. Carbolic acid evaporated rapidly from timber, and it had been proved that it left no permanent effects behind. When the sleeper was placed in the permanent way the supply of the antiseptic could not be renewed, and the timber would rot if more stable antiseptics were not present in the shape of the heavier oils.

As regarded naphthaline colors Mr. Bamber was also mistaken. They were very successful as a manufacture, and their use was largely increasing. He accused the author of "condemning country oils," and of saying that they "were not good for creosoting timber." In the paper the exact contrary was stated. The author advocated the use of both London and country oils, and he habitually used large quantities of both. What he condemned was the use of oils, whether London or country, which were so manipulated as to contain a large proportion of volatile substances at the expense of the more durable, and therefore for this purpose more valuable antiseptics. Were Mr. Bamber's theories carried into practice, about one-half of the creosote manufactured in England, the enormous bulk of the "London oils," would be excluded from use by the timber-preserver. Nevertheless they were precisely the creosotes which had given the most unmistakably good results, whether, as in the case of the early Indian sleepers, and of the sleepers of the Chemin de Fer de l'Ouest, the percentage of tar-acids had been proved to be small, or whether, as was the practice of the Belgian Government, the tar-acids had been altogether and avowedly struck out of the specification.

In reply to Prof. Voelcker, he desired to state that he had purposely abstained from connecting the names of administrative bodies with the questions of controversy. He was not aware of any specification officially issued by the War Office which bore on this subject; but it was known that the distinguished chemist of that department had been consulted by various administrations, who could have had no other object in view than to obtain the best engineering material. The views of Sir Frederick Abel on all the most important points of a creosote specification were substantially the same as those of Dr. Tidy and of the author. And what the author considered to be the most important points were, 1st, that the presence in considerable volume of the heavier and least volatile distillates, *i. e.*, those distilling at or above 600° Fahrenheit, must not merely be tolerated but insisted upon. That naphthaline, and the other usual semi-solid constituents, should be admitted, provided they were completely fluid at the temperature to which the creosote was raised when injected into the wood. It was known that these views had not been adopted by the Crown agents for the colonies, but he hoped that this discussion might be the means of clearing away many misconceptions. Respecting the point which he considered subsidiary to the other two, although not unimportant, viz., the percentage of tar acids, Sir Frederick Abel, as well as Dr. Tidy, had recently recommended a reduction, and the last word had not been said on this question. Prof. Voelcker was mistaken in thinking that Dr. Tidy had recommended 8 per cent. of carbolic acid. The 8 per cent. was of total tar acids, including carbolic, cresylic, and all other tar acids which could be removed by a specified solution of caustic soda. Dr. Tidy, in his report to the Gaslight Co., mentioned his reasons for not stipulating for a fixed quantity of carbolic acid. Whenever any stated quantity of this body had been mentioned in specifications by English engineers, it had been fixed at one-half of the total tar acids. Hence the quantity had varied from 2½ per cent. to 5 per cent., the latter being the largest quantity of crude carbolic acid which the author had ever known to be required by any specification issued in this country. He might be permitted to express his satisfaction that Dr. Voelcker had recently joined the ranks of investigators into the properties of creosote oils, but he was sure that so distinguished a chemist would be the last to depreciate the experiments and experience of the

numerous chemists and practical men who had placed the results of their labors on record. It could surely have only been by some misconception that Dr. Voelcker recommended an entirely new departure by asking for 10 per cent of carbolic acid in creosotes used for young timber or sap-wood, although he admitted the probable superiority of the heavier oils for timber intended for railway sleepers and other engineering purposes. Dr. Voelcker had not produced the results of any original experiments in support of his views. The typical experiments which he asked for had been tried and recorded; they proved that carbolic acid and the lighter tar acids were not reliable as durable antiseptics for timber. Engineers were familiar with the preparation of young wood and sap-wood as well as with that of older timber. The same creosotes were always used for both, and with complete success. It had been clearly established that the heavy oils preserved sap-wood from decay. It would be remembered by many members of the Institution that the late Mr. Bethell had even advocated the use of young wood in preference to older timber, because the sap-wood absorbed the creosote so readily, and that Mr. (now Sir John) Hawkshaw had combated this idea, not from any doubt of the preservation of young wood, but upon the ground that the engineer must choose for many purposes the kind of timber best adapted for resisting impact or heavy strains. Amongst the numerous successful specimens of creosoted wood which had been exhibited at the Institution during the discussion, and which had been taken from various railways after periods of endurance varying from sixteen to thirty-two years, nothing was more striking than the perfect preservation of the sap-wood, although careful analysis had shown that the heavy oils, and not the tar acids, were the enduring agents of preservation. The allusion of Dr. Voelcker to telegraph poles had elicited much practical information. Nothing could be more conclusive than the evidence of Mr. Preece as to the behavior of the young timber, surrounded by its girdle of sap-wood, which was used for telegraph poles in this country. The author had been responsible for the creosoting of a large portion of the poles alluded to by Mr. Preece; these had as a rule been prepared with the usual London oils. But it was only right that he should state another circumstance. He believed that the success of the poles, creosoted for the Post-office Telegraph Department, was largely influenced by the care taken by that department in the seasoning of the timber. The date of delivery of the poles, landed and stacked at the creosoting yard, was a matter of contract, but there was no fixed date for the creosoting. On the contrary, the engineer did not allow them to be creosoted until he pronounced them to be dry, and ready for the process. Sixteen years ago, at a meeting of the Institution, he had urgently recommended the adoption of some such method for ensuring the proper seasoning of timber. The very interesting and satisfactory evidence of Mr. Bouissou, the Engineer of the West of France Railways, confirmed the experience of Mr. Preece, both as to the satisfactory results of creosoting, and also as to the great importance of seasoning before creosoting; the precautions adopted for the latter purpose by the French company being substantially the same as those of the English administration. With reference to the preparation of telegraph poles, a very valuable paper had been contributed by Mr. William Langdon, M. Inst. C. E., to the Society of Telegraph Engineers, on the 25th of March, 1874. Mr. Langdon had also contributed to this discussion, and had confirmed by his experience many of the views entertained by the author. With regard, therefore, to the observations of Dr. Voelcker as to green or unseasoned timber, the author would add the results of his own long and varied experience in this and other countries, by saying that the attempt should never be made to inject creosote, or any other oily substance, without previously, or at the time of the operation, expelling watery moisture. Timber should not be felled whilst the sap was in it.

As regarded the effects of living organisms, and the introduction of their spores through cracks in the wood, the views of Mr. Carruthers entirely agreed with those expressed by the anthor. But what was the remedy? The botanical aspect of the question had not been lost sight of, from the days when Dean Buckland and others discussed at this Institution the question of timber preparation from that important

standpoint, and it had not been overlooked in the modern systems of injection. Exogenous trees, whose annual growth took place by the formation of concentric layers of vascular tissue added externally, furnished the timber with which engineers had almost exclusively to deal. The softer and younger wood, containing the greatest portion of albumen, was on the outside; it was more liable to decay than the harder portions. It was the chief merit of the system of injecting under pressure that it precisely met this difficulty. The softer parts absorbed more of the antiseptic than the rest, the pressure followed the line of least resistance, the antiseptic fluid gorged the sap-wood, and penetrated to all cracks or shakes. There was but little analogy between this method and the application of a surface coating of pitch, as although he recommended by preference oils of a heavy character, and containing semi-solids, the whole of these bodies were perfectly liquid at 100° Fahrenheit, the temperature to which they were usually subjected at the time of injection. On cooling, they solidified, not on the surface merely, but within the pores of the timber, which they sealed up against the incursion of the agents of decay. Mr. Carruthers had referred to the experiments of the celebrated Dr. Koch. The researches of Koch, and of other German scientific investigators, were very damaging to the claims of carbolic acid as a germicide, and as a coagulator of albumen. In his treatise "Ueber Desinfection," Dr. Koch deduced from his careful and laborious experiments minutely described, that the value of carbolic acid was greatly limited as a germicide, and that for the destruction of spores it was altogether useless, being almost without action; but that it could be used to destroy micro-organisms free from spores. This was when used in a watery solution; still stronger was his opinion as to an oily solution. He stated that in solutions of oil or alcohol, carbolic acid did not exhibit the slightest antiseptic action. To this, the remarks of Dr. Sansom had already pointed. It must be remembered that it was in an oily solution, *i.e.*, dissolved in the tar oils, that carbolic acid was applied to timber. G. Wolffhügel and G. v. Knorre followed up Koch's investigations, and spoke of the inactivity of an oily solution of carbolic acid; of its inferior powers of penetration into porous solids, and of its inferiority in the destruction of fungi. F. Boillat, who followed up the experiments of Koch in the laboratory of Professor Nencki at Bern, found that albumen, when completely coagulated with an excess of carbolic acid, formed no permanent combination therewith. He was able to wash out on a filter the whole of the carbolic acid from the albumen precipitate, after which, upon exposing it to the atmosphere during forty-eight hours, the albumen became putrid. Mr. Carruthers had spoken of the presence of free crystallized carbolic acid in the cells of a small piece of a wooden hurdle. But carbolic acid would not crystallize out of the oils holding it in solution; it could only be obtained in that state of purity by a long and complicated chemical process, and the crystals would immediately liquefy when exposed to the atmosphere. The minute particles seen by Mr. Carruthers were probably naphthaline, or one of the other semi-solids of the higher distillates of coal-tar. The condition of this hurdle corresponded exactly with that of enormous masses of successfully creosoted timber as typified by the samples exposed during this discussion, and the author thought that the final question of Mr. Carruthers had been fully answered by many authorities quoted in the paper.

In reply to Mr. C. de Laune, the author would remark that his paper had a much wider object in view than the mere question of carbolic acid; the presence or absence of that body would not explain Mr. de Laune's difficulty. No honest creosote made from coal-tar, whether "London" or "country" oil, whether with much or little tar acid, contained any ingredient which could injure timber; the only question was, which of those ingredients was most efficacious and most durable. The question as to which was the easiest to put into the timber was of much less importance. Some small pieces of hurdles, &c., had been shown during the discussion, and alluded to by Dr. Voelcker, Mr. Carruthers, and Mr. de Laune; Mr. E. A. Cowper had detected the reason why one had succeeded and the other failed. The first had had plenty of creosote put into it; the others but very little. Mr. de Laune had made a

detailed statement to the author, which was briefly as followed: That he had been in the habit of preparing different kinds of timber of various densities, and frequently in a wet or unseasoned state by boiling the wood in creosote in open tanks and without a thermometer; and that he did not keep the timber in the tanks more than twelve hours, as a longer operation rendered it brittle—a very significant fact. He said that he had not latterly superintended these operations personally, and that he did not regard the process as a scientific one, but thought that it could be carried out by odd hands, old men, or boys. A good many years ago, the author had had considerable experience in preparing timber in open tanks with corrosive sublimate, sulphate of copper, and also with creosote. The time for leaving the timber in the tank, to be injected by the metallic salts in watery solution, which penetrated more readily than creosote, was generally calculated at about twenty-four hours for every inch in thickness of the wood. With the creosoting process it was essential that the water in the timber should be first got rid of; the presence of the water prevented the entrance of the creosote oils. Even with the cylinder-process, where the oil was driven in under pressure, engineers insisted upon the timber being dry, and they weighed it before and after the operation, to check the quantity of creosote injected. With the open-tank system more care, and not less care, was necessary than with the superior apparatus. But soft young timber, if properly seasoned and then subjected to creosote at a moderate heat, could without difficulty be made to imbibe a sufficient quantity of creosote of any kind manufactured in this country. But if the timber was wet, it was not amenable to treatment by creosote in open tanks at a moderate temperature, and if the creosote was raised to a temperature even approaching to its boiling-point, which was about 400° Fahrenheit, it would cause the timber immersed in it to become as brittle as a carrot. Timber should not, under any circumstances, be subjected to a higher temperature than 250° Fahrenheit. It would, therefore, appear that Mr. de Laune's difficulties were to be explained by his methods of operation. He had told the author that he had for many years procured all his creosote from the same works, a small local manufactory, where the tars of the district were distilled. It had been ascertained that the creosotes manufactured at the works in question had not essentially varied in type, whilst even as regarded carbolic acid, if the analysis quoted by Mr. de Laune was correct, the quantity contained in the sample was considerably above the average, although this was a point to which the author attributed but little importance. He was surprised to find, in the report accompanying the analysis alluded to, a statement to the effect that "good creosote should yield quite 75 per cent. of volatile oils (*sic*) containing 10 to 15 per cent. of crude carbolic acid." No creosotes used for timber-preserving, under any specification, had ever been required to contain more than from 2½ to 5 per cent. of crude carbolic acid. The recommendation of "volatile oils" was a mistake which was obvious to all experts; but it might have a bad effect in encouraging the use of some of the worst adulterants, substances sold as creosote which were not derived from coal tar at all. The report, although issued from the laboratory of the Royal Agricultural Society, was signed for, but not by, Dr. Voelcker. The author had understood that Dr. Voelcker was at the time absent owing to illness; he would not therefore have alluded to it but for the fact that this report had been brought so prominently into notice by Mr. de Laune, and that extracts from it had been published in an agricultural journal.

The author had used creosoting for farm purposes, for fences, hurdles, and for many years also, for piles and fences for his wharves. He always used for himself the type of creosote he recommended to others, and it had proved invariably successful in his own case.

The author was asked by Mr. Cleminson why he had not alluded to the process of Mr. Blythe. If by Blythe's process was meant the attempt to introduce the creosote oils, or any part of them into timber in the form of vapor, the subject had been fully treated in the paper. For the operations described as having been carried out for the Compagnie des Chemins de Fer de l'Ouest, the apparatus used was supplied by Mr. Blythe. The experiments of Mr. Seidl were described

by him as having been carried out by "Blythe's process." Engineers in England had recently had an opportunity of witnessing similar experiments at the works of Messrs. Connor, at Millwall. After the dismantling of these works, the author had purchased the greater part of the machinery for the purpose of adapting it to his own processes, so that he had again had an opportunity of studying the question. By slow evaporation, fluids gradually volatilized at temperatures much below their boiling-points. But pressure from their vapors could only be obtained at temperatures exceeding their boiling-points. Thus water gradually evaporated even from a frozen surface, but no tension of its vapor could be produced except at a temperature exceeding its boiling-point, 212° Fahrenheit. The boiling-point of the creosote oils ranged from about 400° to 760° Fahrenheit, that of carbolic acid when separated from these oils being 360° Fahrenheit, and of cresylic acid 390° Fahrenheit. Now, it was well known that timber for the purposes of the engineer was injured and rendered brittle and unsafe at a temperature much exceeding 250° Fahrenheit. How then could those tar products be introduced under pressure into the timber as vapor, whether accompanied or not by superheated steam, without injuring the timber? Either the temperature must be raised above danger point for the wood, or nothing but the vapor of water would be driven into it. This applied to the first part of the process. Of course, if it was followed up by an injection of the creosote oils in the usual manner, this second part of the process covered the deficiencies of the first operation. The presence of any of the components of the tar-oils could be detected in the timber by chemical tests. When specimens of wood had been produced, which had been prepared by the injection of tar-oil vapors in sufficient quantity to have a practical value in the preservation of timber, and at a temperature not exceeding 250° Fahrenheit, the author would be very glad again to give his best attention to this part of the subject.

He was glad to be able to reply to the question of Mr. Lawford, with regard to the Midland Railway Company. In 1866, at a meeting of the Institution, Mr. Crossley, the engineer of that company announced that, although he admitted that creosoting stopped decay, he had given up that process from a calculation of economy based on the assumption, that with very heavy traffic like that which prevailed over the lines of his company, the sleepers were worn out by hard work before they had time to decay. The author would suggest that incipient decay of unprepared sleepers often set in at a very early period of their service, especially through cracks and bolt-holes; the fastenings of the chairs thereupon became loosened, and the mechanical destruction of the sleepers hastened. But Mr. Lawford would be glad to hear that the Midland Railway Company had again adopted creosoting; they had had large quantities of sleeper creosoted during the last few years.

In reply to Mr. Roberts, the author had never found any difficulty in completely saturating the sap-wood with the London oils where the timber had been sufficiently dry. Mr. Coisne's experience with shavings were for the purpose of ascertaining what kind of creosote lasted best, and he effected a complete saturation both with the thin oils and with thick oils. The thinnest oils did not preserve the woody fiber from rotting, even with so good an injection, whilst the heavier oils did. *A fortiori*, the thinner oils would be, by themselves, still more unreliable with the inferior injection carried out in practical operations with timber. It must also be borne in mind that Mr. Coisne did not stop at these experiments, but had confirmed them by twenty years' subsequent treatment of timber on a very large scale, for the Belgian State Railways. The chapter in Mr. Coisne's 1871 pamphlet, upon the choice of creosote oils was a most interesting and practical one.

With reference to the author's process for removing water from the timber at the time of creosoting, the following experiment had been carried out at his works since the paper had been read.

Six square fir-sleeper blocks, each 8 feet 11 inches by 10 inches by 10 inches, saturated with moisture, were cut into 10 inches by 5 inches sleepers. One sleeper, A, from each block was prepared by the new method, the corresponding sleeper, B, from the same block, by the old method, so that in each instance the results

with the two halves of the same log could be contrasted. Care was taken to choose blocks having the heart in the center, and with the texture of the two halves as nearly as possible similar.

From the six sleepers, A, water was withdrawn by the new process to the extent, ascertained by weighing the water, of 120 lbs.; yet the sleepers, when withdrawn from the cylinder after the process was completed, weighed 155 lbs. more than when put in, thus showing that they had absorbed 275 lbs. of creosote. As their total cubic contents were 18.57 cubic feet, their average loss of water was 6.45 lbs. per cubic foot; their average gain of creosote was 14.8 lbs. per cubic foot.

The six sleepers, B, were creosoted by the ordinary process. Being, like the others, very wet, and having no moisture extracted from them, the results of their being weighed before and after creosoting showed an absorption of 116 lbs. of creosote only, or an average of 6.29 lbs. per cubic foot. The separate absorptions of these six sleepers were as followed: 9.04 lbs., 4.52 lbs., 2.9 lbs., 6.13 lbs., 9.36 lbs., and 5.49 lbs. per cubic foot respectively, thus illustrating the uncertain results of creosoting timber when too wet by the ordinary method. They were placed in the cylinder with a charge of ordinary dry sleepers, which took up on the average rather more than 10 lbs. of creosote per cubic foot.

The result with sleepers A was interesting, as it showed that by the new process wet timber could have its moisture at once removed, and a large quantity of creosote injected without difficulty. All twelve sleepers, both A and B, were afterwards cross-cut at 6 inches, 9 inches, 12 inches, and at 4 feet 6 inches from their ends, the corresponding section of A and B being constrasted and photographed. The sleepers A were found not only to have absorbed a large quantity of creosote, but the creosote was much more evenly distributed than was the case with sleepers B.

Might the author be permitted to sum up the evidence which had been produced during the discussion as to the best class of antiseptics for timber? Both engineers and chemists would probably agree with him that after forty-five years' discussion of this engineering problem the time had gone by for dogmatic assertion, unsupported either by experiment in the laboratory or by recorded experience in engineering works. In the paper he had called the germ theory a severe but salutary test for these antiseptics. As a matter of fact, the subject had received valuable elucidation from the labors and discoveries of a number of eminent men, who had studied the physiology of the bacteria. In the application of the remedies, however, the operations of the timber-preserver diverged from those of the physician to the human body. In combating those terrible enemies the bacteria, which were pathogenic to animal life, the great difficulty was that many of the remedies effectual against the bacteria intefered with the vital functions of the patient. On the other hand, the physician could repeat remedies whenever the malignant symptons reappeared. Therefore antiseptics, more or less volatile, were sometimes more useful to the physician than others of a more permanent character, because they did not accumulate in the system of the patient. In preserving timber, the problem differed materially. The vital functions of the plant had ceased; stronger poisons, and substances which clogged up the cells and tissues, could be employed, provided always that they were of such a nature as not to injure the structure of the wood. But the remedy must be applied once for all. In the majority of cases where timber was once placed in engineering works the supply of the antiseptic could not be renewed. Therefore the very first condition was that the antiseptic should be of a permanent constitution. Let this rule be applied to the evidence offered during the discussion. Antiseptics for timber had been described: 1st, as coagulators of albumen; 2d, as germicides; 3d, as sterilizers, rendering the cells of the wood unfit for the development of fungi or bacteria; 4th, as germ-excluders, closing the entrances against the intrusions of the enemy.

Was not too much value still attached by some to the coagulation of the albumen in wood? Albumen formed an extremely small portion of the wood; in fir it varied from 0.5 to 0.9 per cent. Those parts of timber containing the smallest portions of albumen were nevertheless liable to decay; the mere coagulation of

the albumen did not protect the bulk of the timber from destruction. Did coagulation preserve even the albumen itself from destruction? Sansom, Angus Smith, and other authorities found that it did not. The author took a hard-boiled egg, a very complete specimen of coagulated albumen, removed the shell, and exposed it to the sea breezes on a high point of the Atlantic shore of the island of Mull. In a few days signs of putrefaction were visible; in eight days the albumen was coated with various species of micrococcus, cromogenes and other agents of destruction. The egg had become a mass of corruption. Coagulation had not protected albumen from putrefaction. What was the result when coagulation was produced, not by heat, but by the action of an antiseptic body? Did not the result depend mainly, if not altogether, upon the germicide properties of the antiseptic, and upon its abiding presence? Or, thus produced, did coagulation *per se* effect a new combination with permanent results? In the case of carbolic acid, a host of investigators said, No. Their experiments appeared to prove that carbolic acid was volatile in the air, soluble in the water, and that its compounds were not stable. Boillat, in his experiments, realized the extreme conditions desired by those theorists who thought that carbolic acid had a permanent effect upon timber; he produced a perfect coagulation of albumen with an excess of carbolic acid. Yet a mere washing with water removed the whole of the carbolic acid, and the albumen putrefied on exposure to the air. Carbolic acid, therefore, would appear to have had no permanent effect upon albumen. The author agreed with Dr. Bernays that if coagulation by carbolic acid were desirable, 2 per cent. of that body might be retained; but having in view the foregoing evidence, what was the value of the coagulation theory at all as applied to timber-preserving? There had been some idea that carbolic acid lingered in the timber in some unrecognized form. The author had had occasion to test sleepers a few weeks after creosoting: if this were done before the carbolic acid had time to evaporate, it could be found in the wood by the ordinary tests, and a quantitative analysis made. On the other hand, Dr. Tidy, who was not unwilling to find it in combination, had searched for it after twelve months, and had not found it by the ordinary tests, that is, in sufficient quantity to have any practical result. But whenever it was present there were tests subtle enough to detect it, even in such infinitesimal quantities as to have no practical value, as was evidenced by the experiments of Mr. Greville Williams. Notwithstanding theories and experiments, did carbolic acid, when put into timber, do any good there? Mr. Coisne, Mr. Greville Williams, and the author, not only never found it to have contributed to the success of old creosoted timber, but Mr. Coisne's experiments went further still. He injected woody fiber with light oils and an excess of tar acids, and the woods rotted, whilst the woods creosoted with heavy oils, and without any tar acids, were preserved.

There was one point respecting which there had been a consensus of opinion on the part of all who had taken part in the discussion, namely, that for the preparation of timber, creosoting had been undeniably more successful than corrosive sublimate, sulphate of copper, or chloride of zinc. Could this be at all due to carbolic acid? How was this possible, when a host of authorities proved that carbolic acid was less permanent in its effects than the three metallic salts alluded to, and very considerably less powerful as a germicide than corrosive sublimate or sulphate of copper. In a valuable work upon bacteria by Magnin and Sternberg there was a long list of antiseptics, with a statement as to their comparative potency as germicides, compiled from the latest authorities. Carbolic acid was low in the scale. Dr. Sternberg gave the strength of solutions of different kinds which had been found efficacious in preventing the development of the septic micrococcus, the following amongst many others:

Corrosive sublimate...	1 part in	40,000
Sulphate of copper....	1 "	400
Chloride of zinc.	1 "	200
Carbolic acid..........	1 "	200

These were in watery solution. To this must be added the statements that in oily solution the antiseptic power of carbolic acid was diminished, according to Sansom and Angus Smith; altogether it was nil according to Koch. It was evident that there was a vast accumulation of scientific evidence in confirmation of the

continually reiterated statements of those practical men, who had had the largest and longest experience in preparing timber, to the effect that it was not to carbolic acid, but to other substances contained in the tar oils, that the superiority of the creosoting process over the other three methods was due. Mr. Lowe was well known as one of the highest scientific and practical authorities upon the tar acids, and he had given much valuable information in his communication to the Institution. On the other side, the absence of evidence was even more remarkable than many of those interested in this subject would perhaps have anticipated. No chemist had brought forward even a laboratory experiment in proof of any permanent effect of carbolic acid upon albumen. No practical man had produced a proof that that substance had had any lasting effect upon timber. The author submitted that the claims of carbolic acid as an antiseptic for timber had not been proven.

What, then, were the substances in the creosote oils which had insured the superiority of that process over the others? If the author were asked the question, he would remark that the object being the prolonged preservation of timber, antiseptics should be chosen which remained longest in the timber. That the different constituents of the creosote oils, showed a gradation from the lightest and most volatile bodies at the carbolic, or left-hand end of the scale, up to the least volatile and most permanent bodies at the right-hand end. Divide the bulk of the oils roughly in half. Would the constituents of the right-hand half of themselves insure the preservation of the timber? Yes, excellently well. They contained germicides and solidifying materials; they were both sterilizers and germ-excluders; they would not evaporate, except at an enormously high temperature. Nevertheless in their united bulk they were perfectly fluid at a temperature of 100° Fahrenheit; they were insoluble in water; they could be injected into timber, in quantity exceeding the maximum which any engineer had as yet required. Would the other, or left-hand part of the group, taken by themselves, preserve timber? Much less perfectly, as they were more volatile. Would a still further fractioning to the left, if it were practicable, insure a better result? Not so, but a worse one still; for the lightest oils, which contained the greatest portion of the tar acids, were, like the tar acids themselves, the most volatile portions of all.

The author trusted that he had made clear his reasons for specially objecting to large percentages of tar acids. Take an honest heavy cresote, free from adulteration, free from mutilation, containing, say, 5 per cent. of tar acids. If this sample were refused because it did not contain 8 or 10 per cent., the tar-distiller was induced to remove a large portion of the heavier constituents of the bodies to the right hand of the scale, in order to make the proportion of tar acids larger in the portion remaining. He believed that those heavier portions were the best. He thought that, provided the oils were sufficiently fluid at the temperature at which they were injected, there should be no restriction as to maximum specific gravity or maximum boiling-point. If larger and stronger doses of germicides were desired, it would be far better to put them into the wood in the shape of corrosive sublimate or sulphate of copper, in addition to the heavy oils. This could be done by a double process of preparation, with respect to which he had been lately experimenting.

Timber preserved by antiseptic treatment was an engineering material competing with other materials, both as to price and durability. Members of the Institution would appreciate the endeavors of the author to emancipate an important industry from the effects of any theories which, themselves unproven, might stand in the way of improvement, either as to diminished cost or increased efficiency.

STRIKES and rumors of intended strikes are on the increase in Germany, and some of the revelations as to the "starvation" wages that prevail in certain branches of labor are somewhat startling. Thus, the linen weavers of Erdmannsdorf, a village in Silesia, have ceased work in an attempt to secure an advance of about 20 per cent. on their wages, which average six and a-half marks per week for twelve hours' work per day, and seldom or never exceed 9 marks, equivalent to as many shillings.

PORTLAND CEMENT.

By HENRY FAIJA, M.I.C.E.

From "Iron."

Although a great deal has from time to time been written about Portland cement, the author is bold enough to think, and also to say, that very little is known about it, except to those intimately connected with its manufacture or who happen to be large users, and have, therefore, been obliged to make themselves thoroughly acquainted with the subject. In general, little is known about it, so little, indeed, that the specifications submitted to manufacturers are often absolutely impossible to work to; and even supposing they could be, the result would probably be a material of which at present nothing is known; it might, in some instances, do the duty of cement, but even that is doubtful, and it certainly would not be a Portland cement.

The error into which most of the drafters of these specifications fall is that they have heard or read that a cement should be extremely finely ground—the finer the better—also that it should be heavy; therefore, by inference, the heaviest weight is specified in conjunction with the finest grinding, and these, unfortunately, are properties which cannot both be obtained in the same cement. It is not at all uncommon to see in a specification that the cement shall weigh 120 lbs. to the striked bushel, and be so finely ground as to leave only 10 per cent. of residue when sifted through a sieve having 4,900 holes (70 by 70) to the square inch. Now, it would be impossible to produce a cement which would comply with these requirements. It should be remembered that the finer a cement is ground the more bulky it becomes, and consequently weighs less per striked bushel. The weight given above, 120 lbs. per striked bushel, would probably correspond with a fineness, or rather a coarseness, of a residue of perhaps 30 to 35 per cent. on the 70 by 70 sieve, and 20 per cent. on the 50 by 50 sieve; while the fineness mentioned, 10 per cent. on a 70 by 70 sieve, would correspond to a weight of perhaps 106 to 110 lbs. per striked bushel. Of course, the bushel measure is to be filled by one or other of the recognized methods, so that it shall be filled as lightly as possible, and in no way touched or shaken until the cement in it has been struck level on the top. For the practical purposes of the user, however, the weight of the cement is now acknowledged to be of very little service in assisting the determination of its value. It is a test which originated with the manufacturers, and to the manufacturer working always with the same raw materials, it is of use, for it enables him to form an opinion as to the degree of calcination to which the cement has been subjected, the harder burnt producing the heavier clinker. But to the user the weight is no guide whatever, since peculiarities in the raw materials, without being detrimental in any way, may produce, on calcination, a heavier or a lighter cement. In the author's testing room a weight test is carried out if it is specially asked for, but as for being any guide or assistance in forming an opinion of the value of a cement it is absolutely useless, and, except when asked for, he has not carried out a test for weight for many years.

Another matter in which specifications often err, is in the wording not being sufficiently explicit. The following is a very favorite sentence, which, from its popularity, must be derived from some published work or specification of which the author is not cognizant; it is very misleading, and has, within his knowledge, led to more than one dispute. It is: "The breaking weight per square inch seven days after being made in a mould, and immersed in water during the interval of seven days, shall be," &c., &c. Now, the wording evidently reads that directly the cement is gauged and filled into the briquette molds, molds and all are to be put in water, because it would be evidently impossible to remove the briquettes from the molds until they were set, a proceeding directly contrary to practice and to reason, but one which the author has seen insisted upon.

Again, if the briquettes are not placed in water until twenty-four hours after they are gauged, which is the usual practice, it would become an eight days' test, *i. e.*, briquettes gauged on a Monday would be tested on the Tuesday week, leaving seven clear days in between, which, of course, is in favor of the manufacturer, because the cement has one day more to acquire its strength; but the wording is altogether vague and indifferent, and, worst of all, read it any way, it is not in accordance with ordinary practice, and comparative results between different samples of cement cannot be obtained if the same means and procedure are not adopted in every instance.

Specifications are matters which affect the manufacturer more than the user; the user has a right to ask for what he likes, and if the manufacturer undertakes to supply him, he is bound by his contract to do so. It is, therefore, surprising that manufacturers have not before now drafted a specification, and formulated a manner of procedure for carrying out a cement test which would satisfy the requirements of a good cement, and which they would be prepared to work up to. To enumerate a tithe of the absurdities which appear in cement specifications would take up more time than can be spared, and their consideration would not be profitable. Having given two instances, it may be at once stated that a cement specification is about the easiest and shortest to write, when once the nature of the material is known, and the particular requirements of the case ascertained. The object of testing cement is to determine its value as a constructive material; the object of the specification is to define the value which is required, and unless the latter is clear and explicit it is hardly possible for the manufacturer to comply with it, or for the test to be of much value.

So far as experience goes, the ultimate strength of Portland cement is not known, nor for practical purposes is it necessary that it should be. A good Portland cement is known to continually increase in strength for several years, and there is no reason to suppose that it will —within the limits, at all events, of the life of other constructive materials—in any way deteriorate. It must, further, be borne in mind that cement is always too strong for the purpose for which it is to be used, and that it is always weakened—or, as it were, thinned out—by the addition of sand or other ingredient. If, therefore, the strength of the structure is determined, as well as the proportion of cement to sand, or aggregate, which is to be adopted, the required strength of the cement may be ascertained—with this proviso, however, that the size, nature, and other properties of the sand or aggregate will materially affect the strength of the structure; therefore if only an indifferent quality of sand is obtainable, either more cement must be used, or a greater strength demanded. The simpler way, however, is not to specify or ask for a cement which is not usually made; take the ordinary article of commerce, and thin it out with as much or as little sand or aggregate as meets the requirements of the case. The matter of specification is thus reduced to the very simplest form, and the object of the test is to ensure the delivery being in accordance with the ordinary demand.

The only three properties of a cement which it is necessary to know in order to determine its constructive value are: 1st, its fineness; 2d, its tensile strength; and 3d, its soundness. Though usually placed in this order, it would be more rational if their order were reversed, for, if a cement is unsound it is evidently useless, while its fineness and tensile strength merely mean the use of so much more or less sand. Taking these, however, in the order in which they are here placed, the author will consider, first—fineness. A mortar or concrete is composed of a certain quantity of inert material called "aggregate," which is bound together by a cementing material, the matrix, and it is evident that, to secure a sound mortar or concrete, it is essential that each piece of aggregate shall be entirely surrounded by matrix, so that no two pieces are in actual contact. It is hardly necessary to explain that the finer a cement is ground the greater surface will the same weight cover, so that the finer a cement is ground the more economical will it be to use. But there are other reasons why it should be well ground. Cement, until ground, is a mass of partially vitrified clinker which is not affected by water, and which has no setting powers; it is only after it is ground that

the addition of water induces crystallization. It therefore follows that the coarse particles in a cement have no setting power whatever, and may, for practical purposes, be considered only as so much sand, aggregate or practically an adulterant. So far, therefore, it appears that an impalpable powder should be asked for; but here the question of cost and economy steps in. The manufacturer will say that it is possible to grind cement to any degree of fineness if paid for, but there is a degree of fineness which is economical, *i. e.*, when it becomes cheaper to use more cement in proportion to the aggregate than to pay the extra cost of additional grinding. With present appliances and means of grinding, that point is, in the author's opinion, reached when the cement is ground to such a degree of fineness that when sifted through a sieve having 2,500 holes (50 by 50) to the square inch, it shall leave a residue of not more than 10 per cent. by weight. Cement ground to this fineness will leave from 19 to 21 per cent. of residue on a 4,900 (70 by 70) sieve, and practically nothing on a 625 (25 by 25) sieve, and most manufacturers grind now to that degree of fineness.

Secondly, tensile strength.—It would be useless to recapitulate the numerous experiments as to the strength of cements which have been made from time to time by the author and others, and published in the Proceedings of this and other institutions. The deductions which have been arrived at by an examination of these tests are that the quick-setting cements and those which acquire great strength in a short time, or within the limits of test, do not, as a rule continue to increase in strength after a few months, and that after they have attained their ultimate strength they have a tendency to fall off. The extent of this depreciation has not been conclusively proved, but it is evidently not desirable to use a material which may, after a lapse of time, seriously deteriorate in strength. It is better to use a cement the strength of which is somewhat less at the early dates, but which it is known will continue to increase for an indefinite period. Such a cement is secure when it is slow setting, and when tested at different dates shows a marked increase in strength with the age of the briquettes. It was formerly the practice to demand the strength of a cement at the expiration of twenty eight days from gauging, but such a lapse of time is impracticable, because in most cases the cement would be in use before its strength was ascertained. The author has, therefore, entirely abandoned it, except as a corroboration of an opinion formed by tests at shorter dates. Seven days is, as a rule, the limit of time available for carrying out a test, but the strength at only that date would not enable a definite conclusion to be arrived at as to the value of the sample, and it is therefore desirable to carry out a test at an earlier date Three days is customary, and the ultimate strength of the cement is judged by the increase in strength between those dates. Experience shows that a cement need not have great strength when only three days old, but that it should show a marked increase between that and the seventh day's test. At the same time, in order to secure a material which will be capable of exerting, or rather of acquiring, a certain strength in a fairly short time, a minimum strength at both dates must be specified. The author is of opinion, and he is well supported in it, that a cement should carry, at least, 175 lbs. per square inch at the three days' test, and should show an increased strength of at least 50 per cent. at the seven days' test, but that the minimum strength at seven days should be 350 lbs. per square inch.

These strengths, of course, refer to the cement when gauged and treated in the ordinary manner, the briquettes being of the most approved form to resist a tensile strain. The form of briquettes which gives the best results is, so far as its form to resist a tensile strain is concerned, that designed by Mr. Grant, of the Metropolitan Board of Works, although it is slightly varied in other respects. Independently of the form of the briquette, there are, however, several other points which materially affect the results obtained, and though it is hardly within the province of this paper to give a full description of the means of gauging cement, still it would be incomplete without referring to them. They are—the amount of water used in gauging; the expedition with which the gauging is accomplished and the briquette formed; the

time which is allowed to elapse between the gauging of the briquette and placing it in water; and, lastly, the rate of speed at which the strain is applied to the briquettes when being tested.

There is no doubt that a briquette which is put in water directly it is set, will show a greater strength than if it is left in the air for a considerable time. But in adopting this practice, it is difficult to decide the exact time when a cement is set. If put in water too soon the strength is deteriorated. For this reason, and to secure uniformity in procedure with all cements, whether quick or slow setting, it is usual to place the briquettes in water twenty-four hours after they are gauged.

The second point really means skill in gauging, as it is evident that anyone accustomed to the work will be able to bring the cement to a proper consistency with less water, and in a shorter time than a novice, and the less water used and the quicker the operation is performed, the better will be the results. To obviate this difficulty as far as possible, the author some years ago devised a small machine in which to gauge cement, and he has since used it continuously. It is extremely simple, and consists of a stirrer revolving around its own axis in one direction, and around the pan the reverse. To use it, a weighed quantity of cement is placed in the pan, and the exact quantity of water (which, by previous experiment that particular sample of cement has been found to require) is put in at once, and the handle turned until the cement is reduced to the proper consistency. It is then turned out of the pan on to the gauging slate, and beaten up with the trowel into a convenient form and placed in the molds. It is then lightly rammed and shaken so as to remove all air bubbles, smoothed off on the top, and not touched again until the next day, when each briquette is marked and dated, taken out of the molds and placed in water, where again they are not touched until they are to be tested. In determining the strength of a sample it is well to take the average of five briquettes at each date, as from unforeseen causes one might not fairly represent the strength of the cement, and, with this object, it is well to have the molds arranged in nests of five.

In a paper which the author presented to the Institution of Civil Engineers last year, and which was published in their *Proceedings*, vol. lxxv., he gave the results of a large number of experiments he had made, with the view of determining the variation shown in the strength of a cement, due to the difference in the speed at which the strain was applied, and within the limits at which it would in practice be possible to apply the strain—viz., 100 lbs. per second, and 100 lbs. per 120 seconds. There was a difference in favor, of course, of the quicker rate, of more than 23 per cent. Without claiming any very great sagacity for carrying out this experiment, for in testing other materials the rate of speed is specified, the author believes he was the first to carry the matter out to a practical issue, for though some thought the strain should be applied quickly, and others slowly, there seemed no very clear idea of what was slow and what was quick. In order to arrive at a happy medium in this respect, the author is of opinion that a rate of speed of 100 lbs. per 15 seconds is a fair one. It is one he has used for a number of years, even before he made the above-mentioned experiments, and he sees no reason to alter it. Of course, any speed may be adopted, but if the usual speed—*i. e.*, 100 lbs. per 15 seconds—is not intended, the speed required should be mentioned in the specification.

With regard to the third point—soundness of the cement—it is to be observed that, naturally, if a cement is not sound —if it contracts or expands, cracks, or what is called "blows," it is absolutely useless, and it is desirable to determine in as short a time as possible whether or not a cement possesses this characteristic. Because a cement carries the required strain at the three and seven days' tests, it should by no means be assumed that it will not blow. Many cements are slow in developing this characteristic, and may even carry through the twenty-eight days' test successfully and blow after that. It is usual to make pats of cement on pieces of glass, or other non-porous material, to place them in water directly they are set, and examine them each day, and see if any cracks show themselves, first of all, around the edges. Now, with all deference to those who

carry out this experiment. The author regrets to say that he has never been able to arrive at any satisfactory conclusion from the examination of the pats. If the cement is really a blowey cement, the briquettes will develop the cracks as well as the pats, and, therefore, the pats are of no use; if the pats blow and the briquettes do not, and carry the desired strain when tested, the inference is that the pats were put in the water too soon, and before they were set, and thus the blowing is accounted for. Cements vary so much in their behavior when placed in water before they are set. A slow-setting cement, which should generally be a well made one, will fall entirely to pieces if put in water before it is set, and will afterwards harden up in the form in which it has fallen, while a quick-setting one, *i. e.*, an overclayed cement, will set quicker and harder if put in water immediately on being gauged; in fact, many cements may be gauged and made up into a ball and put in water at once, and will not alter in form nor disintegrate in the slightest degree, but will harden up as quickly, or even more quickly than if left in the air. To get over these difficulties the author uses in his testing room an apparatus by means of which he is able to determine the soundness of cement in twenty-four hours, or even less. He brings it forward with some diffidence, for the reason that though he has used it now continuously for some years, and has never been wrong in an opinion formed by its indication, in the hands of the unskillful or careless much harm might be done, and many good cements condemned; in fact, it is more suited to a laboratory than to a testing room. It is well known that a moist heat is conducive to, and accelerates, the setting of cement. A dry heat, although it may make a cement set quicker is not beneficial, because it tends to make it friable by removing from it the water which is required for its perfect crystallization. When a cement is set it may be materially hardened by immersing it in a warm silicious bath, and the combination of the moist heat and the warm silicious bath has formed the subject of a patent by the author for accelerating the hardening of concrete. Now, the author's method of determining the soundness of a cement is based on the theory that by accelerating the setting and induration of a sample, all its properties, good and bad, are developed in a short time, and hence the result, which, in the ordinary course of things, would not be known for, at all events, some days, and it might be weeks, is determined and ascertained in a comparatively short time. Following out this idea, the apparatus was designed, which consists of two vessels, the one inside the other, the annular space between the two being filled with water, which tends to maintain the inner one, which may be called the working vessel, at a fairly even temperature. Underneath is arranged a lamp or gas burner, which is so regulated that the water which fills the working vessel is maintained at an even temperature of 110° Fahr. Inside the working vessel, above the water line, are arranged ledges, on which the pats of cement are placed directly they are gauged, the slight evaporation from the water tending to keep the upper portion at a moist temperature of about 95° Fahr. A thermometer is placed in the water, and projects through the cover, so that the temperature may at all times be ascertained. It is also usual to insert a maximum thermometer, so that any undue increase in temperature through varying pressure of the gas may be ascertained. If, during the night or temporary absence, it is found that the temperature of the bath has risen much above 120°, the fact of a pat having blown is not taken as an indication that the cement is unsound, but that excess of heat has been the cause. Hence the diffidence of the author in placing the apparatus in unskilled or careless hands. Any sound cement treated in this manner will stand 120°—some much more, even as much as 140°; but many really unsound cements will stand 100° to 105° for the twenty-four hours without showing decided signs of blowing. The steamer, as it is called, for want of a better and more appropriate name, having been started and regulated, the pat of cement on a glass slab is placed on the rack directly it is made, where it is left for four or five hours. The exact time is not of much importance, but the pat should be perfectly set. It is then taken off the rack and put in the warm water underneath, where it is left for about twenty-four

hours from the time of its being made. If it is then still hard and tight on the glass there is no doubt that the cement is perfectly sound; if even it is off the glass, but presents a smooth glazed surface, the cement may be considered sound. If, having left the glass, it is cracked on the under surface, the cement is looked upon with suspicion, and another trial is made, as the seeming unsoundness of the pat may possibly only be due to the extreme freshness of the cement. If the pat is thoroughly blown, there is not much to say in favor of the cement.

The specification which is deduced from the foregoing remarks is extremely simple, and is as follows:

1. *Fineness.*—To be such that the cement will all pass through a sieve having 625 holes (25 by 25) to the square inch, and leave only 10 per cent. residue when sifted through a sieve having 2,500 holes (50 by 50) to the square inch.

2. *Soundness.*—That a pat made and submitted to moist heat and warm water at the temperatures and in the manner already described, shall show no signs of blowing in twenty-four hours.

3. *Tensile strength.*—Briquettes which have been gauged, treated and tested in the prescribed manner shall carry an average tensile strain without fracture of at least 175 lbs. per square inch at the expiration of three days from gauging, and those tested at the expiration of seven days from gauging shall show an increase of at least 50 per cent. over the strength of those at three days; but the briquettes broken at the seven days' test shall carry an average tensile strain without fracture of at least 350 lbs. per square inch.

Such a specification meets all requirements, and satisfies the peculiarities of nearly all cements, except, perhaps, the very quick setting ones, for which a slight variation in the tensile strength and percentage of increase between the dates named would have to be made.

The present paper would hardly be complete without a few remarks as to the manner of taking samples for testing. There are certain precautions to be observed, as well in the interests of the user as in those of the manufacturer, and it is always desirable, in order to avoid a multiplicity of tests, to obtain a fair sample of the whole delivery. If the sample is taken from bulk in the manufacturer's warehouse, at least seven or eight samples should be taken from each 100 tons. They should be taken from different parts of the heap, and not from the surface or outsides, but from about a spit down. These should be taken to a clean part of the warehouse floor and thoroughly mixed by being turned over and over with a shovel, and sufficient of this (about 10 lbs. is enough) should be taken for testing. If in barrels or sacks, one or two samples should be taken from each hundred and mixed in the same way. The samples in this case should be taken well from the center of the barrels or sacks, as the outsides may have been damaged by exposure or other causes over which the manufacturer has no control. Samples in this way should be taken from about each 100 tons and tested separately. It is always desirable, when time permits, and especially when sampling from bulk, to spread the cement out on a tray for a day or two to allow it to cool before commencing the test. Theoretically, a Portland cement may be used directly it comes from the millstones, but in practice this is not found to be the case, hot, fresh cement being always more or less dangerous to use, and the remark that applies to the cooling of the sample previously to testing also applies to the treatment of the cement before using it. As soon as it arrives at its destination let it be turned out of the sacks or barrels on to a cool and dry warehouse floor, and there let it remain as long as possible, turning it over occasionally. Unless it gets wet or damp, it will not deteriorate in quality, but will improve. It may perhaps be said that the manufacturer should deliver the cement properly cooled and fit for use, but independently of the purely commercial reasons which suggest to the manufacturer the advisability of not keeping too large a stock, the amount of warehouse space which he would require if this were the practice, would be out of all proportion to the size of the rest of the works.

In the early part of the paper the author used the word "adulterant" when referring to the coarse particles of a cement. He refers to it again, because the Cement-Makers' Union in Germany has

lately had a paper submitted to it, being the result of certain experiments by Dr. Fresenius, made with the object of deciding whether or no a cement is adulterated. Dr. Fresenius says that cement has lately been much adulterated with a material so similar to what cement should be, that even chemical analysis fails to detect it, and he therefore submits to the Union a number of tests, or rather chemical processes, by which such adulteration may be detected. The author is glad to say that in his practice he has never yet met with an adulterated English cement, any shortcomings in a cement having been due in all cases to more or less faulty manufacture, or to the use of improper raw materials. As every matter of chemical research, or it may be said of testing, connected with cement which emanates from Germany has great weight with many in this country, the author thinks it but right that he should say this much in honor of the English manufacturers who have not had the opportunities of the technical education which is so general in Germany, and which seems, in this particular manufacture, at all events, to have enabled the pupil to circumvent the master.

So far as regards these special tests advocated by Dr. Fresenius, the author is not in a position to say whether they are of much value; he is at present trying them in his laboratory, and may be able to say more about them on a future occasion. *Prima facie*, it would seem that they are not required, for evidently if the adulteration is carried to any great extent, the ordinary tests for tensile strength would detect it. However, the user may be quite satisfied that when using English cement, he may have a bad or an unsound one, which the tests advocated in this paper will detect, but he is not at all likely to meet with an adulterated one.

THE RELATIONS BETWEEN ENGINEERING AND ARCHITECTURE.*

By W. GOLDSTRAW.

From "The Building News."

What *are* the relations between engineering and architecture? We may take them to be, on a reduced scale, the relations between science and art. Indeed, it is scarcely an exaggeration to say, that the numerous objects brought together in the great museums of science and art are but illustrations of engineering and architecture in the widest sense, with their accessory arts and sciences. These two great departments of knowledge and skill are complementary to each other, as the masculine and feminine natures, strength predominating in the one and grace in the other; and although they have many characteristics in common, they have each their special place and functions. It may be useful, therefore, and will, at least, be interesting, to essay a brief consideration of their relative positions and values as honorable and lucrative professions. In order to get clear ideas on the subject, let us try to attach a definite meaning to the expressions employed. What is engineering? For an answer to this question we naturally turn to the great Society which is the recognized embodiment of all that is foremost in the engineering world. Now, the Charter of the Institution of Civil Engineers contains a lengthy attempt at a definition of "that species of knowledge which constitutes the profession of a Civil engineer." It is there described as "the art of directing the great sources of power in nature for the use and convenience of man, as the means of production and of traffic in States, both for external and internal trade." This is the gist of the definition, which then goes on to specify five main branches of "the art," "as applied (1st) in the construction of roads, bridges, aqueducts, canals, river navigations and docks, for internal intercourse

* A paper read before the Liverpool Engineering Society.

and exchange; and (2d) in the construction of ports, harbors, moles, breakwaters and lighthouses; and (3d) in the art of navigation by artificial power for the purpose of commerce; and (4th) in the construction and adaptation of machinery; and (5th) in the drainage of cities and towns." This definition is not very clear, and not quite comprehensive. There is no mention of railway, mining, hydraulic, gas, or electric engineering; and it is only with great difficulty that these important branches of the subject can be brought within the scope of definition. The fact that some of them had not been developed at the date of the Charter is not a sufficient answer to the objection, and even this explanation does not account for the omission of mines and waterworks. Too much stress is laid on using the power of Nature "as the means of production and of traffic" for purposes of *trade*, whilst, at the same time, what has come to be "sanitary engineering" is distinctly included. These considerations incidentally show the wisdom exercised by the founders of the Liverpool Engineering Society in adopting so expressive and practical and comprehensive a title, and in admitting to its membership "engineers of any branch of the profession." It is *engineering* with which we are concerned, not any one branch of it, not even such an extensive one as that known as *civil* engineering. And, without venturing on any exact definition, it will perhaps be sufficient to say that engineering is that entire system of knowledge and skill which comprises all mechanical pursuits so far as they supply the material wants of men. What is architecture? "The art of ornamental and ornamented construction," chiefly as applied to buildings and suchlike structures. Building, considered as a science, is clearly an important branch of engineering. And, as architecture is chiefly concerned with building, it follows that engineering is, in one aspect, an essential component of architecture, though the science may subsist without the art. In other words, whilst there can be no architecture, without engineering, there may be engineering without architecture. Therefore we are led to the conclusion that architecture is the development and refinement of an important branch of engineering. Thus, in a certain sense, the profession of architecture is, in its higher capabilities, more honorable than that of engineering. For it is disparaging to any particular architect to say of him that he is merely an engineer, since this is equal to saying that, so far as his artistic abilities are concerned, he is not an architect but a builder. And, on the other hand, it is not regarded as a discredit to an engineer to pronounce him to bè no architect. The sum of these considerations is that engineering construction is scientific and utilitarian, whilst architectural construction is not only scientific and utilitarian, but is also ornamental, and even artistic or beautiful. This distinction is not exact, and cannot be made so. At the same time it is practically convenient, and expresses the principal facts. Having cleared the way thus far, it may be profitable to inquire (1st) whether the relations between engineering and architecture are fixed and unalterable, and (2d) whether, if they are not permanent, it is desirable that they should be modified in practice. Now, when we endeavor to ascertain whether the relative positiôns of these pursuits are stable or not, we have to glance at their history. With regard to engineering, many of the mechanical arts and sciences comprised in it are so modern in their origin and development that they can hardly be said to have a history. Engineering, as a profession distinct from architecture, is a thing of today. Architecture also, as a "profession," is comparatively modern. But engineering and architectural pursuits have occupied men's talents and energies from the earliest times. They were always formerly practiced by the same persons. The new feature is that they now diverge into separate channels. This is, of course, only a phase of the 19th-century system of the division of labor. And as that principle is constantly operating in all departments of knowledge and skill, and must go on dividing and subdividing every trade and profession as the knowledge and skill grow more exact and positive, it appears quite probable that engineering and architecture, as now understood, will never again be practiced together to any great extent by the same persons at the same time. But, as we have said, it was not always so. And there is no reason in the essential nature

of things why it should be so now. Chiefly, what may be said is that the requirements of latter-day science have made it inconvenient and difficult for any one man to follow at once engineering and architecture equally well. And, secondarily, it must be acknowledged that modern ideas as to the province of the architect have much to do with the severance which we are considering. It appears to be taken for granted that the work of the architect should be confined entirely to buildings. But the modern historian of architecture (Fergusson) maintains that "there are no objects that are usually delegated to the civil engineer which may not be brought within the province of the architect. A bridge, an aqueduct, the embankment of a lake, or the pier of a harbor, are all as legitimate subjects for architectural ornament as a temple or a palace. They were all so treated by the Romans and in the Middle Ages, and are so treated up to the present day in the remote parts of India, and wherever true art prevails." Now, this is but equal to saying that in many large public works there is room for the engineer and the architect alike, or, at least, for their special talents. The architect should have some advantage, however, in the fact that the scope of his calling is wider, if fairly regarded, as it includes much that is simply engineering. But if the principle of the division of labor is to run to its natural issue, architecture will be considered as supplementary to engineering, not subordinate, perhaps, but rather superior, in the sense of its being the application of embellishment to the naked structure, or the incorporation of ornament into it, or the tasteful disposition of its parts. For, as Fergusson says, "where the engineer leaves off, the art of the architect begins. His object is to arrange the materials of the engineer, not so much with regard to economical as to artistic effects, and by light and shade, and outline to produce a form that, in itself, shall be permanently beautiful." If these considerations are allowed to have due weight, they tend to show that, although the connection between engineering and architecture has become relaxed, it is quite capable of being drawn tighter, and that the two branches of construction are by no means firmly settled apart, notwithstanding the force of convenience and custom, and the general disregard of art and beauty. It is, therefore, practically possible that the engineer should be more of an architect, and that the architect should be more of an engineer. We may now turn to the second part of our inquiry. For, if we have shown that the relations between engineering and architecture are not fixed and unalterable, the question naturally follows, Whether it is desirable that those relations should be modified in practice. Now, the answer to this question will depend upon another, which has already been touched at some points: How does the present arrangement work? Take, for instance, the specially modern case of a railway and its appurtenances. The actual railway itself, both as to the surveys for its course and the planning and construction of its different parts, is the work of the engineer. The tunnels and bridges are as properly assigned to him as are the track and signals. And, in many instances, the station buildings are regarded as coming equally within his province to design. If, however, the buildings are of great extent, and occupy an imposing site in a large town, they are sometimes put into the hands of an independent architect, with a view to insuring, amongst other things, a fairly artistic effect. This is constantly the case when the station buildings are connected with an hotel placed so as to mask the station itself. And, although the smaller or country stations are frequently designed by the engineers of the company, there are instances where high-class architectural firms are employed to take in hand everything in the nature of buildings connected with these stations, including even the roofs of iron and glass, which are often of greater extent than the actual buildings and attract more readily the notice of the public. So that in these examples, whilst we see the architect and the engineer each venturing into the other's domain, or what is usually so considered, we see also that the architect is the chief aggressor and gets most of the spoil. When, however, the engineer reaches the open country, or even the streets of the town, he works his own will on the bridges, viaducts, embankments, tunnels, ventilating shafts, *et hoc genus omne*. Especially with regard to goods

stations and warehouses, the engineer has it all his own way, and directs the expenditure of vast sums on these buildings, acting in the capacity of an ordinary architect. In this respect the architect may consider that his preserves are being poached by the engineer. Turning now to another branch of engineering, that connected with waterworks, what do we find? The reservoirs and pipe lines, and the works connected therewith; tunnels, bridges, and pumping stations, with their buildings and machinery, all come naturally within the engineer's legitimate business. And none of these works are now even thought of as belonging to architecture, although there is much scope for architectural taste in many of the embankments, aqueducts, towers, engine-houses and such like structures. At any rate, since the rise of engineering as a separate profession, the architect has had to yield up possession of these works. When, however, the water has been duly conveyed to a large town, and the question of providing public baths and wash-houses presents itself, the architect either steps in or is called in, and the buildings, at least, are made to receive the impress of his art, even though the actual purposes of the edifice have to be fulfilled by the special work of mechanical and hydraulic engineers. In some cases, it is true, the local authorities do not employ an independent architect to design such buildings, but entrust them to their own town surveyor, borough engineer, or water engineer, or whatever his official style happens to be. This officer, from the nature of his duties, has really a dual character. With regard to the now more or less distinct vocations of engineering and architecture, he has to fulfill a double function, which is, of course, not confined to the erection of the buildings we have mentioned, but extends to all the various engineering and architectural works of the public authority in whose service he is engaged. And so with the dock and harbor engineer. Although in his case, undoubtedly the bulk of his work is such as must be classified now-a-days as engineering, yet he is called upon to design and construct many buildings and other structures which have, or should have a decidedly architectural character, such as piers, lighthouses, hydraulic machinery buildings, public waiting-rooms and offices, clock towers, and other erections. Here, again, the engineer may be said to trench upon the hereditary domains of his cousin the architect. Even in connection with the partially lapsed art of canal making, the engineer is probably destined to renew his acquaintance with the architectural features of numerous locks, bridges, and aqueducts, to be constructed (even in this iron age) for the most part of stone, which has always been the pet material of the architect. As to the followers of the more purely mechanical branches of engineering, so closely connected with machinery, they are developing a kind of natural affinity for architectural work in quarters where it was least expected. In times not long past, the projectors of extensive factories and works to be fitted up with peculiar or costly machinery were accustomed to employ an architect for the erection of the building and a special engineer for the supplying and fixing of the machinery. Considering that an ordinary architect is equally ready to design a church or a distillery, it is hardly surprising that his Ishmaelite relation, the engineer, should wrest from him some of the specialties, and appropriate them to himself. Accordingly, we find new tribes of the great engineering family flourishing as gas works engineers, sugar works engineers, brewery engineers, cotton and silk mill engineers, and so forth, who undertake the designing and constructing of the great piles of buildings which are to enshrine the machines and engines required for that particular trade or industry. Occasionally it happens, nevertheless, that an architect of high standing, chiefly concerned with the more artistic side of his vocation, is employed to plan and execute buildings which are now by general consent, regarded as the proper work of the engineer. In such cases as these, whether it is of their own will or at the will of the public, the members of the two professions are playing a friendly game of tit for tat. The present condition of things, then, appears to be this: The practical relation between engineering and architecture are not sharply defined nor carefully observed. We may now recur to the question whether those relations ought to be modified. If so, should the two great branches of constructive

skill be drawn closer together, or should they be made more distinctly separate? Now, can it be maintained that the present state of affairs is satisfactory? This is not a quasi-philosophical question, but a very practical one. Two kinds of interests are involved in it—the interests of the persons whose occupation or livelihood is concerned in it, and the interests of art in its æsthetic aspect, whereby intellectual happiness is influenced. Well, in so far as uncertainty and confusion exist in the relations between engineering and architecture, it seems expedient that their boundaries should be more exactly laid down. Like two great political States, these two great professions, as they grow more powerful and approach more closely, have the greater need of a clear understanding as to their natural and scientific frontiers. In this age, few professional men can govern in both provinces. Even the admirable Crichtons will have enough to do with their talents in either domain. But as things are, we see one practitioner styling himself "Civil Engineer and Architect," whilst another is described as "Architect and Civil Engineer." These men are no doubt at present performing a special and useful function. But the race will die out. A pupil articled to such an engineering architect must be greatly perplexed by his divided allegiance to Rankine on the one side, and Palladio or Pugin on the other. It was much easier for Tintoretto to live up to his motto, "The day to Titian; the night to Michael Angelo," than for a nineteenth-century student to set his affections profitably on Gothic vaulting or the Ionic capital, when he is chiefly engaged in a sewerage scheme, or a system of tramways. Clearly, it is desirable that he should understand, as far as possible, the distinction between engineering and architecture, even if he has to draw an arbitrary line for his own observance. But in thus making the two professions more distinctively separate, there is no reason why the natural bond between them should not be respected or even drawn tighter. If architecture is the mother, and engineering the daughter, they should be on good terms. Nevertheless, a man does not marry his mother-in-law, and, as a rule, they agree better when living apart. Let the engineer and the architect each stick to his last. Whilst, however, he practices his special calling only, he ought to have a considerable knowledge of the other profession in those points where he necessarily touches it. The architect cannot be well qualified generally if he is ignorant as to the capabilities of iron columns and girders, and of concrete floors, the overturning force of the wind, the pressure of embankments against walls, and the laws of mechanics. Nor can the engineer satisfactorily design his bridges and towers if he has no knowledge whatever of either the Classic orders or Gothic styles. For, although, as Fergusson remarks, "it is not essential that the engineer should know anything of architecture, it is certainly desirable that he should do so. On the other hand, it is indispensably necessary that the architect should understand construction. Without that knowledge, he cannot design; but it would be well if, in most instances, he could delegate the mechanical part of his task to the engineer, and so restrict himself entirely to the artistic arrangement and ornamentation of his design. This division of labor is essential to success, and was always practiced where art was a reality; and no great work should be undertaken without the union of the two. Perfect artistic and perfect mechanical skill can hardly be found combined in one person, but it is only by their joint assistance that a great work of architecture can be produced." If this be so, and it will hardly be doubted, the work of the man who styles himself architect and engineer is not likely to be of the very highest merit. In the present relations between the two professions, however, such a practitioner makes himself respected or feared on both sides, and deservedly so. But as the distinction between them becomes better defined and more generally recognized by the public, his position will be increasingly difficult, and in the end untenable. This need cause no regret, for, as we have seen, it is desirable in the interests of both professions that they should be as much as possible *practiced* apart, even when a considerable acquaintance with both confers as advantage on its possessor. We appear, then, to have been led, whether we are willing or not, to the conclusion that engineering and

architecture ought to be made more distinctively separate. But can they not, at the same time, be in some way more closely united? If an ordinary man is not Colossus enough to bestride the strait between the two professions, may he not take his stand on one side and join hands with his friend on the other? Now this thought brings us to a practical suggestion, with which this paper may fitly be drawn to a close. Seeing that engineering and architecture are both concerned with building work, and must always approach each other more or less nearly, it would probably be a successful working arrangement in many cases if a well-qualified engineer and a well-qualified architect were to join in partnership. Such a style as "Septimus Jones, F. R. I. B. A., and Orlando Smith, M. Inst. C. E., Architects and Engineers," if justified by the quality of the work done by the firm, would carry weight with the public, and would secure many commissions which Jones or Smith by himself would fail to get, or would imperfectly carry out. Thus we finish with a marriage, and everyone is happy, or ought to be so. Should the blessing of Providence rest on it, and any good issue result from it, then the time spent in considering this subject will not have been utterly wasted.

THREE PROBLEMS IN RIVER PHYSICS.

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From the Proceedings of the American Association for the Advancement of Science.

In the past five or six years, there has been a great deal of money spent upon, and study given to, many questions of a scientific nature pertaining to the improvement of our western rivers. Your attention is called to some conclusions that may be drawn from recent investigations on the three following problems:

I. The Transportation of Sediment and the Formation and Removal of Sand Bars.

II. The Flow of Water in Natural Channels.

III. The Relation of Levees to Great Floods, and to the Low Water Navigation of Rivers.

I.—*The Transportation of Sediment and the Formation and Removal of Sand Bars.*

The solid matter carried by streams of variable discharge and cross-section, flowing over sandy beds, is at once the cause of, and remedy for, most of the obstructions to the navigation of such streams. A proper understanding of this subject is a prime requisite to an adequate conception of the conditions governing the flow of water in natural channels, the improvement of low water navigation, flood confinement, etc.

In all natural water courses, the material carried by the stream may be graded according to the method of its transportation, as,

First—That carried in continuous suspension.

Second—That carried in discontinuous suspension.

Third—That carried by rolling on the bottom.

Sediment in Continuous Suspension.—This is composed of such finely divided particles of clay and mud that any slight disturbance of the water in a vertical direction is sufficient to prevent its deposition, and so it mostly remains in suspension until it reaches the sea. A large part of the sediment carried by the Mississippi River is of this class. It mostly comes from the western tributaries, more especially from the Missouri. If a quart of this water be placed in a glass jar, and set away, it requires some ten days for the water to become clear. Evidently, this kind of sediment does not form sand bars, neither can the engineer avail himself of it in building artificial banks in contraction works. It is therefore of no consequence to the river engineer. It is neither helpful nor harmful, and may be ignored.

Sediment in Discontinuous Suspension.—This kind of sediment is composed of sand, more or less fine, according to the velocity of the stream. It is the material of which sand bars are made, and it is also the material out of which the engineer builds his artificial embankments by the square mile behind his permeable dykes. It is constantly being picked up at one point and put down in another by the action of a law as unchangeable as the law of gravity. In the river's tireless efforts to attain a uniform flow, it is constantly scouring out the engorged sections, and filling in the enlarged sections. For any given stage of water, the volume discharged at successive sections is the same, but since the sections do not have equivalent areas the mean velocity at successive points is a constantly varying quantity. The river undertakes to overcome this inequality and so engages in an enormous carrying trade, which consists in cutting out the bottom where the section is less and the velocity more, and carrying this sand to the first point below where the section is more and the velocity less, and there it is deposited. Evidently, if any given stage of water should continue long enough, this double action of scouring the small sections and filling the large ones would result in a nearly uniform flow, when such small particles as could be carried by the resulting velocity would be so carried in permanent suspension, leaving a bed of tolerably permanent character, being only influenced by the matter rolling on the bottom. But here comes the trouble. No sooner has the river set to work to adjust itself to any given stage, than behold, the stage changes, and then it must go to work, cutting and filling at new points, and as the stage is ever changing, the river is ever beginning anew in its endless task.

It may not be clear why this change in stage should so disarrange matters. Let us see. The cause is the great variation in width. Take an actual case. At Plumb Point, on the Mississippi River, sixty miles above Memphis, the following extremes were found in 1879 on a reach eight miles in length.*

		Wide Section.	Narrow Section.
Surface width, in feet,	High water.	8840	1680
	Low "	7330	1375
Mean depth, in feet,	High water.	23.9	60.7
	Low "	6.0	23.9
Area of section in sq. ft.	High water.	260000	94500
	Low "	30220	68500
Mean velocity in ft. per sec.	High water.	3.67	10.1
	Low "	2.65	1.15
Fall per mile in feet,	High water.	0.26	1.75 †
	Low "	1.15	0.07

Thus, at low water, the wide section had a mean velocity of 2.65 feet per second, while the narrow section had a mean velocity of only 1.15 feet per second. At a stage 26.6 feet above low water (which in the table here given is called "high water," although it is some eight feet below extreme high water) the conditions are reversed, for now the wide section has a mean velocity of 3.67 feet per second, while the narrow section has a mean velocity of 10.1 feet per second. Here the wide section was some eight miles above the narrow one. Evidently the river is cutting out the shoal and filling up the pool in low water, and cutting out the pool and filling on the succeeding shoal in high water, and in fact this is what is always observed to occur.

The amount of this local scour and fill on the Mississippi river is enormous. In the case above noted there was a total scour over the reach of 350,000,000 cu. ft. or enough to cover one square mile to a depth of 11 feet. This occurred from Nov. 13 to Jan. 3. In many cases the rate of fill and scour, as well as the total amount, is much greater than this. It is not uncommon for the shoal places to be built up as much as from six to ten feet in time of high water, so that the bottom of the river is then higher than the surface of the water will be after this same fill has been cut out by the succeeding low water. In other words, the river bed is a succession of narrow, deep pools, alternating with wide shoals. The pools are in the bends, and the shoals are on

* See Report of Miss. Riv. Commission for 1881, p. 69.

† From plate 7, p. 120 of same report.

the crossings. The shoals are like so many dams, fifty to seventy-five feet in height, stretching across the river bottom say every ten miles. In high water these wide dams are built several feet higher, and the narrow pools are dug several feet deeper. In time of low water the dams are scoured down several feet, and the material deposited in the succeeding pool. What is true on the Mississippi river on a large scale, is true of every stream flowing in a friable bed, on a corresponding scale. This sediment is, therefore, said to be in temporary or discontinuous suspension. It settles very quickly where a sample is caught in a vessel, and the amount carried by the river is only determined by taking samples at various depths. There is little of it near the surface, except when there is violent vertical agitation, as in "boils" and eddies. It is constantly and rapidly falling towards the bottom, and is only prevented from reaching it by the upward motion of the medium.

Material Transported by Rolling on the Bottom.—When the grains of sand are too large to be held in temporary suspension they may yet be moved along on the bottom from being unable to resist the force of the current. This motion is also mostly discontinuous inasmuch as the particle is pushed up the gentle slope of the upper side of a sand wave or reef, and dropped upon the abrupt down-stream side, there to remain until the reef has progressed so far as to again expose it to the action of the water on the up-stream side. The amount of material so transported is fairly represented by the size and rate of travel of the wave.* These sand waves, or reefs, may be as much as 8 to 15 feet high, extend from one-half to three-fourths the distance across the river, and move from 10 to 30 feet a day. They succeed each other at intervals of some 300 feet. The same action is seen by the roadside, on a small scale, after a shower. It obtains in all natural water courses, is found in both pool and shoal alike, but is most strongly developed on the shoals. It is largely instrumental in the formation and removal of sand bars, but is of little use to the engineer in building embankments. It is in this way, however, that the slow currents of a low stage may cut a channel through a wide and high bar. Although the mean velocity may be small, the bottom velocity is relatively large on account of the shallow depth. The period of its action is also apt to be longer than that of flood stages. The amount of matter so transported is, however, small on the Mississippi river, compared to that carried in suspension. A knowledge of this action is mainly important as going to explain the regular changes that are observed to take place on the bottoms of cross-sections of rivers.*

* See Report of Chief of Engrs. U. S. A. for 1879, Vol. III., p. 1968: also, Reports of Miss. Riv. Com. for 1881 and 1882.

II.—*The Flow of Water in Natural Channels.*

It is now generally admitted that no formula, involving only the variable functions of slope and cross-section, can ever be used to give even approximate values of the mean velocity, across a given section, of a stream flowing in its natural or irregular channel. Farther than this, we are now forced to the conclusion, that no such formula can ever be used to give the continuous discharge of a single stream, at a given station, even though the actual observed mean velocities on that section, for one year, be used in its derivation.

The common type of formula is

$$v = c\, r^m s^n$$

where

v=mean velocity across the section.

c=a constant coefficient, either general or specially derived for each case.

r=hydraulic mean depth $=\dfrac{\text{area of section}}{\text{wetted perimeter}}$.

s=slope=sin. of angle of inclination to the horizontal.

m and n are usually taken as $\frac{1}{2}$.

The Chezy formula, which is of this type is:

$$v = c\sqrt{rs}.$$

Most of the efforts made to adapt this formula to the flow of water in natural channels have been spent upon the coefficient c.

It is, however, no longer a question of coefficients or exponents. For regular

* For an article on "Silt Movement by the Mississippi," by Robt. E. McMath, see "Journal of the Association of Engineering Societies," v. 1, p. 266.

and stable channels, like flumes, canals, or ponds controlled by fixed weirs, formulæ can readily be deduced that will give fair results, but in all natural channels, the cross-section, slope, and flow, are so irregular, and the bed so changeable, that the variables affecting the flow are not adequately represented by *r* and *s*, and hence, any formula based on these for the independent variables will surely fail.

A body of finite mass, moving under constantly varying accelerating and retarding forces, never has a velocity at a given point which is a function only of the forces acting at that point. Since the accelerating forces of a stream are due to the slope, and the retarding forces are due mainly to bed and banks, and since the conditions of slope, bed and banks are constantly changing in the progress of a given mass of water down stream, it follows that this mass is subject to constantly varying forces. The velocity of this mass at any section is therefore a function of the slope and bed for an indefinite distance above, and can never be predicted from the immediate conditions at the section.* Whether or not it can ever be predicted, within reasonable limits, is still a question, but some new light has recently been shed on this much mooted subject. If the conditions of slope, approach, cross-section and discharge, should be always the same at the same stage of water, or for the same stage and same rate of rise or fall, then a cycle of observations on mean velocity might enable us to predict what the next cycle of mean velocities would be at that same section. But, as outlined above, in discussing silt movement, it is seen that, in streams of unstable *regime*, the bed is constantly shifting, and that the high ridges, or bars, that alternate with the pools, are constantly changing their height. If these ridges be conceived as weirs, and the stage of water in the intercepted pools be controlled by these weirs, which it is in medium and low stages, then the discharge in this pool may be discussed with some confidence and profit. If the section be located in the lower portion of the pool, so that the conditions of approach over the weir above be of little consequence, then the conditions effecting discharge are, the *effective head*, that is, the surface slope from the section to the weir below, and *the depth on the weir*. The origin, from which the stage should be measured, in this case, is the natural zero of hydraulic activity, viz.: the horizontal plane passing through the crest of the weir. When so taken, a very simple relation is found to exist between *stage* and *mean velocity*, inasmuch as the latter is a linear function of the former.* If this relation be determined by observations at that section, then it may safely be used *so long as the weir condition remains fixed.* When this changes, by scour or fill of the bar, then the origin of the locus changes with the change in origin for stage. But the weir condition could be made the subject of observations, and the locus of the curve of mean velocity and stage so adjusted as still to indicate the mean velocity for this locality, provided the section on weir or bar should remain, the section controlling or limiting the discharge. In high stages, the region over the weir becomes itself the comparatively stagnant pool, and the controlling or limiting section is in the deep and narrow bend below. The low slope is now over the shoal, and the steep slope in the deep pool. This slope is used to generate the accelerated velocity through the engorged section. The condition of a slack pool, retained by a submerged horizontal weir, now no longer obtains, and the narrow, deep reach around the bend with high slope, is similar to an engorgement by lateral contraction. This is a very different problem, evidently, from what we had at a lower stage when the weir section was the limiting one, and a discussion of this state of affairs offers greater difficulties than the other. As a striking example of this change of location of the engorged sections, see the table given above in discussing the movement of sediment. Thus, in low water, the deep section had an area of two or three times that of the shoal section, and a slope of one-sixteenth as great, while

* The problem is similar to the instantaneous relation existing between the pressure on the piston of a crank engine, the resistance to motion, and the velocity of the fly wheel. No one would for a moment suppose that a formula could be derived that would express this relation for any given instant, and yet this is exactly what has been attempted on streams for the last one hundred years.

* The locus is probably an hyperbola, which becomes practically a straight line for all stages above a few feet. See paper by R. E. McMath before Am. Soc. of Civil Engineers. V. 11, No. 239.

at high stage, the deep section had an area of only thirty-six hundredths of that of the shoal section, but its slope was seven times as great.

The object of this presentation is not to discuss or derive formulæ, but to call attention to some of the heretofore neglected functions of the problem. These functions are seen to be so various, and apparently lawless in their nature, that it is highly improbable that we shall ever be able to obtain any formula of much value. A service has been rendered, however, when the impossibility of a successful solution on the lines of investigation so long pursued has been pointed out. Also, when the real determining causes effecting flow are properly conceived and investigated, rational and valuable formulæ may be obtained for exceptional cases of stable conditions.

III.—*The Relation of Levees to Great Floods and to the Low-water Navigation of Rivers, as Illustrated by the Mississippi River.*

That levees may be built which will confine the greatest flood on the Mississippi river, without danger of their giving way, may be admitted. The question is, What shall be their size and location?

The location of a levee is determined by two considerations in favor of putting it as near the bank as possible, and by two others in favor of putting it as far away as possible. The arguments in favor of putting the levee near the bank are:

First—The land near the bank is generally higher than anywhere else. There is generally a well defined slope away from the river, and therefore, a levee here, to confine a given flood plane, will have the minimum height and cost.

Second—The land near the river bank is always most valuable for cultivation on account of its being higher, and therefore the protection of this narrow strip is the chief cause for leveeing the river at all.

The arguments in favor of putting the levee at a distance from the river are:

First—So that it will not fall into the river from craving banks, and *Second*, so that the facilities for flood discharge may be greater, and therefore, the flood stage and necessary height of levee less. This latter argument is so generally advanced that it is deserving of mention, but I will try to show that it is more visionary than real. The force of the other arguments above stated is evident, and they need no further discussion.

The height required for levees in a certain locality is a very complicated problem, and unfortunately for the lower Mississippi river, it is a problem on which we have no direct evidence. By having no direct evidence, I mean that, as there is no point on the river below Cairo where the whole of a great flood has ever been confined between levees, so there is no argument from experience.

In this dilemma we are forced to fall back upon theoretical considerations.

For the past three years, 1882,'3 and '4, we have had great floods on the Ohio and Lower Mississippi rivers, and a great deal of data has been obtained, such as we have never before possessed. The first installment of this flood data may be found in the Report of the Mississippi River Commission for 1883.*

The overflow water of a great flood loses itself out over the west bank of the river from Cairo to Memphis, passes down through the St. Francis swamp, and is forced back to the river again by the high bluffs at Helena. It then overflows the east bank, passes down through the Yazoo bottoms, and is again forced back to the river by the bluffs at Vicksburg. It then once more crosses the channel and escapes over the west bank to return no more, but to find its way to the gulf through the Tensas and Atchafalaya river regions.†

Notwithstanding the river has been leveed almost from Cairo to the Gulf, and in the lower parts of the valley these levees have been carefully maintained and gradually enlarged, yet in time of a great flood the surplus water seems quite oblivious of such frail barriers, and always goes over and through them very much as though it were quite unconcious of their presence. And every time the levees are destroyed the people seem as much astonished as though that were the first time such a calamity had ever befallen them. The trouble is that due weight

* Supplemented in the Report of 1884, by several reports on the observed volume of the overflow water in the flood of 1882, including one by the writer on the Flood Discharge through the Yazoo Bottoms.

† See paper by the writer on "Great Floods on the Lower Mississippi," in Journal of the Association of Engineering Societies, v. 2, p. 115.

has not heretofore been given to the enormous quantity of water passing outside the channel. Before the flood of 1882 no even approximate determination of the amount of such water had ever been made, and no one suspected it to be so large. From the observations taken on that flood, which was the largest on record, we find that on some zones, or belts, there was about as much water passing outside the channel as there was in the channel. The total maximum discharged in that flood was some 2,000,000 cu. ft. per second, while for a hundred miles (by river) at and above Lake Providence, and for another hundred in the vicinity of New Orleans, the discharge in the channel, with overflowing banks, was but 1,000,000 cu. ft. per second. The other million was finding its way to the gulf through the swamps. This is a very startling fact. It means that if we propose to completely confine the waters of a large flood between levees, we must, in places, carry twice as much water as the present channel can take. At all points below Cairo, it is now pretty well known how much more water would have passed in a confined channel, than did so pass in the flood of 1882. With such facts as a basis for estimates, engineers still differ very widely as to the necessary height the levees must have to confine these additional volumes. Some engineers affirm that the levees need be but little, if any, higher than they now are, while others insist that in places they will have to be from ten to twelve feet higher than they now are. The whole question turns on whether or not the bed of the river will scour out when all the water is confined to the channel. The popular conception, as well as the expressed opinion of most writers on the subject, is to the effect that a confining of the waters to one channel will increase the velocity of the stream (which everybody admits), that this increased velocity will give increased scour (which everybody admits, in a sense) and that this increased scour will result in a general lowering of the bed (which must be denied).

In other words, there are two schools of engineers, the concentration school, and the equalization school. The former affirms the beneficial results of an increased volume at high stage; the latter not only denies any beneficial results, but affirms that actual harm would ensue. Again, the question turns on the methods of transporting sediment. The concentration school argue as though all sediment were of the continuous type, and when once taken up it would be carried to the sea, provided all the water remained in the channel. But they say, in effect, that in case of a flow over the bank, while the water pauses to consider whether it shall go over the bank or remain in the channel, the sediment escapes from its grasp and settles to the bottom. On the contrary, we know, that when two forces, at right angles to each other, act on a moving particle, it moves over a path which is a resultant of the two forces, and so moves faster than if either force acted alone. Therefore, instead of the water being checked by the escape over the bank it is really accelerated. Again, the more the energy of a river is increased by increasing the stage the more unequal becomes the difference in area of successive sections in wide and narrow places, and at high stage the narrow and deep places are always the engorged sections. If the energy of the river be increased at this stage it simply scours out the deep places deeper, and drops this matter on the succeeding wide shoal. That is, any influence toward a temporary adjustment of the river's bed to a high water discharge is an influence which is adding to the heights of all the sand bars on the river. The greater the stage, the higher the bars and the deeper the pools become; and so far is this from facilitating the discharge, that, in the language of the Secretary of the Mississippi River Commission, "the effect of an approaching flood is to impede its own discharge, and the impediment outlasts the flood." An example of this action is found in the flood of 1858, at Columbus, Ky., where the river was leveed. The discharge of the river exceeded 1,100,000 cu. ft. per second, four times from December to June. The stages at which the river discharged this amount were: 29.5 ft., 32.5 ft., 33.5 ft., and 35.0 ft. on a rising stage, and 31.6 ft., 34.7 ft., 35.9 ft., and 36.9 ft. on a falling stage.

If the successive differences between rising and falling stages at which the discharge was 1,100,000 cu. ft., be taken out for each rise, we have 2.1 ft.. 2.2. ft., 2.4 ft. and 1.9 ft., as the legitimate effect

of the change from a rising to a falling condition.

If, however, we compare the first and last rising stages when the discharge was 1,100,000 cu. ft., we find that in June it required a stage 5.5 feet higher to discharge this water than it did the previous December. Making the same comparison on falling stage we find a difference of 5.3 ft. This was due to deterioration of channel caused by a building up of the bars or weirs, by a sufficient amount to raise the stage 5.5 ft. in order to discharge the same amount.

It is highly probable that the two high waters of 1882 and 1883, in the Ohio River worked such detriment to the channel that the flood of 1884 was raised several feet higher than it would have been had the same amount of water passed in 1882.

It would seem, therefore, that in the river's present condition, there is no evidence that a confined flood will scour out its bed so as to facilitate the discharge, and there is considerable evidence against it. If the river flowed between straight, parallel bank, such as Capt. Eads has constructed at the mouth of the river, then there could be no such thing as discontinuous transportation of sediment, and hence no alternate scour and fill. Then concentration of volume would be beneficial, and would ultimately lower the river bed. But this condition of things can never be reached on the Mississippi River, and hence the concentration of flood volume will ever be harmful rather than helpful.

It follows from this, that if an additional quantity of water is confined to the channel in time of flood, we must provide accommodation for it at the surface rather than at the bottom of the river. If this point be settled, it is not difficult to approximate to the new stage to pass the new volume. There are many stations on the river below Cairo where discharge observations have been conducted for one or more years. If, for one of these sections, we plot stage and discharge, we get a curve showing the relation of the two from low water to say 50 feet above. If the levees be near the banks, which they usually are, and they be high enough to confine the flood, the condition of flow will not be materially changed as the water gets above the bank. If, now, we know what the additional quantity will be to confine it all, we could *simply extend the discharge curve* until we reach the proper volume, and then read off the corresponding stage. The only uncertainties here are in the curve itself, which may not rigidly apply for another year at the same locality, and in its extension. These uncertainties may have a maximum value of several feet, and yet it is the only argument we can use, and it will give approximate results, which are more likely to be too small than too large. If the heights of levee necessary to confine such a flood as that of 1882 be found in this way, and the additional cost estimated, it makes an item of over $50,000,000, to complete the levee system from Cairo to the Gulf. It is questionable if such levees ever will be built.*

In regard to putting the levees farther from the banks, the two main objections have already been mentioned. There would be also little advantage from increased facility of discharge. The ground would have to be kept clear of all timber, weeds, and undergrowth, or else it would offer very poor opportunities for discharge. This could hardly be done. The water flowing across a neck, also, does more to hinder than to help the flow around the bend, by lessening the volume in the channel.

There is another expedient which has been advocated, which appears to be practicable, but for which there are yet not sufficient surveys to determine. It consists in having ordinary sized levees, such as are now built, which will confine the ordinary floods, with waste weirs at such points as offer the best facilities of discharge through the swamps, to let off the surplus waters without detriment to the levees and with the least possible damage to the lands. There are a great many natural outlets from the river back to the main drainage channels of the bottoms, which could be utilized in this way. This sort of outlet, however, is more like the spilling of the water over the bank, than like a crevasse in the levee. These weirs would have to be miles in extent so that the depth of overflow need not be more

* The Miss. Riv. Com., in their Report for 1883, estimate this cost at some $11,000,000, but Gen. C. B. Comstock, President of the Commission dissents from this view.

than two feet. This arrangement has been adopted on the Rhone with very satisfactory results.*

The relation of levees to low water navigation has already been trenched upon above in showing the effect of increased stages at time of high water, in building up the bars. What is wanted is the removal of the bars, and for this purpose the low water energy should be increased and the high water effect diminished. In other words, the extremes should be brought nearer together, which is all included in the term equalization of volume. Any influence, therefore, which forces these extremes farther apart is hurtful. Levees tend to make high water higher, and therefore, they are an injury to navigation.*

Evidently it would be of no advantage to navigation if the river bed were uniformly lowered a hundred feet, provided the same irregularities remained in the matter of pool and shoal.

If ever the river should be regulated in width so that the discontinuous movement of sediment becomes inappreciable, then the stage may be increased to advantage. It is highly improbable that this ever will be done, and therefore, the building of levees on the banks of the Mississippi River will never prove an aid to low water navigation. On the contrary, they will always tend to produce a certain amount of damage.

* See paper by the writer on "Protection of Lower Mississippi Valley from overflow" in Journal of the Association of Engineering Societies, v. 3, p. 169.

* See paper by Robt. E. McMath on "Levees,—their Relation to River Physics," in Journal of the Association of Engineering Societies, v. 3, p. 43.

A COMPARISON OF BRITISH AND METRIC MEASURES FOR ENGINEERING PURPOSES.

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Proceedings of the Institution of Civil Engineers.

I.

Some of the existing systems of measures, such as those of time, angles, velocity and electricity, are practically international. This paper may therefore be limited to comparing the advantages for engineering purposes of those measures of length, surface, capacity, weight and pecuniary value, adopted in England, with the metrical and decimal systems of the same classes of measures.

The system of arithmetical notation used by all civilized nations is decimal; consequently all decimal systems of measures have the advantage over others of dispensing with the compound rules of arithmetic, and of facilitating the use of logarithms in engineering calculations. Among decimal systems of measures, the metric system has the advantage of being already used by 240,000,000 of people, with whom 60 per cent. of the foreign trade of England is carried on, and of all its terms being correlated and systematically derived from one convenient, well known, standard measure of length. The natural character of the metric unit, and the accuracy, or otherwise of its relation to the meridian is practically immaterial. The collection of measures used in England for engineering purposes is partly duodecimal, partly decimal, chiefly binary, and generally unsystematical. Some British measures have the advantage over all decimal measures of being capable of finite ternary subdivision, but this advantage is of comparatively little practical importance.

England and the United States of America hold at present such a prominent position in the manufacture of engineering machinery and material constructed to British scales of measurement, that a considerable practical advantage is on that account derived from the use of British measures in engineering works. The capital invested in machinery constructed to manufacture to sizes of British measurement is very large, and the literature on engineering subjects expressed in terms of British measurement is most

extensive and valuable. English and American engineers have their minds saturated with guiding or standard dimensions expressed in British measures and ready for application; so that the substitution of other measures in their general practice would prove a source of considerable personal inconvenience to the current generation of engineers in England and the United States. Most British measures in common use have the great advantage of being already familiar to the masses of the English and American people, and to the large number of skilled workmen which these people include.

As regards the depreciation in value of English engineering literature, written in terms of British measurement, that would ensue on the introduction of metric measures, it may be remarked that, long before the existing generation of English and American engineers passed away, the gist of the information contained in English engineering books would be translated into the international language of measurement. The question of the depreciation in value of costly machinery, constructed for manufacturing articles to scales and exact sizes of British measures, is more difficult, and the loss and inconvenience from such depreciation must form a considerable set-off against the advantages to be derived from a general adoption of the metric system. It must be remembered, however, that the progress of improvement in machinery, and the wear and tear of existing machines are so rapid, that in a comparatively few years most of the existing individual machines of this kind would either be worn out, or have become obsolete, from other causes than a demand for articles manufactured only to regular metric sizes.

Both the meter and the British standard yard have the advantage of being the approximate length of a human stride, which has always been the readiest and most extensively used natural unit of linear measure for engineering field-work. The length of a meter is about the limit to which a human being can conveniently place his hands apart when measuring with accuracy on a vertical surface.

It may be observed, incidentally, that the British foot is, strictly speaking, not a British standard measure, but is simply a ternary subdivision of the standard yard, and is itself divided duodecimally into inches, while the inches are again divided binarily and fractionally into quarters, eighths and sixteenths. In much actual practice the foot is not even adopted as a unit of linear measurement; for instance, most carpenters use a two-foot rule, and having found the number of lengths of their two-foot rule contained within the length of the plank they are measuring, they proceed to compute this length in feet by doubling the number of two-foot-rule lengths they have measured, and then adding to this the odd foot and inches and fractions of inches which may go to complete the total length of the plank. A workman measuring with a meter simply measures the number of meters, and reads off at once the decimal parts of a meter which make up the total length of whatever he is measuring. It may be said that the amount of computation required to double the number of two-foot-rule length is so small as not to be worth consideration, and that such an objection to measurement by feet is merely fanciful; but for all that it cannot be denied that an additional mental process has to be exerted by a British workman every time he measures a length with a rule which does not coincide in length with the unit in which he expresses the total length measured. A plank can be measured with accuracy with a meter in two-thirds of the time required to measure it with a two-foot rule, and in one-third of the time required with a one-foot rule; and in a carpenter's day's work the sum of these differences of time is by no means unimportant.

The British inch is much too large to serve as the lowest integral unit in a scale of linear measurement, and even the sixteenth of an inch is too large for minute work. The instruction so often given to a British workman to make an article "five-sixteenths full" or "bare three-eighths" are hardly compatible with precise accuracy of workmanship. The millimeter is, on the other hand, a very convenient unit for ordinary minute work, and its decimal subdivisions are quite as convenient for microscopic work as thousandths of an inch. Another example of the additional computation involved by English practice is the taking

of levels for building work in feet and tenths and hundredths, and the necessary translation of the resulting dimensions into feet, inches, and fractions of inches for the workmen.

For all ordinary field-leveling, the division of a leveling-staff into decimeters and centimeters only, is sufficiently minute. The centimeter can easily be binarily subdivided by ocular estimation, and the practical advantage of reading to the hundredth of a foot on a leveling-staff in straightforward leveling is very doubtful. Certainly more rapid, and probably quite as accurate, leveling can be done in a day with a staff subdivided in centimeters as with a staff divided into hundredths of a foot, and the practical accuracy of metric leveling when applied to building work will be greater, as the dimensions given to the workman will be in the terms actually noted through the level.

The chain used in the metric system for field work is usually 20 meters long, or 4½ inches shorter than Gunter's chain, though for difficult country a 10-meter chain is preferable. Assuming that the length of Gunter's chain is that found most convenient for field-work, the twenty-meter chain has practically the same advantages in that respect, but the use of a twenty-meter chain involves additional computation, owing to the number of arrows held by the rear chain-man having to be doubled to get the number of decameters to be entered in the field book. The use of a ten-meter chain of course does away with this objection, and it has the same advantages as regards direct correlation with the hectare as Gunter's chain has with the acre. The twenty-meter chain has the advantage over Gunter's chain that it is capable of subdivision into the smaller linear measures, whence Gunter's link is the lowest integral subdivision of Gunter's chain, and is too large for any work but land measuring.

For purposes of engineering calculation, it is obviously desirable that the unit of linear measure should have its multiples and submultiples arranged in accordance with the universal system of arithmetical notation, that it should be the basis of the units of measure of surface, capacity, and weight, and that it should be as international as possible. The meter complies much more fully with these conditions than the yard. In all questions relating to the pressure of materials upon surfaces, the intimate correlation existing in the metric system between the units of measures of weight and capacity is valuable, and hydraulic calculations are much simplified thereby. Between the English horse-power of 550 foot-pounds per second and the metric horse-power of 75 kilogrammeters per second there is little practical difference in point of convenience, but it is highly desirable that scientific units of this nature should be international.

An engineer using the metric system has practically but one denomination of measurement to deal with throughout his work, both in the field and in the office; all his measurements and the scales of all his plans being expressed decimally in terms of a meter. An engineer using British measurement measures a line of works in chains, furlongs, and miles; takes levels in feet divided decimally; gives dimensions for buildings in feet divided duodecimally into inches and in inches divided fractionally; calculates earthwork in cubic yards; measures brickwork in rods, and land in acres, roods, and poles; measures lime in bushels or barrels, water in gallons, and weighs materials generally in tons, hundredweights, quarters, stones, pounds, and so on, almost *ad infinitum.* Then when he comes into the office he plots his surveys in scales of chains to the inch and feet to the inch, and makes his drawings in scales of various fractions of an inch to the foot.

Besides the measures and scales above specified, the English or American engineer has to deal with an infinite number of gauges, trade sizes, and trade and local customs which render comparison of cost exceedingly difficult, and many of which would become obsolete if the metric system were in general use. The habitual employment of British measures debars English and American engineers to a great extent from profiting by the extensive and valuable foreign technical literature written in terms of metric measurement. However perfect they may be in knowledge of foreign languages, their minds cannot, if they have been accustomed to work only with British measures, either grasp or retain with facility, dimensions expressed in metric measures, and they are therefore partially shut out from a very extensive source of profes-

sional experience. The advantages of an international language of measurements for engineering purposes are so great, that they may be assumed to counterbalance any advantages claimed on behalf of any new decimal systems based on units of British measurement.

In so much of the routine work of a civil engineers' office as consists in taking out quantities and making estimates, about one-third more work can be done within a given time with metric measures than with British measures, though, of course, the loss of time can be somewhat reduced by the free use of ready-reckoners and tables. On this account, engineers accustomed to and using metric measures, have an advantage over engineers using British measures. It has been urged as an objection to decimal systems that they are not suitable for binary subdivisions, and alleged that binary subdivision is the most convenient for common purposes. It must be remembered, however, that as all binary divisions admit of expression in precise decimal equivalents, there is nothing to prevent binary divisions of a metric unit expressed in fractions, such as $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, &c., being used by those who prefer them. The metric scales for plans are simple and convenient, and have the advantage of enabling the mind to compare very easily the dimensions on plan with the actual distances in nature.

The English practice of plotting plans to scales of so many of Gunter's chains to the inch, expressed in feet, seems to have little to recommend it beyond the questionable advantage of causing the admeasurement of such plans by the uninitiated to be particularly difficult. In a paper "On the Introduction of the Metric System into Machine Shops in America," Mr. Coleman Sellers, M. Inst. C. E., points out that the metrical scales only admit of a descent in integral vulgar fractions from full size to $\frac{1}{2}$, $\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{20}$, $\frac{1}{25}$, $\frac{1}{50}$, whereas the ordinary inch rule furnishes scales of $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{12}$, $\frac{1}{16}$, $\frac{1}{24}$, $\frac{1}{32}$, $\frac{1}{48}$, $\frac{1}{64}$, making twelve scales in all, as compared with seven metrical scales. Draftsmen, however, are not confined to the use of either an ordinary inch or an ordinary meter rule in drawing-offices; and in some machine shops mechanics are, for good reasons, absolutely prohibited from laying a rule on a working drawing, and instructed to work solely from the figured dimensions.

In the same paper Mr. Sellers states from personal experience that the metric system is not well adaped to the machine shop, because it does not admit of advancing in a series of sixteenths of an inch, which he implies to be the most useful and salable series for certain articles. No doubt, as the "ordinary inch rule," divided into binary fractions of an inch, has been the ordinary linear measure used by purchasers of certain articles for numbers of years, they will prefer such a well-known series of sizes, and they are therefore found at the present time to be the most salable; but there is no reason to suppose that purchasers would continue to prefer a series of sizes advancing by the exact metrical equivalents of sixteenths of an inch if the fractional divisions of an inch were no longer in ordinary use as linear measures.

As regards building scales it seems desirable for simplicity that the scale of $\frac{1}{8}$ inch to 1 foot, or $\frac{1}{96}$, should be replaced by a scale of 1 centimeter to a meter, or $\frac{1}{100}$. and the scale of $\frac{1}{4}$ inch to a foot by a scale of 2 centimeters to a meter, or $\frac{1}{50}$; while such a building scale as $\frac{3}{16}$ inch to a foot can scarcely be held to convey a very clear idea of the relation between the size of the building as represented on paper and its actual size when built.

It has been generally conceded that the maximum advantage from the use of the metric system for ordinary purposes cannot be obtained unless the system is used in conjunction with a decimal system of coinage as measure of pecuniary value; and this applies nearly as much to its use for engineering purposes in the preparation of estimates and in recording the cost of work.

It has been further conceded that measures of pecuniary value differ from other measures, insomuch that for political reasons they can never be expected to become international. The reasons given by the late Sir John Herschel and by Sir George Airy, Hon. M. Inst. C. E., for preferring the decimal subdivision of the pound sterling to any other decimal arrangement of British measures of pecuniary value, appear unanswerable; and if so, the only practical question remaining is the simplest and least unpopular means

of bringing such an arrangement into use. It was long ago suggested that by simply enacting the depreciation in value by 4 per cent. of the existing bronze tokens, and thereby making twenty-five farthings legal change for a sixpenny piece, the British currency would be at once completely decimalized. New coins would not be necessary, although the coinage of a silver or nickel ten-farthing piece might be desirable, and the coinage of an increased number of farthings would be necessary in order to provide the odd farthing that would be required, in addition to any combination of pennies and halfpennies in giving change for a sixpenny piece. The existing pennies and half-pennies would serve as four-mil and two-mil pieces respectively in the terms of decimal currency, while popular sentiment would be appeased by the retention of the familiar coins. The alteration of the national measures of pecuniary value, however, could only be effected by compulsory legislation, whereas the general adoption of the metric system, which is already legal, though not compulsory, might be brought about to a great extent by its advantages being gradually impressed upon the common sense of the masses through the influence and legitimate pressure of the engineering profession. It has been found, moreover, by the experience of the introduction of the metric system on the Continent of Europe, that the difficulty of familiarizing the working classes with the system had been much overrated. In Austro-Hungary, for instance, the metric system was extensively used by engineers for their own convenience for some years previous to its general introduction. It had become the interest of contractors and workmen to make themselves acquainted with the terms of measurement in which the plans, specifications, and bills of quantities supplied to them by their employers were expressed, and thus through the medium of the working class a knowledge of the system had been extensively spread through the mass of the people before the system was made legally compulsory for the purposes of general commerce.

Very little progress has been made in England, or America, within the last twenty years towards simplifying measurement; and the general public in both countries appear perfectly indifferent on the subject, although the actual loss of money, through time wasted on computations and arithmetical studies which the use of the metric system would render unnecessary, must amount to an immense sum annually. Judging from continental experience, it would seem that the best prospect for a reform of measures in England lies in the initiative being taken by those professions most directly interested in the matter.

If British engineers were to commence to take measurements, and make plans and drawings in metric measures, they would probably confer a considerable balance of advantages upon themselves, and conduce to confer great eventual benefit upon their country. Whatever views may be held by individual members of the Institution, as to the advisability or otherwise of using metric measures in their own private practice, the author submits that the council might well consider the advisability of taking steps towards obtaining such amendments of the Standing Orders of the Houses of Lords and Commons, as would permit of the dimensions on parliamentary plans and sections, and measures in parliamentary estimates, being expressed either in metric measurement or in British measurement, at the option of the engineer. If parliamentary plans and estimates could be prepared in metric measurements, English engineers would have practical opportunities of testing the relative advantages of metric and British measures; and such a test would be more useful than any amount of writing and abstract discussion on the subject.

The decimalization of the pound sterling would upset existing arrangements so little that it is hard to see why it should not be enacted at once, and the decimalization of the coinage would conduce greatly to acquainting the masses with the advantages of decimal computation, but compulsory introduction of the metric system by legislation seems hardly desirable until after the coinage has been decimalized.

Discussion.

Sir Frederick Bramwell, President, proposed a vote of thanks to Mr. Hamilton-Smythe. All should thank him—those who agreed with his views for ad-

EXAMPLE.

To find the weight of water in a tank measuring:—

Ft. Ins. Ft. Ins. Ft. In.		M. M. M.
10 6 × 6 2 × 1 1,	or	3.20 × 1.88 × 0.33
10.5		3.2
6.17		1.88
735		256
105		256
630		32
64.785		6.016
1.083		0.33
194355		18048
518280		18048
647850		1.98528 tonne.
4) 70162.155		
4) 17540.538		
4) 4385.134		
7) 1096.283		
4) 156.611		
20) 39.152		
1.957 ton.		

vocating them, and those who did not, for the opportunity of stating their objections. He would beg the author to be good enough to explain the tabular comparison given in the example to the paper.

Mr. Hamilton-Smythe said he had put forward a mode of calculation, almost at random, with which engineers were familiar. He had taken British measures to find the weight of water in a tank 10 feet 6 inches by 6 feet 2 inches by 1 foot 1 inch, and also the nearest equivalent metric measures—3.20 meters by 1.88 meter by 0.33 meter. He had done this in British measures approximately. He had not converted the cubic feet into the exact number of lbs., but had assumed 1,000 oz. to the cubic foot, the more exact weight being 998.2 oz., or thereabouts. Taking 1,000 oz., produced an error, but shortened the process. To effect the calculation with British measures by the shortest way required ninety-four figures. It was done accurately in metric measures with thirty-five figures.

Mr. Henry Maudslay said the author had not done himself or his subject justice in putting that calculation in such a form, because British measure of feet and inches would have to be reduced first of all to the lowest denomination before calculations were begun, or else it would be necessary to have a book of decimals, or to work it out in some other form, before arriving at the results which had been obtained. The "5" in 10.5 meant "6" inches. There was thus a mental calculation as to the half of a foot put in decimal terms, and to put measurements of feet and inches and different proportions—eighths and sixteenths—in such a way into figures that the calculation could be commenced, would represent an extra number of figures. Thus, instead of the calculation being as was shown in the tabular comparison, there ought to have been many more figures. He understood that the terms under which the paper had been brought before the Institution had reference only to engineering purposes, and he remembered, as a result of the International Exhibition of 1851, that Sir Jos. Whitworth and others, including Mr. (now Sir William) Armstrong, prepared scales to metric measure, and to decimals of feet, carefully and elaborately made. At the same time they were rendered easy of comprehension. One remark in the paper with reference to money opened another question. He supposed it would make very little difference to the British commercially, or as engineers, whether Russia or Turkey ever adopted the metric system or not. He had traveled considerably in countries where the metric system was in operation, and he found that the ordinary people of those countries were enabled to conduct the affairs of every-day life with greater facility, rapidity, and accuracy, than English people could with their own peculiar system. After the first International Exhibition in 1851, so much was said and done about the metric system, that it was almost impossible to elicit a new idea upon the subject. He did not think the arguments that could now be brought forward would enable any one to form a more sound judgment on the matter than could be arrived at from what had been already exhibited by those who had carefully examined the metric system. If it came to be a question of adopting a form of metric system, he thought that a large majority of engineers would carry the day for it. They were dissatisfied

with the present English measure. The difficulties to be encountered, in changing the figures in calculations, represented possible fallacies and inaccuracies, and the greater the number of figures, the greater the number of calculations to be made, thereby increasing the chances of error. He would distinctly express himself in favor of the metric system, or some system which adopted an easy form for calculation, and that the different terms should be called by names that might be readily learned. The thirty-two millions of people in this country, and the Americans, required that names should be given to the different measures, weights and numbers, and values of money, which they could easily understand and remember, rather than terms and names which would be difficult to learn, and, if learned, would soon be forgotten.

Mr. G. Wells Owen observed that it was one of the drawbacks incident to the lot of suffering humanity, a heritage from the days of the Tower of Babel, that people of different nationalities were hampered in their intercourse with each other by differences of language, and although perhaps not coming so directly from that historical event, by differences of measures, moneys and weights. Many of these difficulties and differences had been considerably modified of late years, and perhaps he was not over sanguine in thinking that they would be still more so. The paper, which tended to a discussion upon one mode of modifying some of those difficulties, was one which he thought the Institution might very well discuss, because it was more than thirty years since a discussion on a similar subject took place.

On the 28th of February, 1854, a paper by Mr. James Yates was read before the Institution "On the French System of Measures, Weights and Coins." The discussion upon that paper related more especially to the matter of the decimal coinage, and, although the consensus of opinion seemed to be in favor of a decimal system, the discussion turned to a great extent upon what was the best standard of value as the basis for that decimal coinage. The part that more particularly interested engineers was that concerning weights and measures, and therefore he would allude no further to the coinage, beyond saying that he hoped that if England ever did adopt a decimal coinage she would disregard all questions as to what the present coins were, and effect a radical change by assimilating it to that of the other countries which were members of the Latin Monetary Union. Those who were accustomed to travel where that league prevailed knew how easy it was to pass backward and forward from one country to another without changing their money, and they also knew what difficulties arose the moment they passed from one of those countries into some neighboring State unconnected with the Union. No doubt there would be great inconveniences in adopting an entirely different standard of value, but, although that might be so, it was only necessary to consider what had been done, for instance, in Germany, where, at the time of the formation of the German Empire, the numerous coinages that prevailed in the various States were abolished. Former travelers in Germany would probably remember the difficulties they experienced there. He hoped that if England did adopt a new coinage it would be assimilated to the French franc. The more immediate question before the Institution, however, was that of weights and measures, and perhaps it would be interesting in connection with that if he mentioned the dates at which different countries had adopted the metric system of weights and measures, in order to show how it had spread in recent years. In France, metric weights and measures were enacted in 1795, but were not made compulsory till 1840. The metric system, with Dutch names, was introduced into Holland in 1820, but from the 1st of January, 1870, the metric names came in, with facultative use of the old Dutch names. The next country which took the matter up was a very small one at a considerable distance from France—the kingdom of Greece—where metric weights and measures, but with Greek names, were introduced by an ordinance dated the 26th of October, 1836. In 1852, Portugal decreed that the metric basis should be used, and a period of ten years was fixed for its introduction and adoption. The metric measures of length came into use in January, 1860, weights in July, 1861, surface measures in July,

1862, and measures of capacity in January, 1863. In Spain the metric system was adopted on the 1st of January, 1859, and became compulsory on the 1st of July, 1868. It was also the legal system of all the Spanish Colonies. In Italy the system was adopted in 1859, when most of the Italian States were united to form the Kingdom of Italy. In the German Empire it came into use in 1872, and in the Austro-Hungarian Empire it was made permissive on the 1st of January, 1873, and compulsory on the 1st of January, 1876. In Switzerland it became the legal system on the 1st of January, 1873; it was adopted in Sweden by an Act of the 19th of May, 1875; and in Norway by an Act of the 22d of May, 1875, to come into force at the expiration of three years after the passing of the law. In the United States it became permissive by an act of the 28th of July, 1866. He did not know the date when the system was introduced into Belgium, but it was in use there, and also in the Ionian Islands, Algeria, the United States of Columbia, Peru, Chili, the Argentine Republic, and Uruguay; and it was also permissive in British North America under an Act of 1873. Of course, if the metric system of measures were adopted in England the old systems must be discarded. As the author had stated, the minds of engineers in America and England were saturated, especially with regard to ironwork, with dimensions expressed in English inches and fractions of inches. That would be a great trouble to English engineers at first, but he thought they must not look so much to what would be a trouble to themselves as to what would be a benefit to the world at large, and no doubt in a short time they would get so familiarized with the new measures, as to use them as readily and freely as they now did the old ones. When the German Empire adopted the metric system of measures, sixteen States fused into that Empire had no less than sixteen different lengths for the foot, varying from 9.84 inches to 12.35 inches. Their other measures varied in a similar manner, but the metric system had been adopted throughout Germany without entailing much inconvenience. All specifications in Germany and Austro-Hungary were now prepared on the metric system, and therefore he thought there would not be much difficulty in getting used to it in England, where he hoped it would one day or other, perhaps by degrees, be adopted.

Mr. Percival Fowler said he had been abroad for some years, and had known many engineers well acquainted with the two systems, but he had never met one who, after a month's practice, did not prefer the decimal metric system. He considered that, with the increasing international communication of the present day, it was of the highest importance that weights, measures, &c., should be uniform in all countries. Scientists had long since recognized the convenience of an international technical language, and in biology, botany, and many other sciences, Latin or Greek names had been chosen which were intelligible to all the civilized world; in electricity the new nomenclature had been adopted by an International Commission. Considering that a uniform decimal system had been adopted in nearly all civilized countries, it was necessary that England should seriously consider the question. English machinery went all over the world, and had to be mounted in other countries by workmen who were not acquainted with eighths, sixteenths, full thirty-seconds, and other like measures. In England the decimal system had been used in many ways. Barometers were graduated in inches and tenths of an inch, level-staffs to feet and tenths of a foot; and in spite of ourselves the decimal system was being introduced into this country. The difficulty of learning the metric system had been immensely overrated. He could speak from practical experience that in Spain, in a week, the workmen became thoroughly conversant with meters, centimeters and millimeters, and he was certain that the difficulty of changing the English system of weights and measures to the decimal system had been greatly overestimated. He had heard it said that English weights and measures were more convenient, but he did not see how that could be. He believed that one Ordnance scale was $\frac{1}{25344}$, and besides the poles, yards, feet and inches, the land measure was 7.92 inches in a link, 100 links in a chain, and 80 chains to a mile. Silver and lead ores were reckoned by so many ounces troy to 1 ton avoirdupois,

so that in order to arrive at the proportion of silver in lead it was necessary to take two separate measures, and calculate them. With regard to the question of calculations for water, since a liter of water weighed a kilogram, the calculation was of the simplest sort. In England the measurement was effected in cubic feet or gallons, and he always had to refer to a book of tables, for he could never remember the equivalent figures. A most inconvenient scale was three-sixteenths of an inch to a foot, and a calculation had to be made as to what proportion that was. He did not understand why it was that Englishmen had not yet adopted the decimal system. As for British pride, the French proverb might be true "Les Français inventent, mais les Anglais perfectionnent," but as regarded the decimal system, Englishmen had neither invented nor perfected it, and he thought they ought to adopt what had been found practicable in nearly all civilized countries. It would be of the greatest convenience, as he did not believe that a single member of the Institution had not constantly to refer to a table to convert English weights and measures to their metric equivalents.

Mr. Rodolph De Salis desired to mention a few facts which he thought told in favor of the present systems. It could not be denied that starting with a clean slate a universal system of weights, measures and coinage would be desirable; but it might be fairly questioned whether a decimal system would be the best, and if a decimal system were adopted, whether it should be based on the meter; and as different systems were in existence, it might also be questioned whether the advantages of a change would counterbalance the disadvantages. In considering such a change, the advantages should be gauged not by the preferences of scientific persons, but by the convenience of the bulk of the people; and he certainly thought, for general purposes, a system admitting of binary and ternary subdivisions was more convenient than a system based on decimal subdivisions. All knew the convenience of dividing anything into halves, quarters, or thirds, or their subdivisions. It was not so clear how conveniently to divide things into fifths, tenths, and hundredths in the ordinary practical affairs of life. Of course, for all purposes of calculation a decimal system had immense advantages; but, practically, that system was in use in all calculations. Everything was decimalized, or was worked by logarithms. Even actuarial computations were always made in sovereigns and decimals of sovereigns; indeed, under the present system, computors had the advantage of using decimals when they liked, and fractions when they were most convenient. The present measures, he thought, were very convenient. A yard, for example, was the $\frac{1}{1760}$th of a mile, and the $\frac{1}{4840}$th of an acre. Then there was the ternary division of the yard, the foot. He had always considered the foot to be a particularly handy measure for hydraulic and other calculations. It correlated very well with the gallon and the pound, 25 quarter gallons or 50 pints making a cubic foot—near enough for all practical purposes—and 10 lbs. of water a gallon.

With regard to the practical uses of the foot, the only instance he would give was that of building, and the cognate trade of brickmaking. A brick was $\frac{3}{4}$ by $\frac{1}{4}$ of a foot, and its breadth was its length divided binarily. Those were convenient fractions, and from them quantities could be estimated. The thickness of a wall was one brick, a brick and a-half or two bricks, and it contained so many courses of brick-work, all in terms of the foot. If the metric system were adopted instead, it would be found that a brick was equal to 0.2286 by 0.0762 by 0.1143 of a meter, which he did not think would be very convenient. But if the brick were to be changed to suit the new system, the inconvenience would be very great; all contracts for the sale of brick earth would have to be revised, and brick-making machinery would become antiquated. Again, it would be a great disadvantage that new work could not be conveniently joined to old work. The old bricks could not be used with the bricks of the new standard. How could old work be bonded on to new? It would be almost sure to result in failure. With regard to the theoretical basis, he agreed with the author that the accuracy or otherwise of its relation to the meridian was not material; but according to Sir John Herschel, the inch was as scientific a basis as the meter. Again, the yard was an extremely convenient step,

and on that account a good basis. It was a better step than the meter. It was a natural step for a tall man, and a very long step for a short man; but adding 3.37 inches for each step would probably put it beyond the compass of a short man, and he could state personally that it was a longish step for a tall man. That was a practical consideration which ought not to be disregarded. What were the advantages to be gained by the adoption of a change which would certainly affect nearly every transaction in life? A map had been exhibited showing the size of the countries using metric measures as compared with the British Islands, and on the theory that the smaller must yield to the greater, it was urged that the metric system should be adopted in England. That argument, if worth anything, operated entirely the other way. The British sway extended over one-fifth of the habitable globe, while the area of metric countries was only about one-thirty-fifth. The British Empire included a population of 310 millions, the United States 50 millions, and the metric countries about 200 millions. The British imports and exports for 1883 to the British possessions amounted to £188,000,000, to the United States to £135,000,000, to various other places, to £142,000,000, making a total of £465,000,000; the amount to the metric countries was £266,000,000. Therefore, as far as size went, the balance was entirely on the side of the present measures. The coinage would certainly be more conveniently affected by a change than weights and measures; but he thought the commercial classes were not in favor of change. In 1849 the florin, the decimal subdivision of a pound, was introduced, and the coinage of half-crowns was stopped. In 1874, a circular was addressed to bankers and others interested, asking whether half-crowns should be discontinued or florins, or whether both should be coined. In official phraseology the answer to the circular showed an "overwhelming preponderance" in favor of the continuance of both coins, while out of six letters to the *Times* on the subject, not one advocated the dropping of the half-crown. He did not regard with the author's equanimity the depreciation in the value of their library which would result from a change in notation. No doubt a certain number of text-books would be translated, but the bulk of the works, all the Parliamentary papers, books of reference, and, he should imagine, nineteen-twentieths of the library would become antiquated and practically unusable; whereas, under the present system, it was found that any good book published in French or German was promptly translated into English with English measures, and took its place beside English literature. It had been urged that the metric or decimal system was much easier to learn. No doubt it was, but surely that was not altogether an advantage. He would put in the form of a proportion sum: As Greek is to French so is the English complicated system to the metric system in point of mental training. He dissented altogether from the idea that the professional classes should be the first to advocate a change of that sort. Such a change ought to come from the bottom and not from the top, and ought to be the product of necessity. That the bulk of the people did not feel the need of the change was, he thought, shown by the fact stated by the author, that the general public in both countries, England and the United States of America, appeared indifferent on the subject. As professed mathematicians, or at least arithmeticians, he thought engineers ought to be ready to adapt themselves to any system that appeared to commend itself to their clients, and not expect them to give up convenient binary divisions in order to facilitate professional calculations. He classed a universal system of measures, weights, and money, in the same category with a universal language. Both were theoretically very advisable, but he thought they were not practicable. No doubt they would be much less anomalous than the present system, but he doubted whether they would be so useful.

Mr. R. C. Rapier said that, ten years ago he should have made much the same speech as that which had just been delivered. In 1868, when the great discussion on the subject took place, he was strongly in favor of retaining things as they were; he then thought there was no measure of length so convenient as the English foot, and no unit of capacity so useful as the English gallon. But circumstances altered cases. During the last fifteen years he had been obliged to have recourse to

meters, kilometers, and such like measures, and he found that they were not so objectionable after all; in fact, he had compiled some useful and handy rules for himself on the subject. Mr. De Salis had spoken of the importance of conserving the English system. Mr. Rapier ventured to say that the English weights and measures were not a system, they were an accident; they were never created upon any systematic principle or plan. In one part of the country 14 lbs. were a stone, and in another 16 lbs., and similar diversities were well known. Whether the standard of length was a pendulum vibrating seconds, or the common yard or foot, he did not care; but he maintained that a decimal system was the right one, and he inclined strongly to the metric system, because it was already established over the rest of Europe. The metric system had the advantage of an intelligible and definite connection between units of length, units of capacity, and units of weight. The meter was referable to the vibration of a pendulum; so would any other unit be, but it so happened that the cubic meter coincided very nearly with the English ton, the French tonne. He thought encouragement might be taken from the fact of that connection. With regard to money, the present system on the plan shadowed forth by the author, and previously mentioned by Sir John Herschel, admitted readily of an adaptation to a decimal sytem, and it had a tolerably near affinity to the system on the Continent, bearing in mind that twenty English shillings were as good as twenty-five francs. The convenience of the relationship of the measures on the metric system was very great. For instance, a cubic meter of water was a ton. The number of cubic meters of any other material multiplied by its specific gravity gave the number of tons. Again, to take a familiar illustration, that of a railway to be laid with rails 35 kilograms per meter, there were 70 metric tons in a kilometer; it was easy to reckon how many English tons there would be according to the English system, but in the other case it was obvious. If the members would regard the matter practically, for about six months, they would begin to think in the new measures, and then the whole would become perfectly easy. To facilitate thinking in metric measures, he had adopted the following ready rules: To convert meters into feet, divide by 3, and move the decimal point one place to the right. To convert feet into meters, multiply by 3, and move the decimal point one place to the left. To convert millimeters into inches, multiply by 4, and move the decimal point one place to the right. To convert inches into millimeters, divide by 4 and move the decimal point one place to the left. To change hectares into acres, divide by 4, and move the decimal point one place to the right. To convert square meters into square yards, add one-fifth. To change cubic meters into cubic yards, add one-third. To convert kilometers into miles, divide by 2, and add one-fourth. All the above examples were worked by the use of the elementary numbers, 3, 4, and 5, and gave results correct within 1½ per cent., and the rules were therefore sufficiently near for the purpose of mental calculation, to facilitate a familiarity with the metric equivalents.

Mr. E. W. Young, to show the comparison between decimals and duodecimals, exhibited a calculation in compound addition by both methods, in which there was one column more in the decimal sum than in the duodecimal. Again, to illustrate the ordinary transactions in commerce, millions of which took place every day in London alone, suppose a shopman wished to ascertain the cost of 17 yards at 5¾*d.*, he did not reduce 5¾*d.* to farthings, multiply by 17, and reduce back to pence and shillings, but he applied a process of mental arithmetic, thus: he said, Why, that is 17 sixpences less 17 farthings, or 8*s.* 6*d.* less 4¼*d.*, that is 8*s.* 1¾*d.* It was done in an instant. Now if the decimal system were in force, and there were 25 farthings to the sixpence, the shopman would have to multiply yards by farthings, which would be far less convenient than the present system. Indeed, for every little purchase made, the shopman would have to pull out pencil and paper to make a calculation. The duodecimal system had the advantage over the decimal system in admitting the use of fractions to a much greater extent, so that sums could be more easily done mentally. It was not the convenience of the scientific or professional man in his office that ought to be considered, but the general convenience of the multitude in the millions of transactions going on throughout the

country. The introduction of the meter would necessarily mean the introduction of the decimal system generally, and he thought that an alteration to the decimal system would occasion very great inconvenience, and it was exceedingly doubtful whether there would be any economy of labor in it. For the purpose of contrasting the meter with the present British standards of measurement, the following example afforded a fair comparison between the two systems. The sum was: required the contents in cubic yards of a block of masonry 88 feet by 3 feet 9 inches by 2 feet 3 inches. The usual way of doing this was by fractions, thus, $\frac{88^y}{3} \times 1\frac{1}{4}^y \times \frac{3}{4}^y$. The threes canceled one another and the result was at once $\frac{110}{4}$ cubic yards $=27.5$ cubic yards. Using the decimal system, it would be necessary first to multiply 88 by 3.75, and the product by 2.25, involving a large number of figures. Even then the result would only be in cubic feet, and though it would be unfair to the decimal system to debit it with the figures that would be needed to reduce cubic feet to cubic yards, the sum so done would be a better comparison of the two systems than the example given by the author, which he thought ingeniously unfair. With regard to the suggestion that twenty-five farthings would equal sixpence; that, he thought, would be a great disaster, because 24 could be divided by 2, 4, 8, 12, 6, and 3, and those numbers were again divisible by others. Altogether there were eighteen divisions and subdivisions, but 25 could only be divided once by 5. He did not know how it was that the present duodecimal system was first hit upon. Evidently those who made 12 inches to the foot had some idea of utility. The same system was constantly used. There were twenty-four hours to the day; the arc was divided into 360°, a multiple of twelve. He had heard, however, that an agitation was going on for a division of the circle into decimals, which he should very much regret. If that were accomplished he supposed there would be a demand for twenty hours to the day instead of twenty-four. There was a disadvantage in the decimal system in consequence of the similarity of some of the terms, such as decimeter, and decameter, deciliter, and decaliter, deciare and decare, decistere and decastere. He believed that working men would be confused with words so much alike, and a great many mistakes would be made through bad writing. If a man forgot to dot the "i" that might make all the difference between a decimeter and a decameter. He thought it would be a good thing to get rid of the 66-foot chain. There were many anomalies in the present system, and a great many things that it might be desirable to change; but it was a serious matter to propose to change the whole of the system adopted in England, in the Colonies, and in the United States. He considered it a mistake to lay so much stress upon foreign commerce. No doubt it was large, but it was small in comparison with the internal commerce of the country. Foreign commerce, too, was wholesale, while internal commerce was chiefly retail; so that there might be ten thousand or one hundred thousand transactions taking place in this country with the present system of coinage and measures for one negotiation in meters. When Englishmen went abroad they took French money with them, and knew how to deal with it, and on returning home they had no difficulty in reverting to their own system. It might be necessary for German duchies about the size of an English county, placed between two large States like France and Germany, to adopt the metric system, but not in a country like England.

Mr. C. E. Cowper wished to call attention to three points. With reference to the calculation submitted by the author, some of the advocates of the British system had computed it in different ways, some by duodecimals and some by vulgar fractions. The author had brought it forward as a sample calculation to show the superior advantage of the metric system, but he did not think he had done his best; because, though the tank might have been marked on the drawing 10 feet 6 inches, its dimensions would be, perhaps, when made, $\frac{3}{8}$ inch more or less than that length. What was a tank to be measured for? If he had to measure the tank to find the weight of the water, in order to ascertain what sized injector to put in, or what size of feed-pump to use for a boiler, he would be content to take it approximately—say 2 tons, which would be near enough. Suppose, on the other hand, great accuracy

were needed—if, for example, the tank were used for gauging water in a pumping-engine trial, measuring the feed-water into the boiler (and the contract price might depend on the economy), the case would be very different. In that case the dimensions of the tank might be 10 feet 6$\frac{3}{8}$ inches by 6 feet 2$\frac{5}{8}$ inches, and the depth of the water, perhaps, 1 foot $\frac{3}{16}$ inch. He thought the duodecimal system, or the vulgar fraction system, would not then work out so neatly; but by the metric system it did not matter whether the measurement was 0.33, 0.34 or 0.35; the calculation was not any longer. Again, the author might, he thought, have made more of the smallness of the unit. Regarding the question as a matter of abstract principle, suppose it were necessary to make an arrangement for a new world, where no difficulties would arise on account of established customs or international intercourse, a small unit of measurement, convenient to work to, and one that could be easily divided on boxwood or steel, would certainly be looked for. Such a unit existed in the millimeter, but not in the inch, or any decimal division of the inch or foot; the tenth of an inch was too large, and the one-hundredth too small. The millimeter was as small as required for any practical purposes, and it could be subdivided microscopically to any extent. Reference had been made to the use of logarithms, but he wished now to direct attention to the mechanical use of logarithms, to which, he contended, the metric system lent itself. He might raise a smile if he referred to the slide rule, because people generally thought of the dirty, black, greasy thing which the fitter carried in his pocket and did not know how to use; the slide rule there was in the wrong place; the fitter did not want it, and the proper place for it was in the office. It was little used in England, and one of the reasons for that was the complicated English system, or combination of systems, of weights and measures. In France, one of the improved forms of the slide rule was found, he had been informed, on almost every engineer's desk; in electrical engineering it was very largely employed. Two of the largest electrical establishments in England used one or other form of the slide rule, or the logarithm slide, to a great extent. He therefore claimed that the metric system, being a decimal system, which allowed every calculation to be checked readily by the slide rule, possessed a substantial advantage. Many persons thought this instrument a trivial affair, but only a practical acquaintance with it would enable them to form a sound judgment as to its merits.

Mr. H. J. Chaney thought it was desirable, in order to facilitate discussion, to separate the question of weights and measures from that of money. The money aspect was no doubt very interesting, but experience had shown that the question of the introduction of a new system of weights and measures could be best considered by itself. The subject under discussion was not that of decimalizing weights and measures, or introducing a decimal system of money, but the desirability of introducing the metric system, which was not only decimal, but binary. He supposed that few persons who had much acquaintance with the present system of weights and measures would admit it to be the best that could be provided. The metric system, *per se*, was by far the most convenient; but then the inconvenience of making a change which would so largely affect the daily transactions of life had to be considered. The discussion, he thought should deal with the difficulties in the way of such a change. The question of the comparative advantages of the metric and British Imperial systems had been well thrashed out. Reference had been made to the Standing Orders requiring that all scales should be according to Imperial measure, as 4 inches to the mile, and he would suggest for consideration the nearest form of metric equivalent. Besides 4 inches to the mile, the Standing Orders might also permit a scale of 65 millimeters to the kilometer. Buildings, also, were to be drawn to an enlarged scale, of not less than $\frac{1}{4}$ inch to every 100 feet; for that scale there might also be permitted a scale of 7 millimeters to every 30 meters. Sections might be given to a scale of 1 millimeter in every 1,200 millimeters, or in every 12 decimeters, as well as to a scale of 1 inch to every 100 feet. Quantities might be allowed to be given in meters and parts of a meter, as well as

in yards, feet, decimal parts of a foot, and inches. Distances might be permitted to be expressed in kilometers and meters, as well as in miles, furlongs, chains, poles, yards and feet.

Mr. W. Walton Williams said he did not think it was necessary to choose immediately between the two systems. Engineers before tying themselves down to the metric system, ought to consider whether it could be in any way improved. According to French engineers the metric system was absolute perfection, and Englishmen were a set of idiots for not adopting it. He had been obliged to use the metric system for many years abroad, and he desired to point out two or three disadvantages connected with it, in the hope that they might be remedied. No man could step a long distance in meters. He had seen French engineers try to do so; they could step 100 meters, but if the distance was 400 or 500 meters, they would reckon every step to be 90 centimeters, equivalent to the English yard. The author had stated that the meter was the right length for a measure, but Mr. Williams contended that no man could measure with it easily. The eye subtended an angle of 60°, so that a man's arms ought to be a meter long to be able to see both ends of the meter rule. The author had stated that a workman could measure with a meter rule in two-thirds of the time required with a two-foot rule. That he denied from his experience of foreign workmen, who, with a meter measure, were generally longer and less accurate measuring than Englishmen with a two-foot rule; besides which the foreigner generally called in another workman to put a mark at the end. The French themselves did not admit that the meter was absolutely the best unit of length, for any one going into a draper's shop would find that a half-meter and not a meter measure was generally used; the demi-kilo was frequently used for weight, and the customer might be heard asking for "Le quart d'un demi-kilo." With reference to leveling, it did not make much difference theoretically whether engineers divided a foot or a meter by the decimal system, except that they could frequently see the foot mark on a staff, when they could not see the meter mark. He had known instances in which a mistake had been made of a whole meter, because a man would not, or did not, lift up the staff the required distance. If a French staff with a disk were used it would be different, but where the staffman moved the disk up and down, it had to be brought each time to be verified, so that the time was doubled. With regard to drawings, if they were made to a scale of $\frac{1}{10000}$, $\frac{1}{5000}$, or even $\frac{1}{1000}$, it did not matter whether feet or meters were used. In drawing to a larger scale, 1 inch to the foot, the English $\frac{1}{12}$ corresponded to the French $\frac{1}{10}$; but to vary it the Englishman could go from 1 inch to $1\frac{1}{2}$ inch, or to $\frac{3}{4}$ of an inch, while the Frenchman would have to go up to $\frac{1}{5}$ or down to $\frac{1}{20}$. It had been said that $\frac{3}{16}$ of an inch was an absurd scale. Possibly, but it was $\frac{1}{64}$ of a foot, and 64 was 2 multiplied by itself five times. To put $\frac{1}{64}$ of a meter into decimals, six places of decimals would be required. He did not say that engineers ought not to adopt the metric system, but they should not be absolutely tied down to it as it at present existed. It was their duty to improve it, if possible, according to the saying already quoted: Les Francais inventent, mais les Anglais perfectionnent." He wished to address a few words to those who were in the habit of preparing drawings for foreign works. He had had many such drawings sent out from England, and they had been usually drawn to an English scale, and the dimensions inserted in meters. When a Spanish stonemason or an Italian carpenter saw such drawing he was puzzled, and fresh ones had to be prepared for his use. To those who were in the habit of using the 1-inch or $1\frac{1}{2}$-inch scale, $\frac{1}{10}$ was not convenient, but he thought that, considering the immense number of drawings going abroad for foreign work, a little sacrifice ought to be made in order to adapt them to the metric system, even if that system were not generally adopted in this country.

Mr. W. H. Thelwall remarked that he had used both the metric and the English systems for nearly twenty years, and although he was not frightened at the metric system, he was decidedly of opinion that the English was much more convenient in all matters of civil engineering. The author had not given any statement of the relative times taken to do various works. He had said that a

French workman could measure planks in two-thirds of the time occupied by an English workman, but if that statement was to be of any value it ought to be supported by actual experiment. He fully agreed with what had been urged by Mr. Walton Williams that the meter was inconveniently long for accurate measurement. The author's statement with regard to leveling was much too vague to be of any practical value, since it contained no account of the time saved. According to his own experience, French engineers were not satisfied with working to centimeters, but even in trial sections over rough ground worked to millimeters. In the case of sections of railways, the ground and formation levels and the heights and depths of cuttings and banks had been all worked out to millimeters, requiring three places of decimals instead of two, which was a serious matter. Referring all measures to the meter involved the use of a much larger number of figures than the English system. He had copied some dimensions, that he had recently met with in a French publication, of a tramway locomotive. The diameter of the cylinder was 0.220 m., in English measure it would be 9 inches; the stroke of the piston was 0.350 m., in English it would be 14 inches; the weight, empty, was 11,000 kilograms, in English, 11 tons; the weight, running, 13,000 kilograms, in English 13 tons; the heating surface, 18.90 square meters, in English, 203 square feet. Thus, there were twenty-two figures in French, and only ten in English, measure. Take another instance, that of the strains and weight of a wrought-iron pillar, having a load of 25 square meters of flooring weighing 6,600 kilograms per square meter. $25 \times 6{,}600 = 165{,}000$ kilograms. The area of the pillar was—

Web........	450×12	$= 5{,}400$	sq. millimeters.
4 plates......	500×12	$= 24{,}000$	" "
4 angles.....	$\dfrac{100 \times 100}{12}$	$= 9{,}000$	" "
4 "	$\dfrac{80 \times 80}{10}$	$= 6{,}000$	" "
Total....		44,000	

Load per sq. millimeter $\dfrac{165{,}000}{44{,}000} = 3.7$ kilograms.

In English measures the above would be, load of 270 square feet of flooring weighing 0.6 ton per square foot $= 270 \times 0.6 = 162$ tons. The area of the pillar would be—

	Inches.		Inches.		Inch.		Sq. Inches.
Web.........	18	×	$\frac{1}{2}$			=	9
4 plates......	19	×	$\frac{1}{2}$			=	38
4 angles......	4	×	4	×	$\frac{1}{2}$	=	15
4 "	3	×	3	×	$\frac{3}{8}$	=	8
Total....							70

Load per square inch $= \frac{162}{70}$ 2.32 tons.

Such examples might be multiplied indefinitely. They had not been made up for the purpose of showing comparisons unfavorable to the metric system, but were fair samples taken at random from actual French practice, and converted into the corresponding English figures. Again, such things as diameters of pipes were measured by 200 millimeters, 150 millimeters, and so on, the English equivalent for which would be 8 inches and 6 inches. It frequently happened that, in the French system, three figures were used instead of one; and when in taking out quantities the figures had to be multiplied, the difference was very serious. If the author really considered that there was a saving of time by the metric system, he ought to have given an example, such as the estimate of work, or the calculations and quantities in a large iron bridge properly worked out by both the metric and the English systems, and not the long sum in the Appendix, which could be computed with less than half the number of figures given. He was convinced that the more acquaintance English engineers had with the practical working of the metric system, the less disposed they would be to exchange their own for it.

Mr. W. Airy thought that the author had somewhat mixed up two very different matters, the metrical unit and the decimal system, which ought to be kept distinct. It appeared to him that the best unit was that which the experience of people had decided to be the best for the greatest number of measurements. For the great bulk of measurements—those relating to building, construction, and manufacturers generally—the meter could not be used without subdivision, and it was therefore, in his opinion, wholly unfit for the unit for such measurements. That was proved by the fact that, in England, where the people had

the choice both of the yard and the foot—and the yard for that purpose might be considered as comparable with the meter—for the great bulk of their measurements they used the foot, and not the yard. He thought it would hardly be denied that the foot was the real unit of the English people. The meter was much too long a unit for the ordinary affairs of life; and even in France, the headquarters of the meter, there were cases in which the French still used the foot. In Sweden and Norway also, as well as in other countries professing to use the metric system, the foot was employed for a variety of purposes. He thought there could be no doubt that the decimal subdivisions of the unit, whatever it might be, afforded great facilities for calculation; and therefore for all those measures, or classes of measures, which involved frequent or complex calculations, great advantages would be derived from the adoption of that method. It would, for example, be a great advantage to subdivide the foot into 10 inches, and the inches into tenths, and he thought that such a change would be acceptable to the country at large; but for all measures referring to articles of consumption, he believed the binary system, which was much more simple to uneducated people than the decimal system, had a firm hold upon the people, which it would be impossible to shake. The author would have done well to have made some mention of the case of Russia, which, long after the metric system was fairly established in central Europe, had occasion to set her standards in order, and after due consideration had adopted the English foot as the unit, altering the national measures so as to form exact multiples of it.

Mr. E. B. Hanson said he thought that the sum, given in the duodecimal system, ought to have been worked by duodecimals and not by decimals. In answer to Mr. Airy, it would be sufficient to say that the standard yard was divided into feet, of which 1 foot was divided into 12 inches, and so the foot formed part of the standard. The author had found fault with engineers for measuring with a two-foot rule, and afterwards asked them to measure with a 20 meter chain, which was hardly consistent. He then spoke of "five-sixteenths full," or "bare three-eighths," as if they were the same thing. Mr. Hanson had always understood that if there was a hole of exactly three-eighths diameter, the cylinder which would fit into it would be a "bare three-eighths," and *vice versa*. He did not think that any brickwork or any engineering work was measured in rods now; he believed it was always measured in cubic yards. A scale $\frac{3}{16}$ inch to a foot was, he believed, one of the best. It came near to five feet to the inch, and it had the advantage that it was a fractional part of $1\frac{1}{2}$ inch to the foot, which was so useful a scale to the carpenter, inasmuch as $\frac{1}{8}$ inch on his rule represented an inch. Ordinary working plans for railways and so on were plotted with the horizontal scale 2 chains, and the vertical scale 20 feet to an inch respectively. There was not the slightest difficulty in passing from one to the other. The same scale did for both, whereas with the metric system two scales would be necessary to measure by, and there would be a constant changing. If parliamentary deposited plans were plotted in meters, farmers and others would be wanting to know how many square meters there were in an acre. They did not reckon by meters, and it would take years to make them do so. When he was in Paris during the Exhibition of 1878, he went with a brother engineer to visit some shops where several young artisans were being taught under the French Government, and he was much astonished to find that they were using the two-foot rule, and also using English names for the various tools. On asking the reason, he was told that it was found to be so much more convenient. In some shops in England where a large number of Germans were employed, machines had been constructed partly in millimeters and partly in inches. Pulleys and things of that sort were generally measured in inches, and the smaller parts in millimeters. Considering that the meter was introduced into France forty-five years before it was properly legalized, before railways were made, and before engineering works assumed their present importance, the time it would take to introduce it into England now could hardly be estimated. There were certain standards in England which might be considered as hard and fast. The railway gauge was

one of them, and it could never be altered. The measurement of 4 feet $8\frac{1}{2}$ inches looked well enough as it was, but in the new system it would be 1.435 m. There was also a standard measurement of bricks. If the metric system were introduced, he supposed that bricks would be made $\frac{1}{4}$ meter long. A trial had been made of bricks 10 inches by 5 inches by 3 inches, but they had not succeeded, and he believed the use of them had ceased.

Mr. L. F. Vernon Harcourt said there could be no doubt that scientifically the metric system, in which the weights and measures were connected, and arranged according to a definite plan, was better than the English; but there were some disadvantages in it which ought not to be overlooked. The meter, being a larger unit than a foot, required more subdivision to measure small dimensions, and was less well adapted for leveling than feet and decimals of a foot. The constant use of decimals, moreover, in the metric system, led to the abandonment of fractions and proportions, which had a distinct value of their own in certain cases. Errors, for instance, were more liable to occur when gradients were expressed in decimals, such as 0.00125 and 0.000333, instead of as 1 in 800 and 1 in 3,000; and $\frac{3}{16}$ and $\frac{1}{60000}$, were simpler modes of expressing these proportions than 0.1875 and 0.000016. In a string of noughts after the decimal point, one of them was very liable to be omitted, or the decimal point itself might be readily misplaced, causing in either case a tenfold error. Also fractions were expressed more shortly in words, and were consequently more easily remembered and repeated than decimals, where each digit had to be separately named; and the use of commas for marking off the thousands, which greatly facilitated the reading of figures, could not be employed for decimals. The author had stated that scales for drawings were much better represented by the French than by the English system, and Mr. Vernon-Harcourt had therefore taken the trouble of looking out a few scales given in some French engineering works, of which the following were instances of those occasionally adopted; namely, 0.06667 m. per kilometer, 0.00143 m. per meter, 0.004165 m. per 100 meters, 0.0104 m. per meter, and 0.0278 m. per kilometer. Those figures, he thought, would be sufficient to show that the French scales were not always so simple as some persons imagined. No doubt a reform might be introduced, and it would be a great advantage in all engineering literature to have scales that would be comparable. He had himself tried to introduce such scales in the form of definite simple fractions of the natural size. In the illustrations for a book very shortly to be published, he had managed to make two hundred and twenty-five figures to nine scales, all of these scales being multiples, or parts of one another, ranging between one thirty-thousandth and one hundredth of the full size; so that it was possible to compare the various plans together. As to the statement with reference to calculation, he felt sure that any one who had worked with Bidder's tables for getting out earthwork could do so more quickly that way than by any other method. The author had stated that in the case of English measurements, bushels, and cubic yards were used. When inspecting works in France he had frequently asked for the proportions of cement or lime employed in concrete or mortar, and he was always told that there were so many kilos to the cubic meter; so that in that respect there was not much advantage in the French system over the English. If the new system were adopted in parliamentary plans, especially if some plans were figured in meters and others in the ordinary English dimensions, both witnesses and the legal profession would be much puzzled. It was questionable whether the advantage in consulting foreign books, that would accrue from the adoption of the metric system, would for a long time counterbalance the loss resulting from the different notation of the whole existing British and American engineering literature. Much of this literature, including the eighty volumes of Minutes of Proceedings of the Institution, could not be reproduced with metric measures; and the necessary alterations in the Ordnance survey and the Admiralty charts would involve a large expenditure. If the metric system were introduced it would simplify calculations; but, in making the change, it would be necessary to take into account

all the English-speaking communities, and a conference would be required in order to arrive at a consensus of opinion. A common system of weights and measures would be of very great value. The discussion, however, had indicated that much could be urged on both sides of the question; and unless the balance of advantage proved greatly in favor of the change, the present generation could hardly be forced to incur great inconvenience for the possible benefit of posterity. The change, if made, should be gradual, with the concurrent use for some time of both systems, and aided by elaborate conversion tables. Moreover, in such a matter, Great Britain could not be regarded merely as a European power, and the metric system would have to be adopted also simultaneously by the Colonies and the United States if any real benefit was to be gained.

Mr. J. W. Barry agreed with Mr. Airy that the meter was far too large a unit for practical work. Some portion of his early life having been passed in measuring up work with quantity surveyors, he could bear out the statement that in no case was the yard used as the unit of measurement, and in the case even of large dimensions, the foot was universally employed. He could by no means adopt the recommendation of Mr. Airy, that the foot should be divided into ten parts. It was a very great advantage that it was divided into twelve parts. Any one who had had much practical work would know the advantage of being able to divide the foot by two, three, four and six. The decimal system was comparatively useless to a man who had to work with his hands, or for mental calculations, and, moreover, absolute correctness could not in many cases be obtained by it. By the duodecimal system absolute correctness could always be obtained. Very often a measurement was required which had, perhaps, to be afterwards multiplied many hundreds of times, and if there was a small fraction wrong in the original measurement it might result in a serious error. The change from the duodecimal to a decimal system would, in his opinion, be a retrograde movement. He thought nothing was to be gained by discarding duodecimals, and would say, in the words of Lord Melbourne, "Why cannot you let it alone?"

COMBUSTION UNDER PRESSURE.

From "The Engineer."

THE conditions determining the economical efficiency of steam boilers are so complex that they defy the theorist. It is quite possible to lay down certain premises and deduce from them that the efficiency of a steam generator cannot be greater than a given maximum, and this deduction will be true; but it is quite impossible to say what is the minimum below which the efficiency of the boiler, expressed in terms of pounds of water evaporated by a pound of coal, cannot fall. Between the worst and the best possible performance there is a wide tract, and it is in this tract that the engineer finds at once a field for his labors, and puzzles which he can only solve with difficulty. At the present moment the improvement of the marine boiler is a prominent topic, of conversation, at least, with marine engineers and shipowners. Hitherto it seems that in only one direction is economy being sought, namely, in the adoption of forced draught. We venture to think, however, that there are more ways than this in which the economic efficiency of boilers may be improved.

There can be no doubt that the relative dimensions of grate surface, tube surface, and calorimeter area—that is to say, the cross-section of the tubes—play a very important part in determining the efficiency of the boiler. Numbers of instances of this may be cited. Not less important is a point to which little attention is paid, namely, the thickness of the fires. That is a matter left to the stokers, and settled without any reference to the peculiarities of the coal. Yet turning to the Wigan experiments made in 1867, we

said that the reporters, Mr. Thomas Richardson, M. A., and Mr. Lavington Fletcher, say: "The thickness of the fuel on the grates has proved to be an important element in the proper management of north-country coals. We have tried 9 in., 12 in., and 14 in. fires, and in all instances, whatever were the other conditions, the greater the thickness of the fires the more speed and power were obtained from the coals." To illustrate this we may add that "Great 7 ft." coal with 9 in. fires evaporated 9.779 lbs. of water per pound of coal, at the rate of 48 cubic feet per hour, while the same coal with 14 in. fires evaporated 10.494 lbs. at the rate of 53.3 cubic feet per hour. "Black rod yard," with 9 in. fires, evaporated 10.236 lbs. at the rate of 44.74 cubic feet, and with 14 in. fires, 11.057 lbs. at the rate of 45.36 cubic feet per hour. It is sometimes assumed that, with a small grate a thick fire must be carried, or the required quantity of coal cannot be burned in a given time; but this is only partially true. However, it is usually the case in practice that the smaller the grate the heavier is the fire.

Concerning calorimeter, there is reason to believe that it is often too large. We wish first, however, to note an experiment made years ago in the United States, by Isherwood, to determine the effect of various calorimeters on the economic efficiency of a marine boiler. This investigation showed that when the area of the tube opening—calorimeter—was in the ratio of 1 to 11 of grate area, the boiler evaporated 8.57 lbs. of water per pound of anthracite, but by reducing the calorimeter to $\frac{1}{32}$ of the grate area, the economic efficiency was increased by nearly 15 per cent., but the power of the boiler was greatly diminished, because even with the aid of a steam jet, not more than a little over 5 lbs. of coal could be consumed per foot of grate per hour. It is to be regretted that the experiment was not pushed further with the aid of a fan. It appears, indeed, to be certain that the calorimeter which will best suit a natural draught is not that best adapted to a forced draught. For example, if the velocity with which the products of combustion are passed through the tubes, be doubled, the quantity of air driven through the fire being doubled, no economic advantage can be gained, because the hot gas will not have time to part with its heat; and the proof that this is the case is supplied by the tremendous smoke-box temperatures, from 1100 deg. to 1300 deg., obtained when the forced blast is used. There is reason to think that a direct and considerable advantage can be gained by burning fuel under pressure. Why, is by no means clear. Apparenty, the proper method of working a boiler with forced blast is to obstruct the smoke-box end of the tubes with thick ferrules. The gases would rush through these at a very high velocity, while in the rest of the tube their motion would be comparatively slow, and some amount of whirling would be set up which would be highly advantageous. One of the great defects of the marine and locomotive boiler is that the products of combustion move in lines parallel with the tubes, whereas, under all circumstances, the greatest value is got out of heating surfaces when the hot gases strike them at right angles. In no case, however, can too much care be taken to break up the products of combustion and mix them, so that, as Peclet has shown, hot and cold layers may not be formed. The marine boiler is fairly well designed in this respect, because the combustion chamber serves as a mixing chamber before the gas enters the flues. There is reason to think, however, that a type of boiler much used in the United States, and in a modified form, by Mr. Holt, of Liverpool, is better adapted for forced combustion than is the ordinary boiler. In the boiler we refer to, the products of combustion first pass away to the combustion chamber through a number of tubes about 12 in. in diameter, the ordinary $3\frac{1}{2}$ in. or 4 in. tubes returning above them and the furnace in the ordinary way. It is held, we know, that, space for space, this boiler is not so powerful as the ordinary type, but it must not be forgotten that one result of working with forced draught will be, other things being equal, to augment the absolute as well as the economic efficiency of steam generators; so that the objection just stated seems to fall to the ground.

It is much to be desired that some competent firm shall carry out a series of experiments on the influence of the calorimeter, or, more exactly, on the value of combustion under pressure. There would, for example, be no difficulty in burning

fuel under a pressure of 4 lbs., or even 10 lbs., on the square inch. The conditions necessary are, sufficiently powerful blowing machinery and a very small calorimeter, obtained by the use of annular stoppers or tompions in the tubes. Of course, we do not advocate the use of such air pressures as we have just named, at least, without due inquiry, because there is some point beyond which the work spent in compressing the air would cost more than the advantage gained. The principal benefit to be obtained would, we believe, be perfect combustion. Mr. Otto has shown that, in his gas engine he can explode mixtures of gas and air so diluted that combustion could not be effected at atmospheric pressure. This is done by compressing the dilute mixture to about 30 lbs. on the square inch. At high furnace temperatures it is indisputable that there is a strong tendency to dissociation manifested, and this is probably one reason why much carbonic monoxide escapes unconsumed with an unusual waste of fuel. Burned under pressure, it is more than probable that the union of the gases would be more readily affected, and one most important result would be, no doubt, that the total quantity of air admitted to the fire might be freely reduced. It is not indeed impossible that complete combustion might be effected with as little as 15 lbs. of air per pound of coal, instead of the 24 lbs. or 25 lbs. now found necessary. It may be laid down, however, that concerning the value of combustion under pressure for steam generation, we are quite in the dark; the only thing that can be said, as far as practice is concerned, being that all the indications are favorable in a high degree to the adoption of the system. Perhaps some of the engineers now interested in forced combustion will push their inquiries a little further. The cost of an experiment would be very small. An ordinary portable boiler with a closed ashpan, very thick ferrules in the smoke-box ends of the tubes, a Root's blower, and a tank to measure the water pumped into the boiler would suffice. For a very moderate outlay in this way a great deal of very valuable information might be obtained.

BALATA.

From the "Journa. of the Society of Arts."

In the *Journal of the Society of Arts* for November 20th, 1863, a list of subjects for premiums was published, amongst which was one "For any new substance or compound which may be employed as a substitute for india-rubber or gutta-percha in the arts and manufactures." This was responded to in the *Journal* for February 26th, and March 4th, 1864, a letter being published in the latter from Sir William Holmes, from British Guiana, advising the despatch to the Society of a box containing samples of balata, both in the fluid or milky, as well as in the dried or coagulated state. In the letter referred to, Sir William Holmes speaks of the small specimen which was exhibited in the International Exhibition of 1862 as attracting a considerable amount of attention, and further says, so far as he could judge, balata was not to be rivaled either by india-rubber or gutta-percha, possessing "much of the elasticity of the one, and the ductility of the other, without the intractability of india-rubber, or the brittleness or friability of gutta-percha." Sir William Holmes further expressed a hope that balata would, ere long, be included as an important item amongst the exports of the colony, Notwithstanding that this was written as far back as 1864, little or nothing has been done since towards making balata a regular article of import; occasional notice has been drawn to it from time to time, and the subject as frequently allowed to drop. As a proof of the truth of Sir William Holmes' statement as to the ductility of balata, it may be mentioned that a sample of that exhibited in the Exhibition of 1862, and presented to the Kew Museum at the close of the Exhibition, is still in a fairly ductile state, and shows no such brittleness as is the case with gutta-percha.

In connection with this subject of the development of balata, Mr. G. S. Jenman, Government Botanist, and Superintendent of the Botanical Gardens in British Guiana, has just drawn up a very exhaustive report, the result of which, it is hoped, will be to bring the substance into a regular commercial channel.

The title of the report is "Balata and the Balata Industry, Forest Laws, &c.," and it commences with a very interesting description of the bullet tree region, including its inhabitants, character of the vegetation, &c. Coming to the immediate subject of the report, Mr. Jenman describes the bullet tree, from the bark of which balata is obtained, as a large forest tree ranging from Jamaica and Trinidad to Venezuela and Guiana. He refers it to *mimusops balata*, and says: "The vernacular name appears to be applied to two species or sub-species which are united by Grisebach, in his 'Flora of the British West Indies.' Young plants of *mimusops globosa*, of Jamaica and Trinidad, growing in the Gardens, seem to be distinct from the Guiana type. The tree grows to a height of 120 feet, and has a large spreading head. The trunk is nearly cylindrical. The bark is about half an inch thick, with deep parallel fissures an inch or so apart. The hard reddish-colored wood is one of the densest in the colony, and is used for all sorts of purposes where great strength and durability are required. The tree is more plentiful in both the eastern and western parts of this colony than in the intermediate region. From the east bank of the Berbice river to the Corentyn is the region of its greatest plentifulness in the colony, but its distribution extends still eastward beyond the Corentyn into Dutch Guiana, where a grant of several hundred thousand acres has recently been acquired by an American firm for collecting balata. The trees are more plentiful in this region in the depths of the forest than near the rivers, hence the creeks form arteries to the balata grounds. Several of the creeks on both sides of the Canje are instances of this. The wood cutters of this district regard the tree as inexhaustible; in the interior of the forest it exists in profusion and abundance, and lies beyond the reach of the balata collectors as they at present conduct their operations. As the trees near at hand become exhausted, they will, no doubt, alter their habits, and make clearings as drying places in the heart of the forest; but now they are under the obligation of returning to the settlements on the creeks with the milk they have collected to dry. Under this necessity they can at most only penetrate about two days' journey, but, so far as they have explored, they report there is no diminution in the abundance of the trees. The forest at this depth, of course, has never been touched by woodcutters, as, for convenience in getting their timber out, they have to confine their operations to the banks of the river and creeks, rarely going in more than a mile or two."

Regarding the character and value of balata, Mr. Jenman says its strength is very great, and as it does not stretch under tension, for special appliances, such as bands for machinery, it is unequaled. It has recently been pronounced by an American firm of manufacturers as "the best gum in the world."

Dr. Hugo Müller, F. R. S., in a report on the substance says: "It seems that balata is by no means neglected, and, in fact, it would find ready purchasers if more of it came to the market; as it is, the supply is very limited, and generally it comes only once a year. It commands a higher price than gutta-percha, and this in itself is a proof of its usefulness. It is used almost in all cases in which gutta-percha is used, but on account of its higher price, only for superior purposes. It seems that balata is treated by the manufacturers simply as a superior kind of gutta-percha, and, therefore, its name disappears when manufactured. Nevertheless, balata is distinctly different from gutta-percha, and this is especially manifested in some of its physical characters; for instance, it is somewhat softer at ordinary temperature, and not so rigid in the cold.

"In one respect balata shows a very marked and important difference from gutta-percha, and that is its behavior under the influence of the atmosphere, whilst gutta-percha, when exposed to light and air, soon becomes altered on the surface, and changed into a brittle resinous substance, into which the whole of the mass is gradually converted in

the course of time. Balata, on the other hand, is but slowly acted upon under these circumstances. The electrical insulating quality of balata is said to be quite equal to that of gutta-percha."

Mr. Jenman says that the collecting of balata is an open and recognized business, is carried on only in Berbice, but he proceeds to show that the greater part of that so collected is not obtained from trees on Government grants, but surreptitiously from Crown lands; and Mr. Jenman further says that much damage is done to the Crown lands by the depredations of collectors, and "that it is desirable, in the interest of the colony, till effective rules are devised for the protection of the forest and the preservation of this valuable wood, that the trade should be discontinued."

The life of the balata collectors is a very hard one. The ground they have to traverse is generally very wet and swampy. In many cases the traveler sinks at every step up to his knees, and this continues for miles, and water often has to be waded through up to the armpits. When the collecting ground is not too far distant, women sometimes accompany the men, and cook or assist in laying out the calabashes, and collecting the milk while the men fell and ring the trees. The collectors connected with a grant sell the milk they collect to the agent on the grant, and never dry it themselves. The price for pure milk is four shillings per gallon, or occasionally a dollar, and for clean well dried balata one shilling per pound. Considering the circumstances of the people who follow it, balata collecting, if pursued with industry, is a paying business. The calling pays better, while the season lasts, than the best mechanical trade; with fair weather, a man can earn from one to five dollars a day at it, and an exceptionally expert collector has been known to make twenty dollars in three days.

The instruments used in collecting the milk are an axe for felling the trees, a cutlass for making the channels in the bark to cause the milk to flow, and two or three gourds in which to collect the milk. The collector commences operations by chipping a piece of the bark from the selected tree, and if the milk runs well he quickly shaves the moss and rough bark from the side he intends to tap, then stooping down with his back to the front of the tree, but on one side of it, he cuts from the base of the tree obliquely upwards towards himself, in the bark, a narrow channel, then moving round the other side, a similar one. These grooves are generally about eighteen inches long; they form an acute angle at the base, just below which a niche is cut in the bark and is slightly lifted with the end of the cutlass, and a calabash inserted by the rim under it. Occasionally a piece of palm or maranta leaf is inserted under the bark, and the calabash is placed on the ground, the leaf conducting the milk into it. The channels are then quickly cut upwards parallel to each other on the opposite sides, about ten inches apart, the operator continuing them as far as he can reach, which is about eight feet from the ground. The milk trickles from cut to cut down this zig-zag line into the calabash beneath. The best collectors cut the bark with much neatness and precision, and do not injure the trees; but little care is usually taken, and the wood is injured with every stroke of the cutlass, the result being that numerous trees are killed, and left standing. Large trees are always tapped on the opposite sides, careful collectors leaving the intervening spaces for subsequent years. It takes from five to ten minutes to cut the channels in each tree, and the milk runs from forty to sixty minutes; at first it forms a little rivulet, but after about twenty or thirty minutes, it only drips. After a little use, the gourds become so coated on the inside with dry balata, that they have to be occasionally soaked in water, when it peels off freely, leaving them perfectly clean again. The yield of a tree varies according to circumstances. If favorable, a tree 15 inches to 20 inches in diameter, bled 8 feet high, will yield 3 pints of milk. Trees are often felled, and then tapped by ringing the bark in parallel transverse lines, at intervals about a foot apart.

To dry the milk, it is poured into shallow wooden trays, the insides of which are previously rubbed over with oil, soap, or grease, to prevent the balata sticking, and the substance is exposed to as much air as possible, and sometimes to the sun. In fine weather it takes two or three days to dry, and in wet weather a

week or more; when it is sufficiently dry to be removed from the boxes, the sheet is thrown over a line, or bar, to drip, and become hard.

A good deal of foreign matter is found in the milk, and Mr. Jenman says adulteration is systematically carried on, and the agents have at all times to be on their guard against it.

The report concludes with a consideration of the damage done to the forests, and some remarks on their better conservation.

ENGINEERING NOTES.

Hull Alexandra Dock.—This dock, which was inaugurated yesterday week, is 2,300 ft. in length and 1,000 ft. wide, and contains a water area of 46½ acres. This is a very large water space, as compared with any of the older docks at Hull, and the advantages of great width and length are increased by the depth of water, which will be 34 ft. 6 in. The entrance to the dock, which is bell mouthed, has received much attention, and the lock is 550 ft. in length. with a width of 85 ft. There is a swing bridge over the lock for the railway, and there are three pairs of gates for letting vessels in and out. The depth of water on the sill at high water ordinary spring tides is 34 ft. A river bank, which has been made, has a total length of 6,100 ft., and the whole area of the dock estate is 192 acres, of which 152½ acres have been reclaimed from the Humber. With the view of accommodating the largest class of vessels, the Hull and Barnsley Railway and Dock Company has constructed at the north-east corner of the dock two dry docks. One is 500 ft. in length, with an entrance of 60 ft., and the other is 550 ft. long, with a width of 65 ft.

An experiment is shortly to be made in the conveyance of laden railway trucks between the mainland and the Isle of Wight. The Carrier, a paddle steamer, possessing facilities for receiving cargoes, has been acquired, and it is intended to run this vessel from Langstone Harbor to Brading. A wharf is to be erected at Langstone, and a line of Railway constructed to the water's edge. On the upper deck of the Carrier a double line of rails has been laid from stem to stern, affording accommodation for fourteen ordinary railway trucks. The vessel will come alongside the wharf, where there are steam appliances for running the trucks on the deck direct, and at Brading similar facilities will be provided for unshipping them, and again placing them on the railway. It is also anticipated that in the course of time passenger trains may be carried across the water by similar means. At present considerable difficulty is experienced in conveying merchandise to and from the Isle of Wight, the arrangements necessitating the unloading of goods, which occasions considerable delay. This will be entirely obviated should the experiment prove successful. One of the principal objects of the new system is to facilitate the transit of vegetables, &c., for the Isle of Wight, and it is calculated that at least two hours will be saved by this means. It is stated that trucks could be filled with coal at the pits and conveyed to any station in the Isle of Wight without being unloaded, and the same advantage would apply to furniture vans. After the arrival of the Carrier at Portsmouth from Newhaven a trial was made of her sea-going capabilities between Langstone and Brading with satisfactory results.

IRON AND STEEL NOTES.

Examination of Cast Steel.—We learn from *La Metallurgie* that some interesting studies on the structure of cast steel have been made in the laboratories of Creusot by MM. Osmond and Worth. It was already known that cast-steel consists of a kind of cellular network of a carbide of iron, not easily attacked by acids, inclosing particles of soft iron easily attacked and dissolved. In order to examine this structure more closely, MM. Osmond and Worth prepared some very thin sheets from the samples to be examined, not exceeding two or three hundredths of one millimeter in thickness. These were attached to glass plates by means of Canada balsam, and then exposed to the action of dilute nitric acid, which dissolves out all the soft iron, and leaves the network of carbide in a form convenient for examination. It was found that the distribution of the network was not uniform; groups of carbide cells occur together, with spaces between made up of soft iron. The regularity in the diffusion of the carbide appears to influence the quality of the steel, as that steel which had been most worked was most uniform in structure.

From 1862 to 1882 the production of sulphuric acid in Germany has increased from 22,311 tons to 157,961 tons, chiefly obtained from Siegen pyrites. Lump pyrites are burnt in kilns with movable grates, and smalls in Perret or Maletra kilns. The burnt ore from certain mines is afterwards smelted for iron, but the Siegen ore retains too much sulphur to permit of its use in metallurgy. M. Hasenclever seeks to refute the views of Dr. Lunge on the inconvenience of zinc sulphide—blende—in the manufacture of sulphuric acid.

A new method of producing alloys of iron or manganese, or iron and manganese, with tin, is described in a patent taken out by Charles Billington and John Newton, who claim the introduction of iron or manganese, or of iron and manganese not previously alloyed, into a bath of molten tin, kept at a suitable temperature, the iron or manganese being connected by wires to a dynamo machine or battery. When put in circuit it is claimed that the current of electricity throws off the iron or manganese of iron and manganese into the tin bath, and produces chemical action between the metals, which causes them to alloy with each other in any desired proportion that can be regulated with great nicety.

A temperature of 570 deg. will produce a dark blue color on polished steel, and 590 deg. a pale blue. Oil or grease of any kind will answer for drawing the temper of cutlery. The temper for lancets is obtained at 430 deg. Fahr., axes at 500 deg., swords and watch springs at 530 deg., small saws at 570 deg., and large saws at 590 deg. Copper-colored spots are not produced by tempering, but they may be obtained on the polished surface of steel by immersing the article in a solution of sulphate of copper.

The Clapp-Griffiths Steel Process.—There has been a tendency of late to render smaller users — comparatively speaking — of steel independent of large manufacturers, or, putting it another way, to enable them to become their own manufacturers without having to put up an expensive plant. This tendency is illustrated by several devices for this purpose which have been described in our columns, some of which are certainly meritorious. Amongst these latter, and the latest that has been brought under our notice, is the invention of Messrs. Clapp and Griffiths, of Newport, Mon., which consists in a fixed converter for making soft steel to take the place of wrought iron. The objeet is to bring the question of steelmaking within the reach of manufacturers having puddling or charcoal iron forges, at a small cost for plant; and as steel is so rapidly replacing iron, this is an important matter to the trade, the members of which in most cases have to purchase steel from the large makers. The converter is a fixed vessel about 5½ ft. outside diameter, by 10 ft high, lined with silica bricks, and is provided with from four to six horizontal tuyeres, having valves at the rear for regulating the blast during the operation of blowing. A very important feature in the invention is the position of these tuyeres, which are placed about 9 inches above the floor of the converter, and only 4 or 5 inches under the surface of the metal when charged. This enables a soft blast of only from 5 to 6 lbs. per square inch to be used as against from 20 to 25 lbs. in an ordinary Bessemer converter. As the slag rises on the metal, it is run off through a slag hole, and when the blow is completed, the steel is tapped, in the same manner as iron is drawn from an ordinary cupola. It is then taken in a ladle, after the addition of ferro-manganese, to the casting pit and run into ingots. The pig-iron is first melted in a cupola and then run into the converter, where the conversion into steel takes from 15 to 20 minutes. It is stated that plant can be erected at a moderate cost to make from 50 tons and upwards per week according to the number of converters at work, the capacity of the converters varying from 1 ton to 3 tons per charge, and the number of charges per day of twelve hours being from twelve to fifteen for each converter. It is claimed that a Clapp-Griffiths plant can be erected at a cost of about one-half of a Siemens plant to produce an equal quantity of steel, and in cases where forges have small blowing engines suitable for the purpose, the cost is considerably less. The process has been worked for some time past by both English and foreign firms with, we understand, every success.

Not having had the opportunity of inspecting the process ourselves, we have, so far, only the statements of those interested in the invention to go upon. Let us at once say that we accept their statements in perfect good faith. Their views have been so modestly advanced as compared with those of some other inventors which have of late been submitted to us that we feel bound to accept them without hesitation or reserve. It is, however, satisfactory to us to be able to produce confirmatory evidence of the merits of the invention perfectly independently of the inventors or their friends, and, indeed, without their knowledge. It so happens that we have just received from the United States two papers upon this process. These papers, which were read before a recent meeting of the American Institute of Mining Engineers, held at New York, were, one by Mr. J. P. Witherow, of Newcastle, Pa., and the other by Mr. R. W. Hunt, of Troy. From these papers it appears that the Clapp-Griffiths process was tried in South Wales two years since with a reasonable amount of success, the operations having been witnessed by the authors of the two papers. The process has since been tried at Pittsburgh with improved machinery, and with results which it will be seen produced a strong impression at the meeting of the institute to which we have referred. It would appear that the process almost completely eliminates the silicon, while leaving untouched the phosphorus, in fact, the percentage of the latter in the steel is higher than that in the pig nearly in proportion to the concentration due to waste in blowing, and the cinder contains only traces of phosphoric acid. As far as regards the mechanical qualities of the steel, it is demonstrated that low silicon permits of high phosphorus and thus it has been suggested that, since the Clapp-Griffiths process has shown its capacity to turn out with special success mild steels, and since it proves that the elimination of phosphorus is not necessary provided the silicon is removed, therefore it partly covers the ground occupied by the basic process. Mr. Hunt, while believing that it cannot make rails or ship plates in competition with the regular Bessemer plants, holds that it can compete with them in small products, even if it does not make an article which they cannot produce. The quality of the metal made by this process in America has been proved, many hundreds of tons of it having been made by Messrs. Oliver Brothers & Phillips of Pittsburgh, and placed on the market in different forms, and consumers have recorded their approval of it. Mr. Witherow states in his paper that it costs 50s. per ton at Pittsburgh to convert pig-iron into puddled bars, but a ton of pig-iron can by the Clapp-Griffiths process be made into steel blooms for 24s. in mills, or about half the cost, while, if the iron is run direct from the blast-furnace into the converter, the cost will only be about 16s. This testimony is borne out also by Mr. Hunt, who believes the converter will be found desirable for existing works whose products in the past have been exclusively wrought iron. In connection with

the technical features, it may be stated that, when the process was first started at Pittsburgh, the best brands of English Bessemer iron were used, and the steel produced so far exceeded all requirements that a lower standard was ventured upon, and a pig was used having a greater proportion of phosphorus. A mixture was tried which gave a metal with about 0.34 per cent. of phosphorus, and this worked so well that Mr. Hunt doubled his proportion of high phosphorus pig, obtaining a steel with 0.54 per cent. phosphorus, and a test piece was bent double when cold, and worked very satisfactorily when hot. As yet it has not been ascertained how much higher the content of the phosphorus in the pig may go, and this appears to be the only question remaining unanswered. The composition of the steel is shown by several analyses in Mr. Hunt's paper; and, taking that ascertained just before the meeting, we find that there was: Carbon, 0.08 per cent; silicon, 0.01; phosphorus, 0.50; manganese, 0.48; and sulphur, 0.09; while the physical tests gave tensile strength about 80,000 lbs., elastic limit 59,000 lbs., elongation 23 to 24 per cent., and reduction of area 36 to 37 per cent., in all cases ½-inch round test pieces 8 inches in length being used. On the whole, the results appear to be most satisfactory, and, judging by them, the question arises as to how far the Clapp-Griffiths may affect other steel-making processes on a large scale. It appears destined to influence the trade by its application in the direction indicated by us at the commencement of our article, and if the results of working on the smaller are corroborated by those produced on the larger scale—should it be so tried—it might go hard with some of our leading works. Many nice questions, however, will have to be settled before that day arrives, and, in the meantime, there is an ample field for the process amongst users of steel in moderate quantities, who may be glad to avail themselves of the advantages it offers.—*Iron.*

RAILWAY NOTES.

The railways of New South Wales are the property of the Government. The Southern line is in operation from Sydney to Albury, a distance of 386 miles, and will be opened before the end of the present year to Albury, where it joins the line from the Murray to Melbourne. A branch is also being pushed on from Junee to Hay in a westerly direction. The Western line, which crosses the Blue Mountains, is open to Wellington, 248 miles distant from Sydney, and is being extended to Bourke, 504 miles from Sydney, on the river Darling. A suburban line runs to Richmond and Parramatta; while from the important seaport of Newcastle there is a railway communication to Glen Innes, a distance of 399 miles; and to Narrabri, a distance of 320 miles; while numerous other lines are being commenced.

In a report on a collision which occurred on the 23d May at Boston station, on the Great Northern Railway, Major Marindin says: "This collision was caused by the coupling between the engine tender and the brake van becoming detached upon a falling gradient, immediately the engine began to push these vehicles back towards the passenger train. The evidence appeared to show that the shackle was not properly oiled, and that the coupling being stiff, it was pushed off the hook when the engine began to set back. I think that the best means to adopt in order to prevent the recurrence of an accident from a coupling coming loose would be to fit the draw-bar hooks of all vehicles with a spring or a weighted catch as is done upon the London and North-Western Railway and other lines, and until this is done it is clearly advisable that in the operation of putting empty carriages on to a train the couplings should be screwed up sufficiently to make it impossible for them to become detached as in this case. It would also be well to have a porter in the brake van to make use of the brake power if necessary, except, of course, in cases when the vehicles are fitted with an automatic brake coupled on to the engine and in working order."

ORDNANCE AND NAVAL.

In the following figures are given, first, the sea-going merchant fleets of all nations, and, second, the steamships of all nations:—Great Britain, 22,500 vessels, 11,200,000 tons; United States, 6,600 vessels, 2,700,000 tons; Norway, 4,200 vessels, 1,500,000 tons; Germany, 3,000 vessels, 1,400,000 tons; France, 2,900 vessels, 1,100,000 tons; Italy, 3,200 vessels, 1,000,000 tons; Russia, 2,300 vessels, 600,000 tons; all nations, 46,000 vessels, 23,000,000 tons. Thus it will be seen at a glance how tremendously England outrivals every other marine Power. Her preponderence is even greater in steam vessels, as appears in this second statement:—All nations, 7,764 steam vessels, 9,232,000 tons; Great Britain, 4,649 steam vessels, 5,919,000 tons; France, 458 steam vessels, 667,000 tons; United States, 422 steam vessels, 601,000, tons; Germany, 420 steam vessels, 476,000 tons; Spain, 282 steam vessels, 305,000 tons; Italy, 135 steam vessels, 166,000 tons; Holland, 127 steam vessels, 155,000 tons; Russia, 194 steam vessels, 149,000 tons.

The Electric Light and Guns.—Mr. Walter Winans, of Brighton, has applied the incandescent lamp to the front sight of a rifle so as to render it visible in the dusk of evening or when there is insufficient light to take an aim. It consists of a miniature electric lamp fixed near the muzzle of the gun and shielded by a metal screen having a hole in it turned to the shooter, who thus sees a bead of light as the front sight. The current is supplied by a small battery in the stock of the gun. We may add in this connection that the big 14-ton gun in the Inventions Exhibition was brightly illuminated inside by an arc lamp on the occasion of the Society of Arts Conversazione. The lamp was provided by Colonel Maitland, R. A., who devised it for examination purposes. It was manufactured by Messrs. Johnson and Phillips, and supplied by a Holmes-Burke primary battery.

A New Protection to Ships.—The long-standing rivalry between heavy ordnance and armor plates is likely to be disposed of in a manner little expected, as a means appears to have been discovered whereby the effects of shot and shell, and even torpedoes, will be effectually neutralized. For some time past, naval architects have ceased to rely solely upon armor for the protection of ships, for, notwithstanding the enormous thickness to which armor plates had attained, they were found to be no match for the artillery that was brought to bear upon them. Steel plates and compound plates were next tried, but to no avail. As a further increase in the thickness of plates—whether of iron or steel, or both combined—was impracticable, owing to the overweighting of vessels with armor, shipbuilders tried the expedient of supplying a second line of defence in the coal bunkers, which were constructed along the sides of ships, especially those parts where the machinery and magazines are located. They certainly, to some extent furnished that second line without overburdening the vessel, for coals have to be carried under any circumstances. But a far more effective protection appears now to have been supplied by the invention of a composition which, besides being efficacious as a protector, possesses the merit of being light, a desideratum much wanted by naval constructors. This composition is a preparation obtained from cocoanut cellulose, which has the remarkable property, when penetrated by shot and shell, or even after the explosion of a torpedo, of closing up as rapidly as it has been perforated, and thus preventing the influx of water into the ship's hold. The very appropriate name of "cofferdam" has been given to the preparation, which, besides being very light, is highly elastic and tenacious. Some important experiments have lately been made with the composition before a French commission at Toulon, which, if everything that is reported concerning them is true, proves the preparation to be destined to solve the armor-plate controversy. The commission submitted the composition to a threefold test—against shot, shell and torpedo. The target was a cofferdam made of a mixture of 14 parts of pulverized cellulose and 1 part of cellulose in fiber. This composition was compressed to a felt-like mass, of which one cubic meter weighed 120 kilograms, or one cubic foot about 8 lbs. A layer of beams 12 centimeters (4¾ in.) thick represented the side of the ship, behind which there was a layer of cofferdam 60 centimeters (2 feet) thick Against this target a 19 centimeter (7½ in.) solid shot was fired, which penetrated it, taking with it not quite one-fifth of a cubic foot of composition, a very small quantity, considering the size of the shot. But as soon as the shot had passed through the target the cellulose composition closed up again, and so firmly that a strong man was unable to force his arm through the opening made. A box filled with water was then fixed against the aperture, the contents of which ought to have acted in the same way as if the cofferdam had been washed by the sea. It was observed that a few drops of water began to percolate after the lapse of from ten to fifteen minutes, and even after the composition had become well saturated with water, only between three and five pints of water escaped per minute, which could be easily intercepted by pails. As soon as the cellulose had become thoroughly soaked and grown denser, it offered greater resistance to the percolation of water, which finally almost ceased to flow. The experiments with shell gave similar results, the breach made closing automatically. It was also found that the cofferdam was proof against fire. A special experiment was made, in which red-hot coals were placed upon a mass of cofferdam, and covered with the same composition, the result being that the fire went out. The experiments with torpedoes were not so decisive as those with shot or shell. A chest was anchored out at sea, one side of which was lined with cofferdam, a torpedo attached to it, and exploded. The chest floated for a few second and then sank. When fetched up by a diver it was found that the lid had been blown off, but that the cofferdam composition was little injured. The above experiments appear to prove that the material in question possesses the property of automatically closing a leak caused by shot or shell and of protecting a ship to a certain extent against fire. Whether its use will render ships unsinkable remains to be shown, but we understand that, in order to investigate this point thoroughly, further experiments on a larger scale are to be undertaken by the Toulon commission.—*London Morning Post.*

The New Naval Guns —The Woolwich correspondent of the *Times* writes: The new guns which have been designed to maintain the naval supremacy of Great Britain are in an advanced state, but they have to undergo a course of experiments to settle the range tables and other particulars, and it will probably be the beginning of next year before they are ready for sea. This will, however, be earlier than the ships which are to carry them can be completed, and there will be ample time available for a full and leisurely study of their requirements and capabilities. The first of the four 63-ton steel breech-loaders for Her Majesty's ship Rodney, will be shortly finished, and will be used as an experimental gun, care being taken that it is not damaged in the process by any of the surgical operations to which the experimental guns are occasionally subjected. Although 17 tons lighter than the 80-ton muzzle-loaders on board the Inflexible, the 63-ton gun is expected to surpass the older weapon in its destructive power. It will probably throw a 13½-inch shot, of 1,250 lbs. weight, with a powder charge of about 580 lbs., and the estimated velocity at the muzzle is to be 2,100 ft. per second. The 80-ton gun projectile weighs 1,700 lbs., but the cartridge is but 450 lbs., and the muzzle velocity recorded is 1,600 feet per second. Should the new gun realize expectations, it will penetrate 29 inches of wrought-iron armor at close quarters, and prove too much for 27 inches, even at the liberal fighting range of 1,000 yards. Still more powerful, but not in the same ratio of increase, will be the 110-ton guns now being manufactured for Her Majesty's ship Benbow. There are three of these

guns ordered, one of which will be surrendered for the purpose of scientific experiments, while the other two will be sent on board ship, where, however, they will not be wanted until the midsummer of 1886. The projectile will be 16¼ inches diameter, and weigh 1,800 or 2,000 lbs. The powder charge will be the enormous one of 900 lbs., or half the weight of the projectile, supposing this to be 1,800 lbs., on which supposition the velocity may be reckoned at 2,050 feet per second, and its power of penetrating armor at 31½ inches near the muzzle, or 2 inches less at 1,000 yards. The new guns will be greatly superior to the Italian 100-ton guns, which are at present at the head of all the naval artillery in the world, and they are also in advance of the 110-ton guns which are doing duty for England on the fortifications of Malta and Gibralter, although these are larger in the bore by 1½ inch. The substitution of steel for wrought iron admits of heavier charges of powder, and this fact makes all the difference. Two huge sleighs for the proof trials of these and similar guns are being built—the one for use at Woolwich and the other for Shoeburyness, whither both the experimental guns just mentioned will be sent for practice at the sea ranges. To Shoeburyness there is also to be immediately sent the 80-ton gun which has been returned to Woolwich from the Inflexible. The inner tube of the gun is unquestionably cracked but this is regarded as a comparatively small injury, and before it is repaired the gun will be fired with a series of heavy charges at the targets which have been put up at Shoeburyness to represent the Spithead forts. These targets, which are respectively faced with granite, wrought-iron plates, and compound steel, have already been attacked in a course of earlier experiments, and the compound steel has shown to very great advantage. The double barge Magog will, as heretofore, convey the 80-ton gun, but for the 110-ton gun a still larger craft is being built, which is to be called the Gog, and measures 20 feet longer than the Magog.

Heavy Ordnance in the United States.—At the South Boston Ironworks work is progressing in shrinking a series of steel rings upon a 6-inch breech-loading rifled gun, of the same metal, now in process of manufacture. This gun is one of six of like caliber, and two of 8-inch bore, to form a part of the armament of the new cruisers ordered by the American Congress. These guns are all of the built-up pattern. The gun, when completed, will weigh 11,000 lbs., and carry a 100-lb. projectile, propelled by a powder charge of 50 lbs. The initial velocity of the shell is estimated at 2,200 feet per second, and the effective range of the gun at seven miles. The extreme length of the piece is 193.53 inches, the diameter of the chamber is 7½ inches, and its length, including slope, 37½ inches. The rifling consists of 24 lands and 24 grooves (a land being the raised portion between the groove or indentations) with increasing twist of one turn in 180 calibers at the breech, and in a distance of 134 inches to increase to one turn in 30 calibers. The width of grooves decreases 0.05 inch from the breech to the muzzle end of the bore. The breech mechanism is of the interrupted screw pattern, and in rear of the nose-plate is an asbestos and mutton-suet ring, which serves as a gas check. The foundation of the gun is a steel tube 184 inches in length, which is cast in the rough at the Midvale Steel works in Pennsylvania, and which receives interior and exterior finish at South Boston. It is then ready for the jacket, a cylinder of forged steel large enough for the breech end of the original tube to be inserted in it for about one-third the length of the latter, measuring from the breech forward. This tube is set up on end, and the jacket, whose inside diameter is a few thousandths of an inch smaller than the exterior of the tube, is expanded by heat and then lowered into the tube, after which it is shrunk by jets of cold water, making what is called a cold weld. This jacket extends back of the tube 9.58 inches, to accommodate the breech mechanism. In the same manner, excepting that the tube is disposed horizontally, five steel rings are shrunk upon the jacket, in addition to one jacket-hooking ring, one tube-hooking ring, and three chase rings; and, again, outside of the jacket rings are the trunnion ring, which is screwed on, and the elevating ring. The forward part of the tube, 71 inches from the muzzle, is the only portion of the gun not reinforced. The process of shrinking on the jacket and rings is a very delicate one. The expanding is done entirely by gas, the jacket or ring, as the case may be, being introduced into a network of powerful gas jets, which surround the metal both externally and internally with flame. Having been sufficiently expanded, which requires a temperature of 600°, the jacket ring is slipped over the tube, an hydraulic jack of 100 tons' power pressing the enveloping band home. So well does this jacket perform its work that the joints between the several rings are almost imperceptible, and are often no wider than 0.002 inch. The next process is the shrinking, which is done by cold water, as already mentioned. The shrinkage of the jacket amounts to 0.01 inch, of the jacket rings to 0.24 inch, and of the chase rings from 0.19 to 0.16 inch. While the shrinking is going on, the temperature of the tube is kept down by a stream of water running the entire length inside. In addition to these 6-inch guns there are in process of manufacture at the South Boston works one 12-inch cast-iron breech-loading rifle, one 12-inch cast-iron breech-loader with steel hoops and tube, and one 12-inch cast-iron gun with steel tube and a square steel wire coil, having a suction of 0.15 inch. The first of these three guns has been shipped to Sandy Hook. The length of its rifling is 284 inches, of increased twist, sixty lands and grooves. The diameter of the chamber is 13½ inches, and the length 69 inches. The charge is 150 lbs.; weight of projectile, 800 lbs.; the range of the gun is from 6 to 8 miles. The steel tube of the second gun is from the gun factory of Sir Joseph Whitworth, Manchester. It weighs 6 tons, and is 14 feet long. Its rough boring is now being perfected, and the exterior surface machined. It will then be shrunk into a cast-iron envelope weighing 40 tons, and the

finished gun, with its steel hoops will weigh 58 tons.

BOOK NOTICES

Publications Received.

Notizie Sulla Agricoltura in Italia. 1885. Rome.

Proceedings of the Institution of Civil Engineers:

No. 2054.—Metropolitan and Metropolitan District Railways. By Benjamin Baker, M. Inst. C. E.

No. 2066.—Various Methods of Traction Applicable to Railways. By Marcel Deprez and Maurice Leblanc.

No. 2080.—The Steel Permanent Way of the London & N.-Western Railway. By Francis Wm. Webb, M. Inst. C. E.

Student Paper—No. 190.—Blasting Rock at Blyth Harbor. By William Kidd, Stud. Inst. C. E.

—— Water Supply. By William Pole, F. R. S.

Monographs of the United States Geological Survey.—Vol. VI.—Contributions to the Knowledge of the Older Mesozoic Flora of Virginia. By William Morris Fontaine. Washington: Government Printing Office.

Vol. VIII.—Paleontology of the Eureka District. By Charles Doolittle Walcott.

Only students of Paleontology will regard these books with particular interest.

Vol. VI. is embellished with 54 large tinted plates, which supplement the descriptive text in the most satisfactory manner. The plates are tinted, and the typography of this, as of all the other volumes, is excellent.

Vol. VIII. is devoted to descriptions of the fauna of the Cambrian, Silurian, Devonian, and carboniferous formations.

For the student's benefit the text compares the described features with those of species found in other localities, and already known to Paleontologists.

Reference libraries everywhere are incomplete without these reports The sientists of the next generation will not be properly equipped for geological work without the knowledge of what has been recorded during the present researches.

All students of Natural History know how valuable are good illustrations of the objects with which they wish to familiarize themselves, and students have long since learned to place a high values upon the illustrations of the reports of the United States Geological Survey.

Water-Closets: A Historical, Mechanical and Sanitary Treatise. By Glenn Brown, Architect, Assoc. Am. Inst. of Architects. New York: The Industrial Publication Company. 1 vol., cloth. Price $1.00.

In this book Mr. Glenn Brown, who is also the author of one of the latest volumes of the Science Series,* has condensed much useful information, of interest alike to the historian, the manufacturer, the sanitarian and the householder. He begins his subject with a historical review, next describes and illustrates some of the first patented mechanical apparatus for the reception and removal of fæcal matter, and finally discusses at length the modern closets—that is, those types which are at present manufactured and sold. The copious illustrations of the book add much to its value, although we can hardly agree with the author's opinion that they are all made in a uniform style.

On page 144 we notice a slight error in enumerating the well-known English siphon and tumbler-tank of Mr. Isaac Shone as one of a number of water-closet tanks. The tank is never used to supply water-closets with flushing water, but is intended to be placed underground to receive all discharges from soil and waste pipes, and to act as a "hydraulic ejector," as the inventor calls it, that is, as a flush tank to the main house-drain.

We also cannot agree with the writer's opinion of some of the most recent water-closets, and think that upon actual experience in using some of these in his architectural work—and which he evidently did not have when writing his book—he will find it necessary to greatly modify his opinion. In one instance, particularly, his drawing of the closet is accurate, and his description of the same closet faulty and incorrect.

* "Healthy Foundations for Houses." By Glenn Brown, Architect. Van Nostrand's Science Series, Vol. 80.

MISCELLANEOUS.

Alarm for Icebergs.—The recent collision of the steamer Baltic with an iceberg and the delays and narrow escapes from accidents on the part of other vessels on the Atlantic, has stimulated the wits of American inventors, and numerous suggestions and experiments have followed. Prof. Alexander Graham Bell, and Mr. Frank Della Torre, have been experimenting in the Chesapeake Bay with a speaking trumpet attached to the muzzle of a musket and judging of the proximity of objects by means of an echo. The results of the experiments are of a satisfactory nature and may lead to some means for a more practical application of such devices. When the gun was aimed at passing vessels clear echoes were returned up to a distance of a mile, and the interval between the report and the echo would serve as a basis for estimating the distance in the night. A small steam tug, approaching the vessel bow on, produced an echo in answer to the discharge of the gun when at a distance of one-fourth of a mile, although the echo did not have the clearness of those sent back from sailing vessels. The ripples and waves on the surface also gave continuous echoes like the rolling of distant thunder, and the experiments were more satisfactory in calm than in rough weather. As the motion of the water in the open sea is always considerable in comparison with the land-locked Chesapeake Bay, and the large bulk of a steamer would, in itself, reflect sounds, it is probable that in its present form the utility of this ingenious device is, to say the least, unproven; but some application of the principles governing the reflection of sound appears to be the most feasible manner of giving warning of the approach of icebergs and even of other vessels.

THE MECHANICAL TELEPHONE.—A new form of mechanical telephone was shown in operation recently between No. 4 Ludgate-circus and Chancery Lane, London. The apparatus is the device of Messrs. A. A. Knudson and T. G. Ellsworth, and is a modification of the string telephone, with wire for the string (as employed by Mr. W. J. Millar, of Glasgow, some years ago), and plaited willow chips for the diaphragm or tympan. The plaited willow tympan is found to give very good effects. It is shielded by a wooden cover or mouthpiece. The call consists in tapping this wooden cover. In the recent trials the ticking of a watch was heard distinctly across Ludgate-circus., and whispering all the way from Chancery Lane.

AN INDESTRUCTIBLE, SELF-DEPOLARIZING AND CONSTANT EARTH-CONNECTION.—By JUSTIN MALISZ.—A good earth-connection is of great importance for telegraphic purposes, and in the protection of buildings against lightning. The author, after drawing attention to the defects of ordinary earth-plates, describes his method of preparing an earth-connection which conducts well, is self-depolarizing, and has its junction with the line easily accessible.

A hole about 2 meters (6¼ feet) deep, and about 1 meter square, is dug in the ground, and a layer of coke laid on the bottom, and firmly stamped down. A tube with its lower end resting on the coke is placed against one face of the hole, and filled tightly with coke almost to the top. The hole is then filled up, and the earth rammed well round the tube, the upper part of which projects above the surface of the ground. A lump of coke or retort carbon, to which a copper band is rigidly attached by soldering or pouring molten lead over it, is put in the tube, and a final layer of coke pressed firmly around it. The copper band projects from the top of the tube, and to this band the line, or the lightning conductor is soldered.

By this arrangement a constant earth-connection is obtained, as coke has a uniform conducting power under all circumstances, and its large surface ensures for the current an easy path to earth. The column of coke in the tube not only enables the junction between earth and line to be permanently above the ground and easy of access, but it has a depolarizing action on the bottom layer of coke, thereby preventing the prejudicial effect of a counter electromotive force, which in so many cases renders an ordinary earth-plate useless.

The author considers that the coke, which, in the upper part of the tube is in contact with the air, condenses oxygen, and can likewise absorb and oxidize the hydrogen formed by the current. The gases resulting from polarization rise through the column of coke in the tube, and are dissipated in the air, and this column in virtue of its power of condensation plays an important part in the depolarization of this form of earth-connection.

CITY WATER WORKS SYSTEM UNDER PRESSURE.—At present these consist of twelve miles of street mains, supplying 1,500 consumers, or 6 per cent. of the population. There are 139 hydrants, 9 free spills, and 6 drinking and 1 public fountain. The cost to January 1st, 1885, has been about $300,000, while the annual income is now $15,000 per annum. While supplying 1,500 consumers, there have been occasions, during very hot weather in August, when for some hours, the draft on these pipes has been at *the rate of* 1,000 *gallons per head per day*. Street watering accounts for some 100,000 gallons daily; fountains for a considerable amount besides. *but the great bulk of this flow goes for irrigation*. The character of City Creek water is such, Mr. Ottinger informs me, that meters cannot be used at all, owing to quick corrosion, and so great is the effect of the sulphuric acid in this water upon brass, that unless the stop gates are moved at least once every 40 days, they become set and require to be taken out and relieved, so as to be worked at all. It is evident that no matter what other methods may be adopted for conducting water for irrigation, a very large amount will always pass through these pipes and be devoted to that purpose, but it is presumable that some method will be adopted to get payment therefor. When the time arrives, as it will, that the waters of Utah Lake shall be brought to the city, *for household purposes*, by covered conduit or pipe, all these difficulties will be obviated.—*Abstract of Report of C. S. Stevenson*.

AT a recent meeting of the Berlin Physical Society, Dr. Kalischer, described a new secondary battery, intended to overcome the disadvantage of the usual accumulators—namely, that the sheet of lead used as anode was very soon destroyed. This object he is said to have attained by adopting a very concentrated solution of nitrate of lead as electrolyte, and iron as anode. The iron, on being immersed in the solotion of lead, became passive, and resisted every corroding effect of the fluid; in other respects the peroxide of lead on the electric charge became deposited at the anode as a very firm cohorent mass, enveloping and protecting the iron on all sides. The charge was continued till the greater part of the nitrate of lead was decomposed, a condition which was marked by the occurrence of a greater development of gas at the anode. At the beginning of the charge all development of gas must be avoided, as otherwise the peroxide of lead, or, more correctly, the hydrate of peroxide of lead, became covered with bubbles. As cathode a sheet of lead was used, but it was attended by two disadvantages. In the first place, the lead. during the charge, separated itself at the cathode into long crystal threads, which soon passed through the fluid and produced short closing (of the current). In the second place, the nitrate acid, which remained in the fluid after the separation of the lead, acted very powerfully on the sheet of lead. Both disadvantages Dr. Kalischer avoided by amalgamizing the cathode. This accumulator of iron, concentrated solution of nitrate of lead, and amalgamized lead yielded, after the electric charge, which could be carried out without any special preparations, a current of about 2 volts; after about six hours' discharge, however, the electromotive force sank to 1.7 volt, but, on the battery being left to itself for twenty-four hours, it became a little increased. According to the measurements

hitherto taken, the functions of this accumulator were satisfactory. An attempt to substitute sulphuric manganese for nitric lead in this battery did not answer the purpose, as the peroxide of manganese separated itself, not in a continuous layer, but in loose scales.

ANTI-FOULING COMPOSITION.—A competitive trial of some interest has recently been carried out by the naval authorities in order to test the comparative values of two anti-fouling compositions. They are respectively the inventions of Lieutenant-Colonel Crease and Mr. Sims. The trials have extended over a considerable period of time, having been commenced on the Mercury in 1882. In that year the port bow and starboard quarter of the Mercury received three coats of Colonel Crease's protective composition and two coats of his anti-fouling paint. At the same time the starboard bow and port quarter of the same vessel received two coats of Mr. Sims' protective composition and three coats of his anti-fouling mixture. There were thus five coats on the whole of the ship. The difference in cost is said to have been £50 less on the part allotted to Colonel Crease. Upon the ship being docked at the end of twelve months it was found that the part covered by Colonel Crease was generally in good order, whereas the Sims division of the ship required repainting. This was in January, 1883. It was then determined to have another trial, and the Sims division having been scraped clear, two coats of both the protective and anti-fouling compositions were laid on. The Crease part of the bottom was washed only and one coat of protective and one of anti-fouling paints were laid on. The difference in cost this time was £70. After about nine months the ship was docked again, when the results were found to be somewhat similar to those above described. A third trial was then decided upon, and a coat of varnish was applied over the Sims protective composition before the anti-fouling mixture was laid on. The final results appear by the reports to be decisively in favor of Colonel Crease's method.

PROTECTING WOODEN BUILDINGS.—A very simple method of rendering wood factory buildings of greater resistance to fire, consists in filling the spaces between the studding with a grout made of sand, lime, and a large proportion of sawdust, mixed with sufficient water to flow slowly; it becomes quite hard, is a poor conductor of heat, and will not ignite although it is charred by exposure to an intense fire. This applies to a building already constructed, where it would be a difficult task to remove the sheathing, or lath and plaster already on the inside walls. Where the studding is already exposed on the inner side, the space is frequently filled with brick, masonry, or large tiles made for such purposes. A new material made for such purposes in America is called terra-cotta lumber, and is composed of top clay, which overlies the firebrick clay, mixed with equal or double quantities of sawdust. Every vestige of the sawdust disappears in firing, leaving the tiles very porous. Its use is not limited to filling walls, but it is applied to other purposes of construction where refractory materials are desired, as for short joists between iron floor beams, roofs, coverings to iron columns and beams, sheathings for internally fired boilers, and steam pipes. Small cylinders of this material are arranged with suitable coverings, filled with petroleum, and used for torches. Nails and screws can be driven into it, and it can be cut to dimension with edge tools as desired.

A NEW PROCESS OF BLEACHING.—Mr. Charles Toppan, the American chemist who has been identified with many discoveries in the later derivatives of petroleum for surgical purposes, as cerates, petroleum jellies, and certain antiseptic preparations which are widely used in hospitals and in private practice, has recently been applying, on a large scale, a certain solvent of vegetable gums, which is composed mainly of petroleum products, for the purpose of bleaching cotton goods. At the bleachery devoted to this process, a number of tons of goods are finished daily, and the results are of the most favorable nature, the luster of the goods being excellent, while the action of the solvent being limited to the removal of the gummy matter, the strength of the fabric is not impaired, and the loss in weight averaging ½ oz. to the pound. It has also been applied in an experimental manner to numerous fibrous stalks, as flax, ramie, china grass, sisal, esparto grass, certain of the cactus family, and even the stalks of the cotton plant. In the manner of its application the material is boiled in the solvent, and afterwards treated with the "chemic," or mixture of chloride of lime and acid, in the usual manner. With the thick stalks mentioned above, a certain amount of attrition is necessary during the final washing. It has also been used to deglutinize silk cocoons, the principal value of its application being in its use on perforated cocoons, where in leaving the chrysalis for the moth state, the grub cuts many of the filaments and destroys the silk for reeling purposes. Reeling of silk is commercially successful only in tropical countries where labor is very cheap, but the processes of spinning damaged cocoons is akin to those carried on in the spinning of worsted yarns, and carried on in countries where organized mechanical handicraft may be obtained.

THE New British Iron Company, which, bye-the-bye, is one of the oldest iron manufacturing companies in this country, has awoke to the fact that the printing machine may in various ways facilitate the communication from manufacturer to consumer of information, and that the latter will not hunt up for himself when there are so many ready to supply it. Old reputations are not sufficient to make new consumers run after those possessing them, and hence, the New British Iron Company has just published a well-executed catalogue of its manufactures in iron and steel. This company has always produced first-class irons, and its brands, Lion, Corngreaves, and Ruabon, are known to thousands who do not know to whom they belong. The company now makes steel by the open-hearth process in six different grades, and no doubt will acquire the high reputation which it has held for iron manufacture. The New Iron Company was established in 1825.

A paper on "Zinc in drinking water" is given in the *Journal of the American Chemical Society*, by Dr. F. P. Venable. It has long been known that zinc dissolves in water, and that soft water, such as rain water, dissolves it more easily than hard water. Water containing carbonic acid is specially able to dissolve it. The use of galvanized iron for pipes and tanks being so much on the increase, the subject becomes more and more important, and it is desirable to ascertain, as for as possible, to what extent solution of the zinc coating takes place, and how far water contaminated by zinc is injurious to health. The author quotes several investigators as to the latter point, the evidence being to some extent conflicting, but giving a very decided balance on the one side of the view that such water is considerably injurious. Investigations made on behalf of the French Government resulted in the prohibition by the Ministry of Marine of the use of galvanized iron tanks on board of men-of-war. Professor Heaton has given an analysis of a spring water, with a further analysis of the same water after it had traveled through half a mile of galvanized iron pipe. It had taken up 6.41 grains of zinc carbonate per gallon. Dr. Venable gives the results of an observation of his own, where spring water passed through 200 yards of galvanized iron pipes to a house, and took up 4.29 grains of zinc carbonate per gallon. It seems pretty clear that drinking water should not be allowed to come in contact with zinc.

Rainfall of Salt Lake City.—The following table of rainfall is compiled from records kept at Fort Douglås from 1865 to 1875, and from that date to January 1st, 1885, from the U. S. Signal Office in this city:

Table No. 1.

Years.	Ins. Rain.	Years.	Ins. Rain.
1865	15.51	1875	21.07
1866	22.29	1876	18.31
1867	26.14	1877	14.52
1868	17.25	1878	17.86
1869	22 32	1879	13.11
1870	20.96	1880	10.94
1871	23.12	1881	16.88
1872	18.12	1882	16.00
1873	17 37	1883	14.24
1874	19.55	1884	17.52

Mean of 20 years....................18.15

From this table we learn, that, for twenty years past, the average annual rainfall in this valley has been 18.15 inches: and the smallest amount recorded, 10.94 inches, in the year 1880. From the same source it is also ascertained, that the average summer rainfall, or rather that which occurs between the middle of May, and middle of September, has been 3.31 inches. Since we cannot predicate the available water, upon any other than the delivery of a very dry season, that of 1880, the driest known for 20 years, has been adopted as the basis for obtaining the least fall of rain upon the various drainage basins.

With the view of improving the harbor, the Ayr Harbor trustees have resolved to pile the north wall for a distance of 300 ft. at a cost of £2,000.

Fireproof Doors.—The most efficient fireproof doors are wood covered with tinned iron. The door is made of two thicknesses of tongued and grooved boards, crossing each other diagonally and thoroughly nailed together. The sheets of tin are bent over at the edges, forming locked joints as in a tinned roof; it is important that the edges, as well as the sides of the door, be covered, as its resistance to heat lies in the fact that the fire cannot burn the wood thus protected against exposure to the air, nor can it warp it, as is the case with an iron fire-door subjected to slight heat. If a fireproof door is hung on hinges, especial care must be taken to insure their security by fastening them to the door by means of bolts, rather than screws, and connecting them to the wall in an equally secure manner. The latches should be selected with a view to durability, as such a heavy door is apt to be destructive of weak latches Where the position of the doorway permits sliding doors, it is preferable to have them on tracks, care being taken that the cleats be placed on the floor each side of the doorway, so as to secure the door at its lower corners when shut. In the Boston Storage Warehouse, U. S., there are a large number of such doors in the fire walls, arranged to close an electric circuit when they are all shut, and the fact is recorded on the paper dial of the watchmen's clock at certain intervals. Fireproof doors are frequently arranged to close in advance of a fire by means of the yielding of the alloy fusible at 160 deg. Fahr. The track upon which such a door is hung inclines about 1 foot in 8 feet, and the door kept from closing by means of a round stick about 1 inch in diameter, which reaches from one edge of the door to the opposite side of the door frame. At the middle the stick is cut in two diagonally, and a ferrule made of two pieces of thin copper soldered together longitudinally with the fusible alloy, covers the joint in the stick. When this ferrule is exposed to a temperature of 160 deg. Fahr., its yielding causes the ferrule to split open, and the stick separates into pieces and allows the door to shut. In order that the stick shall not fall in the way of the door, and that the door may be shut at any time, the stick is connected to the top of of door frame by small chains near to each end. This simple device was designed by Mr. Lewis T. Downes, president of the What Cheer Mutual Insurance Company. Another method of utilizing this fusible alloy to close fireproof doors and shutters, is by means of a wire extending around the room, and containing in various places links made of two pieces of brass soldered together. When the solder melts and allows the two pieces of brass to separate, the wire allows the shutter or door to close. Mr. Frederick Grinnell has improved the ordinary link by cutting a slot in one of the pieces of brass, and laying a short bit of wire therein, when they are being soldered together; the solder flowing around this wire presents a resistance in three planes, in place of the ordinary joint, which may be imperfect and lies in a single plane, concealed by the sheet brass so as to prevent inspection. Formerly, solid links of fusible alloy were used, but the metal has so little resilience that it is apt to gradually lengthen, and finally break at some inopportune time.

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THE RELATIVE VALUE OF TIDAL AND UPLAND WATERS IN MAINTAINING RIVERS, ESTUARIES, AND HARBORS.

By WALTER RALEIGH BROWNE, M.A., M. Inst. C. E.

Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Mr. Walter R. Browne wished to say, before the discussion commenced, that the current meter used was exhibited on the table; and Mr. Shaw, who had designed it, was present, and would be happy to answer any questions as to its construction, or as to any practical details in carrying out the experiments. Another remark which he desired to make, had reference to a passage he had quoted from Mr. Richardson:—"If any one will look carefully at the different streams in the neighborhood, he will see little streams with little estuaries, medium streams with middle-sized estuaries, and big streams with large estuaries; the channel formed being always in proportion to the amount of fresh water." In a valuable note upon his paper, which had been sent to him from Burmah, Mr. Robert Gordon, M. Inst. C. E., while in a great measure assenting to Mr. Browne's conclusions, took exception to that passage, and said it was contrary to his own experience, and to the facts with regard to the rivers in the East, and also on the west coasts of Scotland and Norway. He would not go into the question of the rivers of the East, with which he was not acquainted; but he was desirous of pointing out that Mr. Richardson's remarks and his deductions from them, only applied to rivers that ran into the sea through flat alluvial lands—the only cases where there was much mud, and where the question of tidal scour came into prominence. In such cases as those of the glens of Scotland and the fiords of Norway, the outline of an estuary was simply the contour line of a particular level in the old valley, which valley had been scooped out by ice, or by subaerial waste, or by whatever agent geologists might finally agree upon as that by which valleys were excavated—that level being what happened to be high-water level at the present day, and having no relation to any question of scour. In fact, in all such cases the waters were perfectly clear both outside and inside the fiord, and there was practically no scour at all.

Mr. W. Shelford was under the impression that the paper was founded upon erroneous premises and imperfect experiments, and, as the conclusion was condemnatory of the whole modern school of engineering, it was revolutionary. The author's first line of argument was that the silt which tended to choke up tidal channels was almost wholly due to tidal water, and not to the fresh water; and many others of that opinion; but he

would state why he differed from them. It would be admitted that such rivers as discharged into tideless seas, like the Nile and the Tiber, delivered whatever material was in the waters into the seas, and formed deltas, such deltas having existed for centuries, that of the Nile being 100 miles wide, and that of the Danube 50 miles wide. The author had referred to the Parret, and stated that the mud near Bridgewater, from which Bath bricks were made, came, not from the watershed of the Parret, but from a cliff of sandy clay below. Mr. Shelford did not know the Parret sufficiently to be able to contest that point, but he was well acquainted with the Humber, and was aware that it was one of the best tidal rivers, discharging the largest amount of fresh water of any river in England, and also being the most muddy river in England. For years the origin of the mud had perplexed many minds. It had been argued that it came from the degradation of the sea coast between Spurn Point and Flamborough Head, a length of 30 miles, where the wasting of the cliff was very great, averaging $2\frac{1}{4}$ yards in width per annum. His own belief was that the mud came from the watershed of the Humber, which was 10,000 square miles, and that the rain falling upon that watershed brought down with it a quantity of deposit, depending upon the volume of fresh water, into the estuary, and it could go nowhere else. The coast degraded by the sea was 30 miles long by $\frac{1}{4}$ mile wide, say, $7\frac{1}{2}$ square miles as against 10,000. Besides, all the material derived from the cliff could not enter the Humber, because it was first scattered about the sea, and a great part of the water passing the cliff never entered the Humber at all. Then it should not be forgotten that the rate of transport of silt, or of matters in suspension in a tidal channel, was much less than that of a fresh-water channel into a tideless sea. The transport of such rivers as the Nile and the Danube, which contained large quantities of silt, would be found to be at least a hundred times that of the transport of a tidal river. He founded his observation upon experiments made by Mr. Bateman, Past-Pres. Inst. C. E., of the progress of sewage in the Clyde, which he believed was at the rate of 5 miles in a fortnight; and that had been confirmed by observations subsequently taken in the Thames. The material which came down from rivers discharging into a tideless sea was, in a tidal estuary, carried backwards and forwards, and could find no rest. At the mouth of the Humber and of the Thames there was no delta, while at the mouth of rivers discharging into tideless seas there were large deltas; and the obvious conclusion was that the material, for the most part, traveled backwards and forwards. Some of it, no doubt, was deposited on the sides; that was the case with the Thames and the Humber, where the whole width of the estuaries had been narrowed considerably within historical periods, but the tidal action was better than ever. To prove that it was tidal action, he might mention that the low water at London Bridge was lower than low water at sea. That could only happen by the action of the tide, and could not take place if it were a low-water river discharging into a motionless body of water, such as the author has described. The second argument of the author was that, on the whole, the tidal water tended to choke the channel, and not to scour it. Before that could be established, some awkward facts would have to be got rid of. The Tiber and the Thames were fairly comparable rivers above Rome and London. At its outfall the Tiber was a river whose function was simply to discharge the fresh water that came down past Rome. Its channel was not passable for a boat; it was only 18 inches in depth, and it was encumbered by shoals thrown up by storms at sea. The Thames, on the other hand, was a magnificent river. The author did not appear to be satisfied with his experiments on the first day, and therefore, they were repeated six months later—he supposed with the little instrument exhibited—and found some results which were certainly most interesting, if true. They were surprising even to the author, but he had not repeated them; and Mr. Shelford ventured to think, knowing as he did the difficulty of making such experiments with accuracy, that unless they were repeated frequently, there was nothing in them to justify the conclusion which the author had drawn.

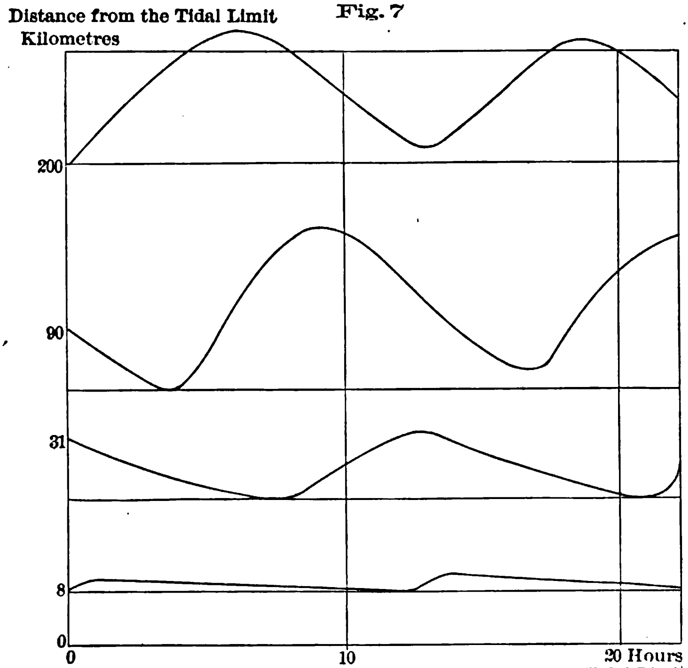
Prof. W. C. Unwin thought it might be interesting to place alongside the meter used for the author's experiments, one or two meters in his possession. One of

them, received from France, was a double Pitot tube, and had been employed by Darcy and Bazin in the most celebrated series of researches ever made on the flow of water in channels; it was, however, only applicable in channels of moderate depth. He had also one of the latest forms of German current meters, which might compare with that adopted by the author. It could be used in the same way as the ordinary current meter. It had a screw fan and a counter; it could be put on a staff, and had a rudder for directing it in the direction of the current. There was, however, one difference from the ordinary meter. The great weakness of the English form of meter was the disengaging arrangement for putting the counter into and out of gear. The meter to which he had referred, had a double ratchet arrangement. Alternate pulls, of whatever strength, put the counter into gear and out of gear. A second defect in the English form was that it had to be taken out of the water for every observation, which involved a great waste of time. In the German meter, that difficulty was got over by an arrangement, which might be used or not, of an electric bell, which sounded at every hundred revolutions. The meter could be sent down in the ordinary way, connected with the battery, and while it was under water the bell sounded at every hundred revolutions. It was only necessary to have a stop watch, and to note the time, say, at every two hundred or five hundred revolutions, and the observations could be repeated as often as was required without moving the meter. A third defect in the ordinary meter was that it could only be used on a staff, and that limited its use to comparatively moderate depths; at all events, its use in great depths was extremely inconvenient. The German meter could be fixed on a universal joint and suspended from a wire. It had a peculiar form of rudder; it was perfectly balanced in the water, and it had below a large weight of 60 lbs. There was a rather ingenious crane by which it could be lowered, and which had a further function of registering the depth of the meter. In addition there was a contrivance for using the meter for sounding, an electric signal noting the arrival of the weight at bottom. Professor Harlacher, who had made more experiments on the screw meter than anyone else, had effected some further improvements in the instrument. He objected to the free swinging of the meter, and he had shown some methods of getting over that objection in a work to which he need not further allude.

On reading the paper it seemed to him as though the author thought it desirable that accepted theories should be occasionally attacked, and that it was well to be forced to look carefully into the grounds on which the opinions of engineers were founded. But there were assertions in the paper which could hardly be supported. When he found it stated that all channels which had no upland waters completely silted up; that it was a well-known fact that all ordinary rivers kept a constant regimen; that silt was deposited most thickly in the deepest part of the river, in spite of the fact that in the deepest part of a river the velocity was greatest; when he was assured that in very deep rivers the bottom velocity was the same as the top velocity; and when, in support of that statement, there were quoted the experiments of Humphreys and Abbot on the Mississippi, although those authors believed that they had established the fact that the vertical velocity curve in a river was a parabola with a horizontal axis at one-third the depth from the surface, so that the bottom velocity must be less than the top; when he was told that for maintaining the channel the entrance of tidal water could be nothing but an evil; he was forced to conclude that the author had been somewhat rash in his assertions. There was a mathematical calculation, in which the square of the velocity of the river was made proportional not to the slope of the river but to the depth of the river. He could not help thinking that the author had been pushing to an extreme his argument for a cause which was not a very strong one. The title of the paper was, "On the Relative Value of Tidal and Upland Waters," but throughout the whole paper no mention was made, except in one paragraph, of upland waters. The author had dealt with what he called the low-water flow of the river. Now, the low-water flow and the upland waters were totally different. It was desirable to understand exactly what the low-water flow of a river was. For this purpose reference might be made to Mr.

D. Stevenson's book,* where a diagram was given of the tidal wave at two points 9 miles apart, in the Dornoch Firth in Scotland, the first point being where the sea wave was almost unaffected by the obstructions of the river, and the second where the obstructions had shortened the time of the flow and increased the time of the ebb. The diagram showed a perfectly flowing curve from the top of high water, and there was no time at which the low-water flow did not contain a large proportion of tidal water.† During the last month or two he had become acquainted with some researches on the Elbe, and he had drawn from a paper of Herr Löhmann four tidal waves observed in that river (Fig. 7); the first was eight kilometers below the point where the tidal influence absolutely ceased, the next at 31 kilometers, the next at 90, and the next at 200. In all those curves the low-water flow consisted chiefly of tidal water. But engineers were not restricted to the use of current meter observations in determining the exact quantity of tidal water at the different sections of a river, and Herr Löhmann had carried out a remarkable investigation of the flow of water in different sections of the Elbe without any use of the current meter. At a series of thirty or forty stations along the length of the river, there were observers, noting at the end of each hour the height of the river, and thus curves of the water surface were obtained for each hour up to the point where the tidal influence ceased. Suppose two of these curves plotted, say, for twelve o'clock and one o'clock, it was obvious that past any section there had flowed into or out of the river during that hour a volume of water equal to the space between the river surface curves above the section + the upland water if the river was falling, — the upland water, if the river was rising. Herr Löhmann had drawn the curve of the water surface of the river for each hour of one tide, and from that he had calculated the quantity of water flowing past different sections at each hour. The results were plotted in Fig. 8. The horizontal line represented 140 kilometers. The minute space between the

Fig. 7

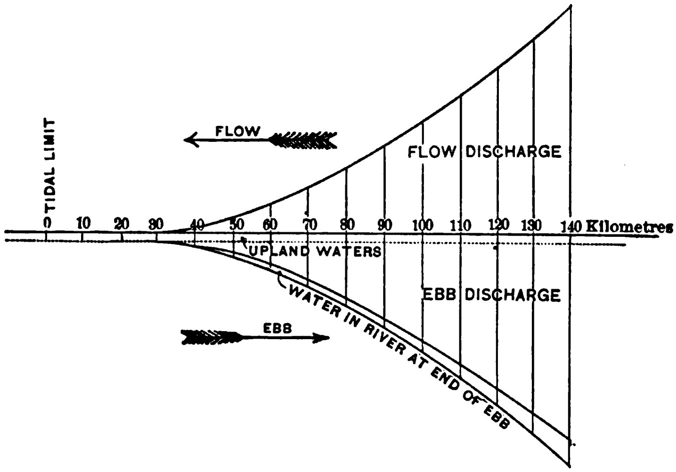


Tidal Wave of the Elbe.

* *Vide* "The Principles and Practice of Canal and River Engineering," 2d edition, 1872, p. 62.

† This was perhaps stated too absolutely. It would not be true of portions of the river near the limit of the tidal action.—W. C. U.

Fig. 8.



Volumes of Flow for one Tide.

black line and the dotted line represented the quantity of upland water flowing down the river. The quantity of tidal water passing each section was represented by the ordinate of the upper curve for the flood, and of the lower curve for the ebb. Between the two small curves at the bottom was the quantity of water remaining in the river at the end of the ebb. Comparing the enormous quantity of the flow at almost all sections of the river during the flood and during the ebb with the minute quantity of upland water, it would be tolerably obvious that throughout the greater portion of the tidal part of the river the tidal water must have a much greater influence than the upland water. But Herr Löhmann was not quite satisfied with the curves, and he had tested the thing in another way. He had calculated the quantity of water flowing in one hour for each meter breadth of the river, in percentage of the upland water. As the river approached its mouth it got wider, and with upland water alone so wide a channel would not be maintained; still it might be useful to find what the quantity of the flow was for each part of the river, and Herr Löhmann had given the necessary data.

That seemed to show that not only the total flow of water, but the scouring action per square foot of bottom, steadily and largely increased from the point where the tide ceased downwards to the mouth of the river. To this the author raised the objection that the silt in the

Mean Hourly Flow per Meter Width of River at Different Sections, in Fractions of Upland Water.

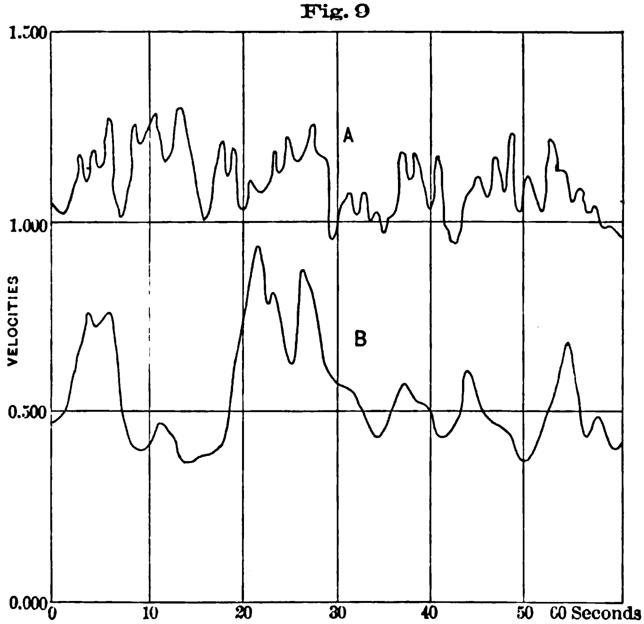
Distance from point where tide ceased	—	50	80	110	Kilos. 140
Ebb..............	1.0	1.06	2.41	3.57	3.82 "
Flow..............	—	1.10	2.59	3.97	4.40 "

river was brought up by the flood-water, and he referred to the fact, that while river water was comparatively clean, the tidal water going up and down was very muddy. The greater muddiness of the water in the estuary, compared with the water in the river above, merely proved that the tidal water had very large scouring power—it had scoured the bottom of the river much more, and had become much more muddy. It still, therefore, remained a question, what became of the muddiness of the water in the tidal compartment of the river? There was no doubt that the tidal water coming into the river was clean sea water, and the water going out into the sea was no longer clean sea water; it was not only muddier than the river water, but it was very much greater in volume; therefore, the muddiness in the estuary was, he thought, mainly carried out to sea. But the question was one which required a different investigation from any then before the members. The author had quoted a number of instances where, on the shallow shores of estuaries, and in the case of the Parret on

specially prepared shelves, there was a deposit of silt. But he had ignored the fact, that in one section of the river there might be going on at the same time a scour in the middle part of the channel and a silting up on the sides. He could not find any case mentioned in the paper which might not be explained in that way.

There were one or two points with reference to the experiments to which he should like to allude. There was, he thought, throughout the paper a misconception about the action of the water in a river—that at a given point there was a definite velocity; for example, at 6 feet below the surface and at 20 feet from the bank. He believed that was an absolute delusion. The motion of the water was, for theoretical purposes, often treated as if it were motion in plane layers at a steady velocity; but the actual motion of the water was immensely more complicated. The amount of viscous resistance of water was absolutely known, and it had been clearly shown that, if the water moved in simple plane layers, rivers would have some hundreds of times their actual velocity. The fact was, that in a river an enormous amount of work was wasted, and there was an enormous amount of retardation, from eddying motions superposed on the general motion of translation of the water. Prof. Harlacher had tested that in a curious way. He had connected one of the electric current meters with a very delicate recording instrument, like Morse's telegraphic recording strip, and had thus recorded every revolution of the meter. The result was shown roughly in Fig. 9, and it would be seen, from one observation near the surface and one near the bottom, how enormously the velocity varied during a period of one minute. One thing was certain, that the inertia of the meter reduced the fluctuations of the curve. The meter was more or less heavy; and if it were perfectly weightless the curves would vary much more than they did in the diagram. The author stated that it was an obvious fact that the scouring action of any current was solely due to the layers of water in proximity to the sides and bottom. But that statement seemed in two ways in error. The scour was due to the eddying agitation of the water, which did not necessarily depend on the mean local velocity at the bottom of the river. In the next place, suppose the silt to be scoured up by the water and mixed with a body of water, it was then carried down stream, again deposited somewhere, again scoured

Fig. 9



A, meter near the surface; B, meter near the bottom.

up and deposited, till it got out to sea. The rate of transport of the silt down the river depended, not on the bottom velocity, but on the mean velocity, and it would be more rapid in a river which had a high mean velocity, than in a river which had a low mean velocity. The rate of transport down the river should be considered, as well as the rate at which the silt was picked up from the bottom. He thought the author had not been fortunate in his selection of results as to the vertical velocity curve. He had sought to prove that there was a certain ratio between surface and bottom velocity, and for that purpose had compared a number of experiments. He did not like to deal with a question of that sort without first considering it in the author's way, and he had worked out some of the results of Harlacher's observations, which had been carried out with more care than any previous experiments. Harlacher had been testing some sections of the Danube where the depth reached 8 meters, and some sections of the Elbe where the depth was only 2 meters, and he had given the proportion of bottom to surface velocity, which was greater in the deep stream and less in the shallow stream. He did not place much reliance upon the result, but it showed that by taking a small number of experiments, almost any result might be obtained. The author seemed to place some reliance upon the results obtained by M. Révy in South America. They were made, no doubt with extreme care and under exceptionally difficult circumstances, and they resulted in an altogether extraordinary law as to the vertical velocity curve in a river. Révy found that the velocity varied inversely as the depth; but that was contradicted by every other experiment, and it could not be received as a general law.* The author in summarizing the results, had arrived at a conclusion that in very large rivers, the bottom velocity was practically the same as the surface velocity. Not only was this mechanically impossible, but to obtain this law he had discarded M. Révy's result, which he had previously quoted, and he relied on the Mississippi results, which were altogether different; lastly, he relied upon some experiments by Mr. Gordon, on the Irrawady. Those who knew what had been doing in hydraulics would be aware that there was something peculiar about those experiments. Seven thousand series of observations had been sent by Mr. Gordon to the great French hydraulician, M. Bazin, and the only way in which M. Bazin could deal with them was by getting a series of computers to work at them, so as to average them, and then the singular result was arrived at that, as a river rose in the flood, the bottom velocity got higher and higher, and that at last the bottom was greater than the top velocity. He believed the experiments were made with the greatest care, and with enthusiasm; but he thought with M. Bazin, that there were some local conditions which altered the result, or that the method of experiment was one which did not admit of accuracy. It was curious that the two sets of experiments which had given trouble in regard to the theory of rivers were the Mississippi experiments and Mr. Gordon's. Both of them had been made by a peculiar method of using double floats.

The scouring action in a river was attributed by the author to direct action of the low-water stream, and thence the conclusion was drawn that the entrance of tidal water into a river was nothing but an evil. Now there was a way of admitting that the upland waters assisted the tidal waters, which did not involve such a conclusion. No doubt the upland waters did act powerfully in maintaining the uppermost section of the tidal compartment of the river, the only part of the river where the tidal influence was likely to be prejudicial. By maintaining this portion of the channel, part of the tidal reservoir was conserved, and thus indirectly assisted in maintaining the river lower down. But if this was the way in which upland water acted, it still remained a canon of practice in river improvement to facilitate in every way the volume and rapidity of the tidal flow.

Mr. J. B. Redman said that, whatever opinion might be formed as to the deductions drawn by the author, all must acknowledge an indebtedness to him in bringing forward for the first time an ex-

* Probably Bazin's formula was the best expression for the relation of the maximum and bottom velocity

$$v_b = V - 36.3 \sqrt{hi}$$

where, v_b = bottom velocity; V = maximum velocity; h = depth, and i = slope. But obviously it depended on the roughness of the bottom and other considerations, which could not at present be taken into account.—W. C. U.

ceedingly important subject. Accepting the dictum of the author, that the scour of a great navigable tidal river was due to the upland water, and also the second proposition that the silt was brought up from the sea; taking the case of the Thames, where the upland water, in ex treme flood, was one-twentieth of the tidal water, that river, in the course of time, must cease to exist. But the author might take heart, inasmuch as the Astronomer Royal, Sir George Airy, in 1877, advanced a theory that, as the ebb lasted seven hours, and the flood five, the transporting power of the flood was as 7 to 5. Accepting the Astronomer Royal's dictum, the Thames, in the same manner, must cease to exist. He accepted as truthful the author's assumption that the silt in a river like the Thames came from the sea, and he thought that in the case of the metropolitan river, the least observant would see that that must be so. During a neap tide, especially during the last neap tides, the river water within the metropolitan boundary was singularly clear. If the remark were made to a man working on the river, he would say, "Yes, but wait till the spring tides, and you will find that the river is muddy enough." Along the length of the river, from Blackwall to Sheerness on the north, and from Greenwich to the Isle of Grain on the south, might be observed the "saltings," which denoted the range of neap tides, and it would be seen that they were serrated and broken down by the spring tides. The spring tides was, of course, of much greater volume and much higher in altitude in its flow, and it stirred up the bed and muddy foreshores of the river. A large amount of deposit from the spring tide was brought into the upper part of the river, but inasmuch as the ebb was seven hours as compared with five of flood, the ebb removed a large amount of deposit by progressive steps down the river. He thought a somewhat indiscriminate statement had been made, when three rivers were mentioned to prove that the author's supposition was wrong, that silt was carried up the river, namely, the Tiber, the Danube, and the Nile—three tideless rivers, falling into tideless seas. There could not be any analogy between those rivers and tidal rivers. In reference to the large amount of deposit in the river coming from the sea, a few years back he was engaged in an inquiry as to an accumulation of mud and sand in the Gravesend reach of the river. It was alleged that the accumulation arose from the erection of the Terrace pier. On inquiry he found below the town that there was an old military landing place, called the "Ordnance" jetty, used for embarking and disembarking troops; there was also another bridge, called the New Tavern bridge, which, at that time, was silted up to the planks. On pursuing the inquiry, he found the Custom House bridge had succeeded the New Tavern bridge, and that the New Tavern bridge stood upon one that preceded it. The accumulation had gone on since the close of the last century, and amounted to about 3½ inches per annum. It was entirely local in the hollow of the river, and was not only promoted by these bridges, but was also kept by them in position. Higher up the river at Northfleet the shore was cleaner, and at Greenhithe, in front of "Ingress Abbey," the owner, an old sailor, had often remarked to him that the foreshore in front of his estate was frequently temporarily covered with a large deposit of mud. He had himself noticed after a gale of wind blowing up the reach, and impinging on the shore, that the foreshore was furrowed almost like a ploughed field down to the virgin clay, and the mud disappeared; but on a recurrence of calm weather the accumulation again formed. There was a constant deposit from the flood tide, which was dispersed under certain conditions.

He wished to refer to that portion of the paper which spoke of a tidal harbor being kept open more by the upland water than by the tidal water; and he would inquire how it came to pass that such harbors as Harwich, Portsmouth, Yarmouth, and Montrose, had maintained their depths for centuries. Portsmouth had 13½ million tons of tidal water, with a width of entrance of 500 to 600 feet, and a minimum low-water depth of 17½ to 20 feet. Harwich had 60 million tons of tidal water, a width of ¾ mile, and a minimum navigable depth of 17 feet. Yarmouth had 6 million tons of tidal water, a width of 200 to 300 feet, and a navigable depth (since the time of Elizabeth) of 10 feet. Montrose, with 40 or 50 million of tons of tidal water, and a width of a furlong, had a navigable depth of from 8

to 10 feet. There were two rivers flowing into Harwich, two small streams into Portsmouth, and three into Yarmouth; but the amount of upland water was infinitesimal.*

On the authority of Mr. Richardson, it was stated that the estuaries of tidal rivers always bore a certain proportion to the upland water falling into the catchment basin. He had had the curiosity to look at the catchment basins of several of the principal rivers in England. Taking the Thames and the Humber, two rivers fulfilling the conditions mentioned by the author, as flowing through alluvial soil, the catchment area of the Thames was 5,000 square miles; the width at high water was $5\frac{1}{2}$ miles, and at low water 3 miles, and the depth from 30 to 60 feet. The Humber had a catchment area of 9,480 square miles, the width at high water being 4 to 5 miles, and at low water 3 miles, and the depth 30 to 70 feet. The catchment area of the Great Wash, into which the Great Ouse, the Nene, the Witham, and the Welland fell, was intermediate between that of the Thames and the Humber, viz., 5,734 square miles; the width was double, 10 miles, and the depth was very great, 30 to 70 feet. The Great Wash was the channel for the influx and efflux of a vast quantity of tidal water before the works carried out by Sir Charles Vermuyden and subsequent engineers were completed, and when a large tract of fen-land was under water; and the ten miles of estuary represented the power of tidal water to keep open the channel. Before concluding, he desired to direct attention to a few words uttered by the late Mr. G. P. Bidder, Past-Pres. Inst. C. E., in an inaugural address. "Points of the utmost importance, upon which to arrive at a clear understanding, are the relative effects of the scour upon our rivers and harbors, arising from the action of tidal, or of land waters. I am free to admit that I am one of those who attribute the smallest possible effect to the action of land waters, whilst I do attribute the most important effects to the action of tidal scour. This conviction has been forced upon me by a general view of our rivers and estuaries. Looking, for instance, to the river flowing past our doors, the river Thames, can it be supposed that the mere driblet of water falling over Teddington Lock, even in seasons of the highest floods, can give any effectual aid to the tidal currents, in preserving the channel from sea to the Nore, and up to above Gravesend." Notwithstanding, he felt sure the author of the paper, possessing comparative youth, acknowledged talent, and, no doubt, courage worthy of the name he bore, would make an excellent fight to maintain the position he had put forward on that great question.

Mr. J. Thornhill Harrison remarked that the paper opened out so many questions, such as the sources of the silt and sand, whether they were brought from above or below; the measures that engineers had adopted to restrain them in both directions; the improvement of rivers, and the means of scouring uotfalls, that he would merely refer to them, and would go at once to a point which he thought was of great importance. The author had laid down certain rules and principles which deserved serious consideration. Upon what did the author base those rules and principles? So far as could be judged, entirely upon observations made by Mr. Shaw at one spot on the river Avon. But were those experiments sufficient, and were the deductions from the experiments correct? It would be noticed that the point where the observations were taken was $7\frac{1}{2}$ miles from the mouth, and only two miles from the upper tidal limit, whereas they ought to have been made nearer the mouth. Again, the experiments were of a very partial character, were only continued over about three hours of tidal action, and were not taken at any other place upon the river; so that there was no information as to the velocity of the water at any other spot, either at the surface or below it, nor as to the volume of water flowing from the river proper above; nor were there any data as to the fall of the water from the point where the observations were taken to the mouth of the river. He thought that, before rules and principles were laid down, experiments should be made not only at one spot upon

* It must at the same time be allowed that of late years dredging operations had maintained and improved the navigable low-water depths of those harbors, and doubtless this would be increasingly so in future years, from the continued increase in the size of steam vessels in the mercantile marine, and the great improvements developed in dredging plant, and reduced cost of raising and depositing the material, greatly resultant on such works as the Suez and Amsterdam ship canals.—J. B. R.

one river, but at many spots throughout the whole tidal range upon many rivers. The information, however, so far as it extended, supplied some useful hints. He had prepared some diagrams from the particulars given by the author, which showed that, for a considerable time from the commencement of the ebb, the water fell and the velocity increased almost exactly in proportion as a body would fall freely under the action of gravity. He had marked three points, the first period up to 9.55 A.M., when there was some check to the increase of the velocity, and a check also to the rate of fall of the water. After a short time both of them became regular; the fall of the water went on regularly, so much every hour, and the velocity became constant. That period he had marked as the second. Then again there was a third period when the velocity diminished, and when the rate of fall of the water also diminished; that went on to the point where apparently the low-water level was reached, or nearly so; the experiments ought to have been continued not only to low water but beyond it. The diagrams appeared to show that the river Avon at high tide was full of water up to Netham dam, the surface having a very slight fall from the dam down to Avonmouth, and the water had, during the ebb, to fall from its elevated position down to low water. The water could only fall vertically at the Netham dam, there could be no lateral motion; but the vertical motion as the water fell was transmitted horizontally, creating a wave motion, which was passed onwards down the river, and taking any point, such as that at which the observations were made, the quantity of water that would pass that point in a given time must be equal to the quantity that had fallen in that time between Netham dam and the point of observation. It was proved that the whole of the water from the top to the bottom was not put in motion, but only the upper portion to an unascertained depth. The volume of water in motion must increase gradually from Netham dam downwards, and, supposing the velocity of the current to be nearly the same throughout the course of the river, the water in motion would take the form of a wedge, whilst the water below this wedge would remain at rest; and so long as there was sufficient depth of water for the generation of the wave, the conversion of the vertical fall into a horizontal motion would be quite free, the motion between the particles of water being almost free from friction. Mr. Shaw's experiments showed that for a period of an hour and a quarter after the tide began to ebb, there was at the point of observation, a steady generation of a wave of translation, as the velocity increased with the time, and the fall of the water increased as the square of the time. But there would come a time when the depth of water in the channel would be so far diminished, and the quantity of water discharged from the river would be so great, that the water would be in motion from the top to the bottom of the channel at some certain point, and then the free generation of the wave would be arrested. That appeared to be the commencement of what he had marked as the second period when, the free fall of the water being stayed, and the velocity not being increased, there was a steady fall directly as the time, the velocity of the water at the moment of the observation continuing constant. The commencement of the third period appeared to be when the tide had so far fallen that the water was in motion from the top to the bottom. That the ebb tide created a wave in the way described was, he thought, proved satisfactorily by the fact that the low-water level at Glasgow and at London Bridge was frequently lower than it was some miles below Glasgow in the one case, and Sheerness in the other. This showed that there was a pendulous action, and that the momentum given to the water towards the close of the ebb tide was much greater than it would be if it were due merely to the inclination of the surface at low water—the water was in fact forced forwards and carried onwards to a lower level even than the low water at the sea. When there was a weir, like the Netham dam or the Teddington weir, which arrested the passage of the tidal water upwards, the water adjoining that weir must necessarily be in a state of stagnation, from the time the tide reached it until it rose to its highest level and fell again and passed onwards. There could be no motion at that point, except that given by the flow of the water over the weir. As far as tidal action went, if there were no

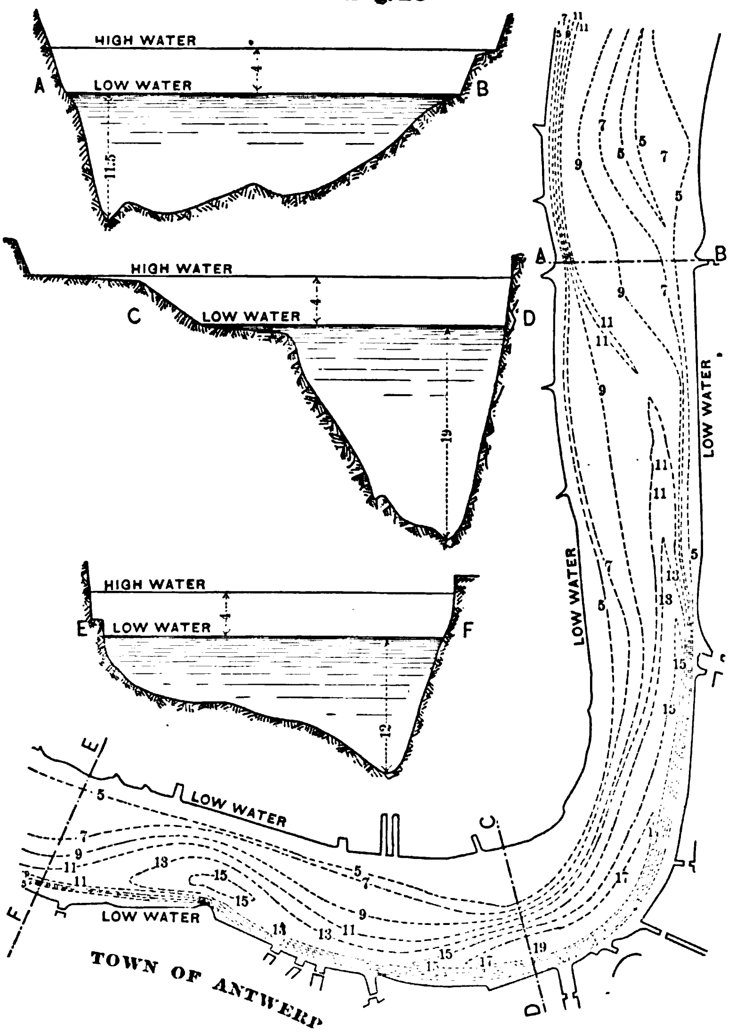
water flowing over the weir the water would be stagnant adjoining it; it would fall downwards, but would have no lateral motion; consequently during that whole period, as the author had pointed out, there would necessarily be a deposit, and this deposit would diminish downwards. To what did that point? That every weir of the kind should be cleared away, and that tidal action should have a free flow as far up the river as possible; it would then pass up the river proper with a rising bed, and the quantity of deposit in the upper portion would be reduced to a minimum. Mr. Shaw's observations did not prove either that the bottom velocity, not far from the mouth of a tidal channel, was *nil* for a great part of the ebb, or that the scouring effect of the ebb tide was little or nothing.

Mr. H. Law had been much startled by the novel views advanced by the author, and the insufficient data upon which, he thought, they were founded. The opening sentence of the paper he could not comprehend. The flow was divided into two parts. There was the tidal flow, which occupied twelve hours in the ebb and flood together; and then the low-water flow, which was strictly defined to be the time which elapsed between the end of the ebb and the beginning of the flood. Now, inasmuch as the ebb designated the water flowing downwards and the flood the water flowing upwards, he was at a loss to conceive what could be meant by the motion of the water at the end of the ebb and before the flood began. Clearly the water must then be stagnant; and it was impossible to maintain, even if there were any short interval, that what was defined to be the low-water flow could produce the whole of the scouring effect in a river, and not the tidal flow, which occupied a space of time much longer, the mean flow over Teddington weir being about one-three-hundred-and-fortieth of the mean flow of the tide. Then followed this clear and precise statement: "In the author's opinion, this assumption [that the tidal flow produced the scour] cannot be maintained, and grounds quite as solid might even be found for exactly the opposite rule, viz., that tidal water should be wholly excluded from a channel, wherever possible." He thought that was a proposition which entirely controverted all the established principles and experience of the profession, and which certainly required a more extensive series of observations than had been adduced in the paper. The instrument which had been used in the experiments had been laid before the Institution. Mr. Law's experience with current meters had been considerable, and he had found after a short time that attempting to take observations in deep water with rapid currents by the old system of withdrawing a detent, which allowed the instrument to revolve and then allowing the detent to fall into gear again, was quite impracticable. If there was any stream and any length of rope out the tension upon the rope alone was sufficient to withdraw the detent and set the machine in motion, and it was impossible to tell whether it was running or not. Then there was the great practical difficulty of having to bring the instrument to the surface to make every observation; and the time occupied to bring it to the surface and lower it again prevented that rapid succession of observations which alone would enable the observer to determine nice differences in the velocity of the current at different depths, and at different periods of the tide. It was that experience, after having tried all the meters he could find, that induced him at last to contrive the machine now exhibited, by which the current could be measured by means of electricity at any depth and continuously. He had found it was necessary, in order to give steadiness to the machine, to have a very heavy bar. Mr. Révy employed one of about 30 lbs.; but Mr. Law had adopted one weighing 90 lbs. The most desirable way of lowering it was by attaching it at each end, and that was done by angle-irons projecting over the sides of the boat at right angles, and the bar was lowered in a perfectly parallel position. The meter was attached in such a manner that it could present itself to the current. Wherever a strong wind was blowing or there was a current on the surface in a different direction, it was necessary that the meter should assume its own position below. He had tried many expedients for insulating the current, and at last that which suggested itself to him, and which was the most simple of all, was that of

placing the insulating apparatus at the top of a vertical spindle, and enclosing the whole in a small diving bell, the air in the upper part of which served to prevent the water ever reaching the insulating apparatus. There being no stuffing box, the meter revolved with perfect freedom. In the air when exposed to a moderate breeze it made six hundred revolutions a minute by virtue of the wind alone. The ball in the center of the screw was hollow and balanced the weight, as in Révy's instrument, and the consequence was it possessed that quality which Professor Unwin said was necessary, a very small *vis viva*, so that it could rapidly change its motion with the varying motion of the currents. At every hundred revolutions the bell was struck and the signal given above. In order to determine the depth there were two cylinders, with two cables wound round them. As the cable might vary in size according to the material used, the cylinders were split in two, and by a simple mechanical arrangement could be adjusted, so that one circumference gave exactly 18 inches of cable. There was then a scale upon the instrument; a guide traveled upon a screw, which at the same time ensured an even winding of the cable, and the depth could be seen by inspection. The number of seconds that elapsed in every hundred revolutions gave the velocity of the current. The instrument was also adapted for sounding. It was quite as necessary to have an accurate section of the river as it was to have the velocity of the stream. For the purpose of sounding, the two angle-irons were brought together over the boat's side, and the two ends of the wire were connected with a sounder which weighed 50 lbs. The moment it touched the ground it struck the bell, and the depth was exhibited upon a scale. It could be then raised a foot or two, and passed to another point, and by getting the signal the depth was obtained immediately. The sounder weighing 50 lbs., and the line being so small, however great the velocity, no deflection took place and it hung vertically. The instrument exhibited was the one with which he had tried a great many experiments, and he must say that the results he had obtained were very different from those arrived at by the author.

Mr. Russel Aitken thought the value to be attached to the author's tidal observations was being lost sight of. The range of tide was about 36 feet, and it was evident that, as the tide rose, water would be found in the bottom of the channel of a greater specific gravity than at the top. A layer of cold water or salter water would probably be at the bottom. The top water would slide on the top of the heavier water, and so soon as the tide got far enough down, and the heavier fell below the meter, it began to turn. It was well known that layers of water having different specific gravities slid upon one another in the most wonderful manner. The Gulf Stream, although it was salter, yet having a higher temperature, was lighter than the waters of the ocean through which it flowed, and slid through the ocean, and so he believed it was in the case under consideration. It would be found that the author's observations could be accounted for in the way he had mentioned. He thought Professor Unwin should not consider water as working, as a general rule, in layers. He believed that when water in a river was of the same specific gravity, and this was generally the case as water in a river got well mixed up, the water above pushed the water below solidly along, and the retarding effect of the eddies caused by friction on the bottom did not cause water to go in layers, but gave it a rolling motion. Until recent experiments the theory that water flowed in layers was generally adopted and acted upon, but this was evidently incorrect. He had represented on a diagram the river Scheldt (Fig. 10) which was full of silt, and was interesting as showing the value of the land water at low water, in extending the low-water channel. The Scheldt went past Antwerp from D to E. It would be observed that at D there was a depth of 19 meters, and the rise of the tide was 4 meters. At AB a little further down there was a depth of 11.5 meters, and further up there was a depth of 11 or 12 meters. At AB the width was the same as that of the Thames at Woolwich, and the tide only rose 4 meters, or 13 feet, as against 18 feet 6 inches in the Thames; yet there was a navigable channel of 26 feet or 28 feet, or not less than 8 meters at low water, whereas in the Thames there was

Fig. 10



only a navigable channel of 14 feet. Of course, in the Thames the bottom was sand and gravel to some extent, whereas in the Scheldt it was fine sand. He thought that a great deal of the depth observed in the Scheldt was due to the exact time at which the water issued from the dykes. The land around the river was situated at a very low level, rather below half tide. The sluices opened a little before low water, and then there was a great current due to the land waters. The river Scheldt was a remarkable instance of how the scour in rivers was assisted by the introduction of land waters at the moment when they could be most effective. There was a general opinion, which he believed to be erroneous, that when water flowed in a river it rebounded from one side to the other. Water, like every other kind of matter in motion, will proceed in a straight line until it met an obstacle, and when it met a bend in a channel it heaped up against the side towards which it went. Therefore at the point below D it would be found that the water was heaped up, and was actually pressing the water further down, and forcing it towards the other side of the river. In every other river, the water changed from side to side, owing, he believed, to the water heaped up on one side pressing

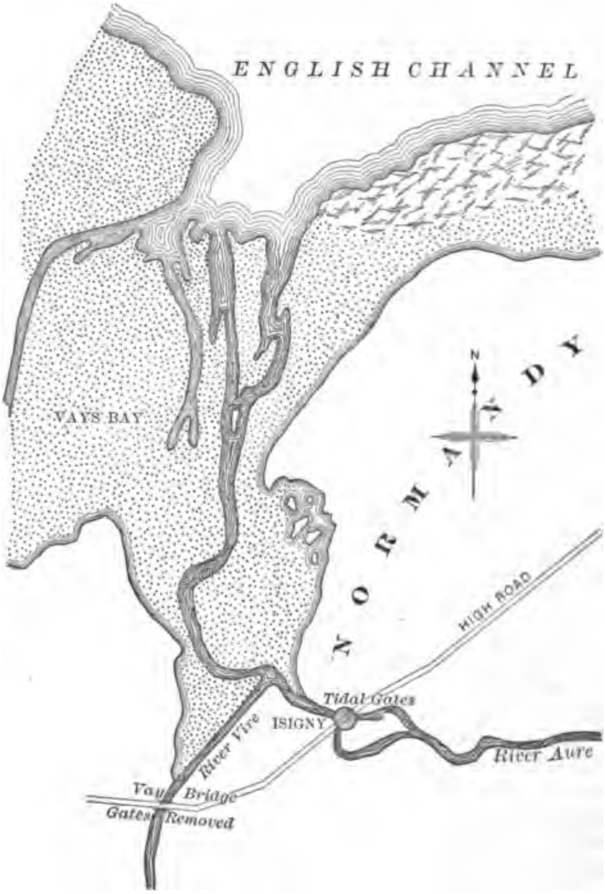
the water back to the other, and it was probably this heaping up of water at the bends which caused the difference of velocities in the various depths of the river, which had been observed by Messrs. Humphreys and Abbot. Professor Unwin had referred to a number of experiments which had been made on small rivers, and had cast a doubt on Mr. Gordon's observations on the Ganges and the Irrawaddy. Mr. Gordon believed that the scour in a river was greatest on the rise of the flood in a fresh-water river. Mr. Aitken thought that statement was correct, and if analyzed it would be seen that it must be so. Take the case of a large river with a gradual fall of, say, 3½ inches per mile, and flowing at the rate of about 5 miles an hour, if the river in flood rose at the rate of 5 inches per hour, then the fall of the river evidently increased by 1 inch per mile during the rise of the flood. In other words, the fall, instead of being 3½ inches, was 4½ inches per mile; therefore there was an increased fall, giving a greater velocity which would of course augment the scouring power of the water. When Mr. Bradford Leslie, M. Inst. C. E., was building the Gorai bridge, over a branch of the Ganges, he had expressed the opinion that the greatest scour took place on the rise of a flood in the river. A cylinder 14 feet diameter and 30 feet long, had been lost on the rise of a flood. It seemed that scour took place with the greater velocity of the rise, and that deposit again occurred during the diminished velocity of the falling flood in the river. If, in connection with the paper, the temperature and the density of the water at different levels had been given, it might have been seen better what was going on. Until that information was supplied, he could not account for the sudden stoppage of the gauge in any other way than that which he had mentioned.

Mr. L. F. Vernon-Harcourt said the author had remarked that the silt which came into tidal rivers was chiefly due to the tidal water. He did not see why tidal rivers should differ in any respect from non-tidal rivers in regard to the silt brought down by the fresh water. In many non-tidal rivers a great deal of silt was brought down. The Mississippi, the Nile, and the Danube, were charged with large quantities of silt, and it ought not, therefore, to be accepted as an axiom that the fresh water of tidal rivers did not bring down silt. No doubt the flood tide introduced a certain amount of silt with it, but a great portion of it was removed on the ebb. There was, however, a great deal of truth in what the author had said, that the tidal water alone of the ebb was not able to remove the whole of the silt brought up by the flood, and that the fresh-water floods did help to scour the channel. But there was another reason why the tidal waters were equally muddy with the fresh waters—the fresh water was constantly flowing past any given point to which it never returned, while the tidal waters were oscillating to and fro, and therefore a portion of suspended matter might pass the same point several times in succession. In the case of the Clyde, for instance, some flotation experiments had been made by Mr. Ure many years ago, to ascertain what happened with the sewage in the river. Some floats were put in and watched day and night from boats, and he believed it took nearly a fortnight for a float to go from Glasgow to the Firth of Clyde. If that were so the whole of the river between Glasgow and the Firth of Clyde had the fresh-water flow of a fortnight mingled with the tidal waters, and therefore it held all the amount of sediment in those 12 miles contained in that flow, and it was totally different in that respect from the fresh-water flow. It was an old proverb that a rolling stone gathered no moss, and he thought it might be considered an engineering proverb that moving water deposited no silt. When tidal water oscillated to and fro there was very little deposit of silt, but deposit began to take place directly stagnation occurred.

The harbor of Southwold had been mentioned by the author as a peculiar case of an entrance being kept open by the ebb and flow of the tide. A reference to the past history of the French and the Belgian harbors on the North Sea, would show that exactly the same thing was occurring; they were kept open by the ebb and flow of the tide, and when the marshy tide-covered lands surrounding them were reclaimed, as they had been by degrees (as at Ostend, Calais, Dunkirk, and other harbors), the en-

trances became partially blocked up, because the sea had a tendency always to close up the channels in forming a continuous line of beach along the coast. The author had also referred to Rye harbor, and did not seem to think it was improved by the blowing up of the sluice. Mr. Vernon-Harcourt believed that sluices in the form of dams had done harm to tidal rivers. There was the well-known case of the Grand Sluice near Boston. In summer the mud was sometimes heaped up 10 or 11 feet against the sluice doors; that would entirely prevent any navigation of a permanent kind going on in the summer, and though no doubt it was scoured out by the floods of the Witham in the winter, it was useless to have an entrance which was blocked up in the summer. There was also the well-known Denver sluice which had been blown up, with regard to which Sir John Rennie had mentioned that the outfall of the Ouse below was much improved during the time the sluice gates were removed. The author said he could find no evidence, or hardly any, as to the bad effect of dams across a stream, and he quoted Mr. Bouniceau, in support of his views. He had a great respect for Mr. Bouniceau, as gathered from a perusal of his book; and curiously enough, though that author agreed certainly with one of the author's premises, he did not agree with his conclusions. He had made a diagram (Fig. 11) taken from Mr. Bouniceau's work, in which he had shown two rivers, the Vire and the Aure. They discharged into the English Channel through Vays Bay, situated between Cherbourg and the mouth of the Seine, and the tide was excluded from the rivers by gates across the arches of the Vay and Isigny bridges many years ago. The two dams were placed there in order to protect the lands from the tidal floods, and it was soon found that the estuary began to silt up, and that whereas vessels were formerly able to go to Isigny, boats of a much smaller tonnage were not able to get up there; and the Vire was also silted up to a considerable extent. Mr. Bouniceau gave the following extract, from the report of a commission of inquiry on the subject, in support of his opinions, in the same chapter from which the author had quoted: "The flood-tide in rising up the Vire brought up a volume of over 3,000,000 cubic yards, which flowed four times a day through Vays Bay. It was therefore traversed by a volume of water amounting to 13,000,000 cubic yards in the twenty-four hours. At the present time it is only traversed by the waters of the Aure and the Vire, which together discharge merely 954,000 cubic yards in a day. It is therefore undoubtedly the Vay bridge which has destroyed the maritime navigation, solely because it has flood-tide gates which have prevented the continual oscillation of the flux and reflux of the tide." There was a further history with regard to those rivers. The authorities were not able to take away the dam at Isigny for local reasons, but they took away the gates from Vay bridge on Mr. Bouniceau's recommendation; and Mr. Bouniceau said that as soon as the dam was taken away the navigation of the Vire was restored to its original condition. That proved plainly enough that the author was not justified in asserting that dams had no effect in reducing the navigable channel of rivers. The author had referred to the question of the embankment of tidal rivers, as a system which the modern school of engineering would condemn and forbid. That was a very difficult question, but he did not think the modern school of engineers were quite agreed that the embankment of rivers should not be carried out. The Seine was a case where embankment had been carried out to a large extent within recent years, with advantage to the river above but injury to the outfall below; and the works on the Meuse below Rotterdam furnished another instance of modern river embankments. He would not then enter into the question, as he had offered a paper to the Institution treating of that subject and of the maintenance of harbors on sandy coasts, but there were several rivers in England, like the Clyde and the Tees, which had been embanked. He thought that the question was not whether rivers should be embanked at all, but to what extent they should be embanked. He considered that the exclusion of the tide from any river would prove fatal to its proper maintenance by causing stagnation and consequent deposit in the summer, and that, on the contrary, the tidal flow should be admitted as far up a river as possible to pro-

FIG. 11.



duce as great a tidal oscillation as practicable at the mouth. He hoped the result of the discussion would be to prove that the author, though right in some of his premises, was not justified in the conclusions at which he had arrived.

Mr. George Higgin remarked that the conclusions arrived at in the paper appeared to be based principally, if not entirely, on the relation existing between surface and bottom velocity. The author appeared to consider that the ratio of bottom to surface velocity increased rapidly as the depth decreased. On a given inclination, therefore, it would follow by this theory that the shallower the water the greater would be the bottom velocity and the greater the scour; hence, the author's belief that the scour of a channel was entirely produced by what he called the "low-water flow." The author supported this theory of bottom velocity by some experiments made by Graeve, on the Oder, by others, by General Ellis on the Connecticut river, and by others again, by Mr. Shaw, under his own directions, on the Avon. He entirely agreed with the opinion already expressed, that these experiments were insufficient to support so startling a theory as that put forward; a theory, it need scarcely be said, entirely contrary to all former ones. He would wish, however, to call attention to the result of the experiments by M. Révy on the River de la Plata. The author appeared to have entirely misconceived M. Révy's experiments, which, so far from proving his theory, proved exactly the contrary. The experiments referred to in the paper, were made by M. Révy, in a place known as "the outer roads," in the River de la Plata, at a distance of about 4 miles from the shore. The river here had a width of

about 30 miles, and the point selected was the deepest part of the channel. The bed of the river from this point to the sea, a distance of 120 miles, was almost horizontal. The experiments were made on the ebb tide. The first day's experiments showed that at this point the bottom velocity bore to the surface velocity, which in this case was the greatest, a proportion of 33 to 100. The depth of the water under the platform was found to be 22 feet. On the following day M. Révy returned to the same platform and continued his experiments; he was surprised to find that with a slightly decreased surface velocity, the mean and bottom velocities were greater than those of the former day, the bottom velocity being on the second day 42 per cent. of the surface velocity, instead of 33 per cent. He ascertained that this increased ratio was due to the greater depth of water, for, whereas, on the first day there were 22 feet of water under the platform, on the second day the depth was 23 feet 8 inches. Other experiments, made higher up the river, in a depth of 50 feet of water, and with a very similar surface velocity to the other ones, gave the bottom velocity as 70 per cent. of the surface velocity. M. Révy's experiments, which were conducted in a most careful and scientific way, led him to lay down several rules:

(1) That with a given inclination the surface velocities varied directly as the depths.

(2) That the mean velocity was an exact proportional between the surface and bottom velocities.

(3) That the mean and bottom velocities increased in a greater ratio than the surface ones.

In fact, M. Revy's experiments seemed to prove that on a given inclination, whilst the surface velocities varied directly as the depths, the bottom velocities varied as the squares of the depths; and that probably at the depth of about 71 feet the top and bottom velocities were practically the same. These conclusions were exactly contrary to those put forward in the paper.

He might state, as a matter of interest in connection with the silt-bearing powers of rivers, that he had ascertained from careful experiments at Buenos Ayres, that the amount of suspended matter carried past that city by the River de la Plata every twenty-four hours, in ordinary states of the river, exceeded 212,000 cubic yards.

Mr. Walter Browne, in reply, said he was quite aware that the paper would encounter a large amount of opposition, and he had no intention of complaining at the way it had been received. Apart from individual criticisms, there were two general lines of argument that appeared to have been taken in objecting to his conclusions. The first was that they were contrary to received opinions; but he presumed that the Institution existed for the progress of engineering, and the progress of engineering, like the progress of all other things, had generally been over the bodies of the received opinions of former generations. The second argument was that his experiments were not reliable.

The remarks of Mr. Shelford might be summed up in one, that the existence of deltas was a sufficient proof that Mr. Browne's conclusions were wrong; because deltas were formed of mud brought down by rivers, and that they were only formed in tideless seas where there was no beneficial tide to sweep them away. Now what was meant by a delta? Simply that a river found its way into the sea by more mouths than one; and what that necessarily had to do with the question of the amount of silt which the river had or had not brought down, was not at first sight very easy to see. The question which really underlays the whole matter was, not what cause which had originally brought the silt into any particular place, but, supposing the tidal flow up and down the river to be taken away, would the silt remain the same, or would it be diminished? and to that question the existence of deltas gave no answer. But Mr. Shelford's argument as to deltas was deficient in its premises. He had rested it upon the allegation that deltas existed in tideless seas, and those only; and he quoted three cases of deltas—the Danube, the Nile, and the Tiber, all of which undoubtedly existed. But he might point out that not far from the mouth of the Danube there were three great rivers, the Dnieper, the Dniester, and the Bug, which flowed into the Black Sea (a tideless sea), but had no deltas. And on the other hand, there were rivers which flowed into

tidal seas and yet had deltas. He might point to the Ganges—all had heard something of the tide flowing up the Hooghly—to the Irrawaddy, to the Yang-tsi, to the Cambodia, and to the Indus, in Asia; in Africa, to the Zambezi and the Niger; in America, to the Mississippi, the Orinoco, and the La Plata. Those were some of the largest rivers in the world, all flowing into tidal seas, and all flowing through deltas. Mr. Shelford further said that rivers could not keep themselves open, because the mouths of the Tiber were shallow, whereas that of the Thames was deep. It was hardly necessary to point out that if a river were split up into several branches, the size and depth of those branches must be less than the size and depth of a single channel would be, supposing the river to empty itself by one channel. Mr. Browne did not say that the fresh water would keep open just such a channel as engineers would like to see, but he did say that it would keep open just such a channel as agreed with the amount of the waters that flowed down.

One or two obvious mistakes had been made by Professor Unwin in quoting from the paper, as, for instance, where he stated that the velocity was taken as proportional to the depth, instead of proportional so the slope; whereas it was really taken proportional to the depth at one particular place, at which the depth was proportional to the slope. Again, he called it absurd to say that silt was deposited most thickly in the deepest water, because in the deepest water the velocity was quickest. Mr. Browne had thought that silt was deposited, not when the water was running rapidly through a channel, but when the water was at rest, and that, in consequence, it would be deposited most thickly where there was the highest column of water from which it might fall. He was not directly answerable for the paragraph noticed about large rivers; he had simply compiled it to complete what appeared to be the facts of the case as far as he could ascertain them, and he still considered that it was substantially true. Professor Unwin had especially objected to Humphreys and Abbot's experiments on the Mississippi, as being supposed to prove that the velocity near the bottom in very large rivers was the same at the top. But Mr. Browne had been careful to say that it was not exactly the same, but that the difference was small. He had since again consulted their report, and he found that the average bottom velocity was about 90 per cent., not of the surface velocity, but of the greatest velocity, which was always at some depth, generally at a considerable depth, below the surface. No doubt in measuring the discharge of a river, that 10 per cent. of loss was a consideration to be taken into account; but it was no consideration whatever in the argument as to scour, because the scour was practically the same, whether a river flowed with a velocity of 5 feet or of 4½ feet per second. His experiments had nothing whatever to do with small differences of velocity, and any arguments based on such differences, for instance as to the meter not being sufficiently delicate, were completely out of place.

With regard to Mr. Gordon, who, as Prof. Unwin had said, had made many thousands of observations, and was still making them (as he himself knew from recent correspondence), were they all to go for nothing, because they could not be made to fit in to the theories of a French hydraulician, who could never have seen a river bigger than the Loire or the Seine, rivers which, as compared with those where Mr. Gordon had experimented, were nothing more than ditches? He had himself had occasion to compare at great length the formulæ of different hydraulicians on a kindred subject, and the discrepancies and confusion then revealed had brought him, while thankfully accepting the facts hydraulicians had to offer, to place little reliance on their general conclusions or theories. Now, the facts which Professor Unwin had introduced were only two in number. One simply amounted to this, that in the particular cases cited, in the lower curves especially, there was practically little or no time between the lowest point of one ebb and the beginning of the rise of another flood. Every one knew that that was the case in the sea outside a river, and perhaps in a wide estuary; he should have thought that everyone also knew that it was not generally the case any considerable distance up a river. To anyone who, like himself, had been accustomed to work on such a river as the

Avon, where one could regularly count on two to three hours of work at low water, during which time the level practically remained constant, that argument did not commend itself as a common-sense view of the question. The other fact referred to the proportion between the tidal and the low-water flow. It really required no diagram to convince anybody that the amount of tidal water running up and down a river was greater than the amount of low-water flow; in all cases it was much larger, and in many cases a great many times larger, than it was shown in the diagrams, Fig. 4. It was, however, difficult to suppose that Professor Unwin could seriously maintain that the scour of the bottom of a river channel was simply proportional to the amount of water running up and down through that channel. With regard to the suggestion that the velocity at any particular point of a river varied from time to time considerably, that might be true, at least in some cases; but it could not affect his experiments, which gave the average velocity as zero. The only other remark made by Professor Unwin which needed notice was that the bottom of a river might be scoured by the tide, while the sides were being silted up. If that were so, all rivers were tending continually to become narrower and deeper, but he was not aware of any facts proving that that was the case. The whole question was, whether the rise and fall of the tide did, in fact, sweep away more mud than it deposited. Now, supposing that, on the whole, it swept away $\frac{1}{1000}$ inch in a single tide, and thus increased the depth $\frac{1}{1000}$ inch, which was as little as one could well put it at, at that rate in four hundred years the bottom of a river would be lowered 25 feet; and he should like to know what evidence there was that during the last four hundred years the bottoms of rivers had been deepened to that extent by the action of the tide. Mr. Browne's way of looking at the matter was this: There were two forces at work; there was the water which came down from above (he was speaking of tidal channels in general, excluding special cases), and there was the water which went up and down with the tide. The way to find out which of those was doing the work was surely to examine the cases where they acted separately. Now, he knew of no case where a stream ran down into the sea, and where it had become completely silted up and unable to keep its channel open. Nor did he know of a case where the fresh water had been taken from a channel, turning it into a creek, and where (of course, in muddy estuaries or muddy seas) that creek had not begun, more or less rapidly (generally very rapidly) to fill up. What did this mean, except that the tidal water tended to choke up the channels, and that the fresh water tended to keep them open?

He was obliged to Mr. Redman for confirming his views as to the silt coming up from the sea and accumulating on the shores, but he altogether repudiated the proposition attributed to him, that the net amount deposited by the tides and the net amount scoured by the fresh water must be in proportion to their respective volumes. He must repeat that the volumes had nothing to do with the question, nor had any reason been given for supposing that it had. He would not go into the question of the maintenance of Harwich and other harbors, because the facts relating to those cases were not under discussion, but Mr. Redman had saved him all trouble by disproving his own case. He wanted to show that the depth of harbors was due to the amount of tidal water which passed in and out, and two instances adduced were Yarmouth, having 6 million tons of tidal water, and Montrose, having 40 to 50 millions, which had just the same depth; where was the proportion between depth and volume in those cases? With regard to Mr. Richardson's view that tidal channels were, in general, proportional to the upland waters, Mr. Richardson expressly spoke of moderate tidal channels, such as those of the streams falling into the Bristol Channel, not such as the Bristol Channel itself, which was practically a part of the ocean. The large estuaries mentioned by Mr. Redman fell into the latter category. In the paper he had expressly excluded the consideration of large estuaries, on the ground that he had not been able to find any observations concerning them.

A complaint had been made by Mr. J. Thornhill Harrison that the experiments did not extend to a variety of points at which he should have liked information;

Fig. 13

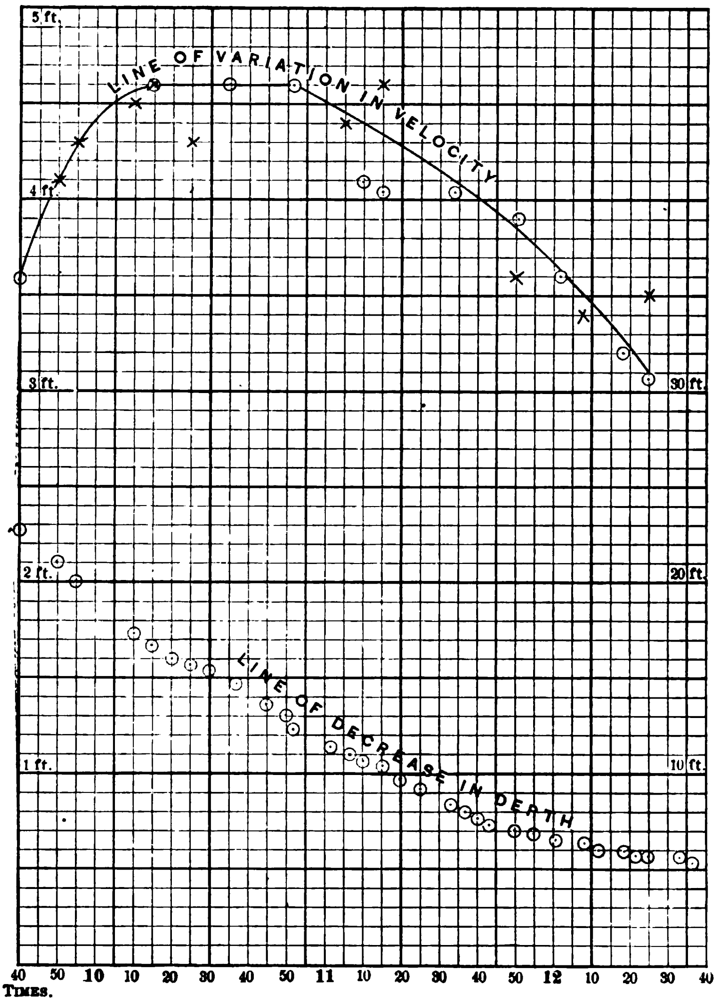


Diagram of experiments on surface velocity in the Avon, September 21, 1880.

N.B.—⊙ denotes velocity determined by meter at surface.

× " " " float, "

but Mr. Browne thought it was better to restrict experiments to the one point to be solved, and which, in this case, was the ratio of bottom velocity to surface velocity, at a certain spot selected, so as to give a fair average result. With regard to the accuracy of the meter, the surface observations for the second series of experiments, both for meter and floats, were plotted together on Fig. 13, and it would be seen that the meter observations were much the more regular of the two. Except for the two observations at 11.10 and 11.15 A. M., which seemed to be too low, they could not be improved. The line drawn was a fair average of both sets of observations, and it exactly answered to the description in the paper. On the same diagram, lower down, he had plotted the observations on depth, and it would be seen that they showed a very steady fall up to about 12 noon, when the rate diminished suddenly, and what he called the low-water period began. He could not follow Mr. Harrison in his views as to the way in

which the water fell in the river. Even at Netham Dam, itself, there must be horizontal motion for the water to get away, without taking any account of the fact that there was fresh water coming over the dam to help it. Mr. Harrison wished to argue that that dam ought to be removed, but that dam was a standing instance of the truth of Mr. Browne's views. There was no doubt that, when put up, it interrupted the tidal flow, and, therefore, on his opponents' principles, it ought to have produced silting up in two parts—above the dam, where the mud brought down by the river would have to settle, and below the dam in the New Cut, where there would be a diminished tidal scour to keep the channel clean. Now there was a good deal of dredging done at Bristol, but it was never done and never required at either of those two places. On the contrary, the places where it was chiefly required were in the half-tide basins, and at the lower end of the floating harbor where the tidal waters entered, and where there were no upland waters to counteract their evil effects. That was a simple fact, to which he spoke from full personal knowledge. Fortunately for Bristol, Mr. Jessup, who constructed the floating harbor and the Netham Dam, had ignored the silting-up bugbear, and his work had been crowned with complete and enduring success.

Some objection had been made by Mr. Law to the term low-water flow, which had already been explained. Mr. Law had also described an elaborate form of meter, which appeared to be valuable, and had mentioned some experiments, of which, as they were not described, it was impossible to say whether they were valuable or not. With regard to the objections taken to the ordinary form of meter, Mr. Law would see, on reading the paper, that none of them applied to the instrument used by Mr. Shaw.

The facts brought forward concerning the Scheldt by Mr. Russel Aitken naturally ranged themselves on his side of the question. He could not, however, agree with the view that differences in the temperature of the water affected the question; in a river like the Avon such differences would be quite insignificant.

With respect to Mr. Vernon-Harcourt's observations, it was, of course, true that some rivers brought down a good deal of silt, but as, unlike tidal waters, they were always in motion, they did not, in general, deposit this silt, at least until they reached the sea, where it often formed sand banks, &c. The idea that the oscillations backwards and forwards of the water in the tidal area kept it more muddy, was, however, a fallacy. The whole fresh-water discharge must get out somehow in the course of each tide, otherwise the river would rise and overflow, and in getting out would carry its silt with it. The case quoted of a sluice near Boston was an unfortunate one, because the fact that the accumulation took place in summer, not in winter, clearly showed that it was due to the tide, and that the scouring power of the fresh water behind the sluice was not used to clear it away. The case of the Vire, mentioned by Mr. Harcourt, had not escaped him. It was a good illustration of the circumstance described in another part of the paper. The tidal waters passing up the Vire, and overflowing a large area of waste land on both sides, greatly increase the flow at the end of the ebb, and when they were dammed off, the bay outside the mouth of the river began to silt up. No doubt that might be prevented to a great extent by judicious scouring, but at any rate it was not a case of injury to a navigable channel, as Mr. Vernon-Harcourt had said, but to a bay or harbor.

A doubt had been expressed by Mr. Atkinson about the expediency of "dockizing" rivers, because he thought it must cause silting both above and below the dam, but experience showed that the former was quite insignificant (at least in ordinary cases), and that the latter would be perfectly dealt with by scouring. Of this the new Avonmouth docks were a striking instance; the entrance lock there (and indeed every entrance lock) formed exactly such a dam, and there was no fresh water flowing through the dock. It was prophesied that the entrance would silt up, but by simply using the impounded water to scour with at low tide (thus creating a spurious low-water flow) it was easily kept clear. If that lock were permanently opened, and the dock left to the mercies of the all-powerful tide, it would be silted up in a year or two.

The case of the Seine was nearly parallel to that of the Vire; but Mr. Shool-

bred's illustration from the Dee was most unfortunate. It appeared that a large quantity of land on that river had been embanked with success, and during that time no complaints were made as to damage to the navigation. But when a further attempt was made, which was a failure, the tide breaking in, and washing over the reclaimed land (as he had himself seen it), then it appeared that the navigation was injured. Whatever might be the cause of that injury, one thing was absolutely certain, that it could not be the shutting out of the tide, simply because the tide was not shut out.

The remarks of Mr. Giles on the Thames at Gravesend, and the Mersey at Liverpool, referred to large estuaries, which had been expressly excluded. With regard to the Clyde, the "correcting" of a river was a totally different thing from embanking it as to diminish the tidal area, as had been done so successfully in the Thames, Severn, &c. The improvement of the Clyde was, no doubt, immense, but it had been achieved by what might be called the brute force of dredging, and that dredging would have to be continued, or the channel would silt up again; whereas, were it turned into a ship canal, as Smeaton had advised (of course on a much larger scale than he suggested), it would keep open.

He accepted, of course, Sir John Coode's correction as to the cause of the travel of shingle along the East coast, which was quite immaterial to the argument. With reference to Mr. Higgins' remarks, it would perhaps have been better not to quote M. Révy, whose experiments were mainly conducted in very large rivers; and in these, the ratio between the bottom and surface velocities appeared to have a character of its own.

CORRESPONDENCE.

Mr. Laroche would confine himself to a consideration of the two alternatives virtually put forward by the author, *i. e.*:

(1) The entire closure of a tidal river by the construction of a transverse dam, so as to shut out the tide from the part above the dam, thus converting it into a floating basin.

(2) The longitudinal embankment of the sides of the river.

In every tidal river with which he was acquainted, if the channel were barred completely by a transverse dam established within the course of the river, *i. e.*, at a point notably above its mouth, as at A A, Fig. 14, the following effects would be produced. If Fig. 15 represented the primitive longitudinal section of the river at low water, this profile would, in a very few years after the construction of the dam, be modified to that shown in Fig. 16. An enormous alluvial deposit from the sea bed would occur at C, tending to reach to the level of low water, and even above it. This would happen below the dam; above it, also, a deposit of fluvial silt would be

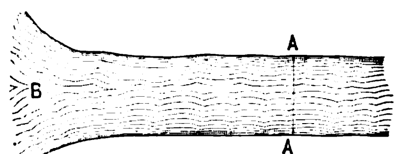


Fig. 14

Fig. 15

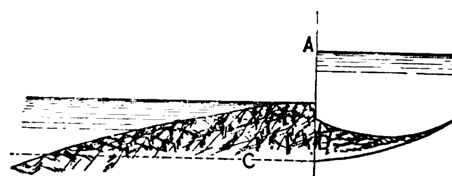


Fig. 16

produced, proportionate to the mean amount of the land water brought down in floods. If, now, it were desired to establish a port above the dam, it would be necessary to cut a channel through the shoal C. To effect this solely by natural action, without having recourse to dredging, the head of water retained by the dam would alone be available. There would apparently be no more efficient way of unsilting the required channel than by allowing the dammed-up water to escape violently at low water. But the effect of this mode of erosion was well known. Water thus escaping soon lost its impetus, the only result being

that when the flushing sluices were opened the deposit was removed from the vicinity of the dam to accumulate at a point more to seaward, as shown in Fig. 17.

If this proposition were as true as he thoroughly believed it to be, it was difficult to see how the condition of a port situated above A could be bettered by the establishment of such a dam. In reality there would have been formed a vast reservoir of fresh water above the dam, but access to it would have been made, if not impossible, at least much more difficult than was access to the river before the dam was constructed.

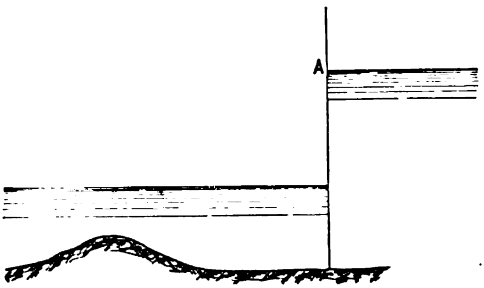


Fig. 17

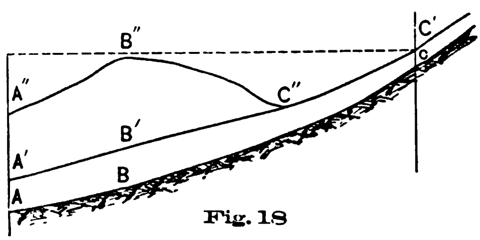


Fig. 18

As regarded longitudinal banks, he would remark at the outset, that of all the important tidal rivers which he knew the land water as a rule formed a very small part of the volume as compared with the sea water, the only exception being the floods brought down at certain known seasons, and for a few days at each season. If ABC, Fig. 18, represented the river bed (A being in the tidal and C in the fluvial part), A′B′C′ the surface of the water at its lowest, and A″B″C″ the surface when the tidal wave, after having expended itself, began to acquire perceptible motion towards the sea throughout the whole depth AA′A″, the whole volume of water comprised between the surfaces A′B′C′ and A″B″C″, added to the fresh water coming down at CC′, would have to escape by the section AA′A″, of which the height AA′ would be constantly diminishing. Now, he had observed in the tidal rivers he was acquainted with the summit of the tidal wave B″ in the river maintained sensibly the same elevation as that of the tidal wave in the open sea in front of the river's mouth. The volume A′B′C″B″A″ would therefore be greater in proportion as the river was wider above the point of low water, A′A″. And as this volume had to pass off at AA″ in a period (generally a little more than six and a quarter to six and a half hours, occasionally seven hours) depending above all on the condition of the tideway below AA″, the speed would be greater in proportion as the volume having to escape (within limits of time of small variation) was itself greater. This volume would also be greater in proportion as the basin filled by the tide above AA′ was greater, that was, as its depth and width above the point of low water were greater. The bottom-velocity augmented rapidly from the time it became sensible, in proportion as the water fell. Therefore the channel of a tidal river should not be reduced above the point A, lest the depth might be decreased at that point; and it followed that the sides should not be embanked. It would even be better to enlarge the capacity of the basin, if regard had only to be had to the question of depth at A.

To sum up, he was not in favor of embanking tidal rivers if it were only a question of increasing the depth of water; for there was too much reason to fear that an opposite result would be obtained. He insisted on the restrictive conditions imposed in the problem as he approached it, because in practice the objects in view were never so simple as he had supposed. The embankment of certain places covered by the tide might be imposed by circumstances altogether foreign to the present subject. He would repeat that his conclusions were solely based on two points observed by himself: (1) That the top of the tidal wave in a river was sensibly the same as that of the same wave in the sea. (2) At a given point in the tidal portion of a river the duration of the ebb depended principally upon the condition of the

tideway below that point, and that it varied within very small limits when the river was in its usual state. It followed that the width of the basin covered by the tide above the point in question had as a rule little influence on the height of the tidal wave or the duration of the ebb tide. The volume of water brought into this basin had, on the contrary, a very great influence on the velocity of the ebb at the point under consideration, that was to say, below the basin. It was evident that where these two conditions did not occur his conclusions would not apply; but these facts had been demonstrated in a certain number of tidal rivers he had observed, and he was disposed to accord to them a probable character of generality.

It might not be out of place to remark also, that if at the same time that the sides of a river were embanked the bottom were dredged and rectified, the disadvantages of the first course of treatment would be largely compensated, and the combination of the two systems would greatly facilitate the propagation of the tidal wave. The level of low water in the upper part of the river would be lowered, and the capacity of the reservoir to be emptied at each tide augmented, to the great advantage of the maintenance of the depth in the lower channel. In a word, if a compensation had been offered for the embankment of the width of the bed by the increase of its depth, and by rectifying its course, such treatment would induce a modification in the conditions at once considerable and advantageous.

ELECTRIC CONDUCTIVITY OF METALS AND THEIR ALLOYS.

BY M. LAZARE WEILER.

Translated from a Communication to the Société Internationale des Electriciens.

IN the study of all electrical problems, and especially of such as relate to dynamic electricity and its applications, we are brought at the outset to the consideration of conduction.

Whether it be a question of light, of force, of heat, of chemical or mechanical phenomena, or, in a word, of any transformation whatever of electricity, it is indispensable to know the quantity of electricity produced at the source, and the amount afforded at the place where it is utilized.

The nature of the intervening conductor becomes then one of the principal elements of the problem.

In the dynamo-electric machine which is only a conductor or a series of conductors subjected to magnetic induction, the property of conductivity is an important one.

The properties, therefore, of different metals and their alloys, as regards their power of transmitting electricity, afford a subject of exceeding interest.

This question furthermore merits our attention now, for the reason that physicists who have heretofore investigated it have failed to arrive at concordant results, either because of the impossibility of obtaining absolutely pure metals, or by reason of the imperfections of the apparatus or the methods for electrical measurement at the date of their researches.

When Mathiessen made his experiments (1863) it was extremely difficult to procure certain metals in a pure state. He mentions the labor necessary to produce 5 or 10 kilograms of pure metal, while now it is a matter of daily occurrence in some manufactories to deposit by electrolysis, metals in a pure state by the ton.

The processes employed in the establishment of the Nordeutsche-Affinerie, at Hamburg, as well as certain works in Belgium and England, lead to the extraction and separation of the metals from argentiferous copper ores, which contain small quantities of silver and traces of gold, not capable of separation by the older methods.

This is profitably applied to ores in which the presence of the precious metals did not formerly affect the commercial value.

Under such conditions it becomes easier and all the more necessary to review and complete former researches. This is

the work that I have been able to undertake by aid of the special facilities afforded me by a carefully fitted laboratory, and a workshop in which are applied on a large scale, processes which were not formerly applied outside the chemist's laboratory.

I shall have the honor to resume before you the researches which I have pursued for some years. The results obtained are far from being complete, but they permit us to closely examine certain points of the theory of a subject upon which our ideas are not yet conclusively formed. They afford in all cases useful corrections to the published tables, which are for the most part afflicted with errors.

This double claim is, I trust, sufficient to engage your kind attention.

Researches relative to the conducting power of different bodies have been pursued only in recent times. Priestley was the first to attempt the determination of the conducting power of metals for a static charge of electricity.

There was no precision in his method of comparison. He employed two wires of the same length and diameter, but of different metals, and sent charges through them until one was fused. The fused wire was considered the poorest conductor.

Davy applied the same general method with voltaic currents, and succeeded in establishing the law relative to the dependence of electric resistance upon the length and cross-section of the wire.

M. E. Becquerel, who has written a memoir on the origin of these researches, gives the table of relative conductivities, as measured by his father, who was the first physicist to engage seriously in this labor.

The table is as follows:

Copper	100
Gold	93.60
Silver	73.60
Zinc	28 50
Platinum	16.40
Iron	15.80
Tin	15.50
Lead	8.30
Mercury	3.45
Potassium	1.33

Since that time Pouillet has given the same measures and added the following table, which possesses a special interest from the fact that it exhibits the influence of the presence of foreign bodies, also of tempering and annealing.

Palladium	5791
Silver (963 fine)	5152
" (900 ")	4753
" (857 ")	4221
" (747 ")	3882
Gold, pure	3975
" (951 fine)	1338
" (751 ")	714
Copper, pure	3838
Copper, annealed	3842
Platinum	855
Brass	200 to 900
Cast steel	200 to 500
Iron	600 to 700
Mercury	100

The selection of a unit for comparison in this table is a matter of some interest.

Pouillet has chosen mercury for his standard obviously for the reason that being a liquid, it is the metal most easily obtained in a pure state, and that also, from this same property of liquidity, it is exempt from those variations of conductivity which arise from annealing, hardening, etc.

These considerations prompted the Congress of 1881 to select mercury for the material of the standard of resistance. At a second reunion of this Congress, this selection was approved, and the *ohm* was defined to be that amount of resistance offered by a column of mercury whose area of section is one square millimeter, and whose length is 106 centimeters; the temperature being 0° Centigrade.

The important researches of Becquerel, in 1846, led to the determination of the relative conductivity of several metals at the mean temperature of 12.75°.

These results are exhibited in the following table:—

	Hammered.	Annealed.
Silver, pure, reduced from chloride	93.448	100
Copper, precipitated by battery and fused	89.084	91.439
Gold, pure	64.385	65.458
Cadmium	24.574	
Zinc	24.164	
Tin	13.656	
Palladium	12.977	
Iron	12.124	
Lead	8.245	
Platinum	8.042	
Mercury (14°)	1.8017	

Mathiessen afterwards published in the *Proceedings of the Royal Society* the

following table, which has been accepted for practical use down to the present time:

	Resistance in ohms of a wire 1 m. long, & 1 mm. diam.		Conductivity.
Silver, annealed......	0.01937	..	100.
" drawn........	0.02103	..	92.1
Copper, annealed.....	0.02 57	..	94.1
" drawn........	0.02014	..	94.1
Gold, annealed.......	0.02650	..	73.
" drawn........	0.02697	..	73.
Aluminum annealed..	0.03751	..	51.6
Zinc, rolled..........	0.07244	..	26.7
Platinum, annealed...	0.1166	..	16.6
Iron, " ..	0.1251	..	15.5
Mercury, liquid......	1 2247	..	1.58
Nickel, annealed.....	0.1604	..	12.
Tin, pressed..........	0.1701	..	11.4
Lead, "	0.2526	..	7.6
Antimony, pressed....	0.4571	..	4.2
Bismuth, " ...	1.689	..	1.1
ALLOYS.			
Platinum 1, Silver 1, (drawn or annealed).	0.3140	..	6.1
German Silver, (drawn or annealed).	0.2695	..	7 2
Gold2, Silver 1......	0.1399	..	13.8

An examination of these tables shows a want of accordance, not only in the relative conductivities of the different metals, but also in their order. Thus silver occupies the third place in the table of the elder Becquerel, whereas it should have been in the first. Also Palladium is in the first place in Pouillet's table, while it should have been placed last.

These differences justify the new researches, which are furthermore prompted by that desire for precision which the daily progress in the practical application of electricity renders necessary.

I now proceed to present a *résumé* of my researches in determining the resistance of metals and alloys: then the results of experimenting upon the influence of temperature, and finally, upon the relations more or less intimate between the electric and thermic conductivity. I shall hand to you a set of small bars which have been prepared for these experiments. These bars, as you see, have been cast with a diameter of about 13 millimeters. They have been cut in such a manner as to exhibit the grain of a fracture, and the detached portion has been drawn into wire, upon which the experiments now to be described were tried.

In case of the alloys which cannot easily be drawn or rolled, as some of the compounds of silicon or phosphorus, the experiments have been tried directly upon the bars themselves by the method of Sir William Thomson.

In conducting the experiments, the bars were supported upon V-shaped rests, placed at an invariable distance apart. These rests were respectively in communication with two resistances composed of two parts, of which one is one thousandth of the other.

A Wheatstone bridge, a differential galvanometer, a battery of four cups, and a contact key complete the apparatus.

The measurements, which were very long and minute, and made upon a great number of specimens, were made in part by the writer at the workshop at Angoulême, with the assistance of Messrs. X. Muller and J. Stahl, engineers of the establishment, and partly by M. Duflon, electrical engineer in the measuring room of M. Sciami, director of the Maison Breguet.

These latter experiments were those made upon the bars themselves.

The number of specimens submitted to your examination is thirty-four. Their conductivities compared with silver and pure copper are given in the following table.

Silver................................	100
Copper, pure..........................	100
" refined and crystallized.........	99.9
Bronze, silicious, telegraphic...........	98.
Copper and silver alloy; eq. parts......	86.65
Gold, pure..........................	78
Copper with 4 per cent. of silicon......	75
" " 12 " "	54.7
Aluminum, pure.....................	54.2
Tin with 12 per cent. of sodium.........	46.9
Silicious bronze, telephonic.............	35.
Copper with 10 per cent. of lead........	30.
Zinc, pure..........................	29.9
Phosphor-Bronze, telephonic...........	29.
Brass, silicious, 25 per cent. of zinc.....	26.49
Brass with 35 per cent. of zinc..........	21.5
Tin phosphide.......................	17.7
Gold and silver alloy; eq. parts.........	16.12
Swedish Iron........................	16.
Tin, pure; banca.....................	15.45
Antimony-Copper....................	12 7
Aluminum-Bronze...................	12.6
Siemens steel........................	12.
Platinum, pure......................	10.6
Copper with 10 per cent. of nickel......	10.6
Cadmium 15, mercury 85..............	10.2
Bronze, Mercurial, dronier.............	10.14
Arsenical copper; 10 per cent. arsenic..	9.1
Lead, pure..........................	8.88
Bronze, containing 20 per cent. tin......	8.4
Nickel, pure.........................	7.89
Phosphor-Bronze with 20 per cent. Tin .	6.5
Copper with 9 per cent. phorphorus	4.9
Antimony...........................	3.88

A large number of experiments were tried upon alloys and compounds not represented in the above series.

It will be observed that in the results recorded above, resistances are not given in ohms, but conductivities are expressed relatively to a selected standard, which is pure silver. A wire of this metal, one millimeter in diameter, offers a resistance at 0° Centigrade of 19.37 ohms per kilometer.

Pure silver and pure copper are the best known conductors of electricity. The ease with which silver can be obtained in a pure state would seem to entitle it to the position of standard of comparison. It also oxidizes with greater difficulty than copper, and its oxide is a better conductor. In this last peculiarity, there is less chance of error when comparisons are made with silver for a standard.

Gold has a conductivity of 78. The alloy of gold and silver calls attention to a remarkable fact. When silver is alloyed with a small percentage of gold, the conductivity immediately diminishes. 2 per cent. of gold is sufficient to reduce the conducting power from 100 to 60. An increase in the proportion of gold, however, is attended with a decrease of conductivity at a less rapid rate, the minimum of 16.12 being the conductivity of an alloy of equal parts of gold and silver.

If the varying conductivities of the different proportions of gold and silver alloy be represented by a curve, it will be found to take the form of a parabola whose axis is perpendicular to the axis of X. The curve descending from the ordinate 100 for pure silver to 16.12 for the alloy of equal parts of the two metals and rising to 78 for pure gold.

This singular phenomenon seems to indicate that a kind of interference is produced analogous to that when rays of light partially extinguish each other by superposition. This is not an explanation, but a simple analogy.

It should be remarked, however, that it is not true, though sometimes asserted, that in an alloy the electric conductivity is always lower than that of the poorest conductor of the constituents. It is simply demonstrated that the union of two bodies modifies to a great extent their separate conductivities, and this fact ought certainly to lead some time to interesting results.

In examining the foregoing table, we find platinum with a conductivity of 10.6. Aluminum, which is distinguished from the other metals by its want of density, has the considerably higher conductivity of 54.2. This is partly compensated for by its low specific weight. But it is a mistake to consider its lightness as a quality fitting it some day for aerial telegraph lines. Its tenacity is rather feeble, and its tensile strength falls from 10 kilograms per square millimeter in the hard metal, to 6 or 7 kilos only in the annealed metal.

Iron, steel, lead, zinc, and tin, possess low conducting powers, and the results in the table are not insisted upon. It is sufficient to consider them in connection with results previously found.

Copper, however, is to be regarded with interest and care, as it, with its alloys, is regarded as the typical conductor of electricity. It possesses when unalloyed and is free from its oxide, a conducting power equal to silver. Its cost is low enough to render it available for industrial uses. It is malleable, and some of its alloys possess a remarkable tenacity.

It is interesting to note the increase in the conducting power of copper with the improvement in manufacture. In the late discussion upon electric conductors, Mr. Preece exhibited the following table, showing the progress in conductivity of submarine cables:

Submarine Cables.	Date.	Conductivity.
Dover-Calais	1851	42
Port Patrick-Donaghadee	1852	46
Transatlantic	1856	50
Red Sea	1857	75
Malta-Alexandria	1861	87
Persian Gulf	1863	89.14
Transatlantic	1865	96
Irish Sea	1883	97.9
Pure Copper	—	100.

A similar progressive increase in the conductivity of the copper wire, furnished to the Administration Francaise des Télégraphes, which, after having for many years exacted a conductivity altogether indeterminate, so to speak, has been gradually led to adopt figures quite comparable to the highest in our table.

The variation in the conductivity of copper is due to two principal causes: the presence of other bodies; and the presence of a varying quantity of copper oxide.

Mathiessen and Holzmann have made a series of interesting experiments bearing upon this subject.

The first table exhibits the conductivities of copper of different sections.

	Conductivity.	Temperature.
1st. Spanish copper from Rio Tinto, containing 2 % of arsenic with traces of oxides of copper lead and nickel	14.24	14.8°
2d. Russian; Demidoff with traces of arsenic, iron and nickel oxides	59.34	12.7°
3d. "Tough cake," location not specified, traces of lead, iron, nickel and antimony oxides	71.03	17.3°
4th. ——, traces of iron, nickel and antimony oxides	81.35	14.2°
5th. Australian, Burra-Burra, traces of iron oxide only	88.86	14.0°
6th. American: Lake Superior, 3 % of silver, with traces of iron and copper oxide	92.57	15.0°

The second table exhibits the conductivity of copper containing traces of different substances.

Copper with	Conductivity.	Temperature.
2.50 % phosphorus	7.24	17.5°
0.95 "	23.24	22.1°
0.13 "	67.67	20.0°
5.4 % arsenic	6.18	16.8°
2.8 "	13.14	19.1°
Traces of arsenic	57.8	19.7°
3.2 % zinc	56.98	10.3°
1.6 "	76.35	15.8°
Traces of zinc	85.05	10.3°
1.06 % iron	26.95	13.1°
0.48 "	34.56	11.2°
4.90 % tin	19.47	14.4°
2.52 "	32.64	17.1°
1.33 "	48.52	16.8°
2.45 % silver	79.38	19.7°
1.22 "	86.91	20.7°
3.50 % gold	65.36	18.7°
0.31 % antimony	64.5	12.°
0.29 % lead	64.5	12.°

These results are approximately the same as those we have obtained, and which we have already referred to.

A third table exhibits the influence of the presence of copper oxide in the metal.

The first five examples are of cold-drawn wire of pure copper:

No.	Mode of Preparation.	Conductivity.	Temperature.
1.	Reduced by hydrogen	93.00	18.6°
2.	Deposited by battery; not fused	93.46	20.2°
3.	Commercial copper	93.02	18.4°
4.	Ex. No 3 remelted in hydrogen	92.76	19.3°
5.	The same treated with a jet of hydrogen while fused (It has been noticed that the conductivity has been raised about 2½ per cent. by annealing.)	92.99	17.5°
	Copper containing an undetermined amount of oxide.	73.32	19.5°
	Deposited copper taken from a bar, fused on charcoal, and cast in an atmosphere of hydrogen	93.3	12.8°
	Copper same as preceding taken from a porous bar; cast in a mould under ordinary conditions	94.8	13.°
	Deposited copper, treated to cementation with charcoal, and containing silicon with traces of iron and phosphorus	62.8	13.°

It may be added as a commentary on the last results that in France pure copper wire is prepared by a special process, which completely eliminates the oxide. Copper thus prepared has the conducting power of silver. It has been measured recently by the telegraphic authorities mentioned above, and with the result of exhibiting a conductivity of 102 by the standard heretofore used, and whose imperfections were thus demonstrated.

I cannot remember by what chain of circumstances I was led to study the compounds of copper and silicon, prepared for the first time by M. Henri St. Clair Deville, and apply them practically to the uses of the telegraph and the telephone.

Coppers and bronzes prepared under such conditions are much used for aerial telegraph and telephone lines. Their electrical and physical qualities permit us to believe that the time is gone in which iron and steel enjoy the monopoly in such transmission.

It is certain, as has been remarked by the eminent electrician in chief of the London Post Office: "Aerial lines should no longer be limited to the single quality of tensile strength. At a time when the apparatus for rapid transmission occupies all the great telegraphic routes, the lines should be made of very good conductors; the surfaces reduced so as to offer the least possible chance for induction, and they ought also to offer but little surface to the wind and snow, and should be practically indestructible."

Mr. Preece does not hesitate to conclude that iron and steel wires have had their day, and they will soon be replaced by wires of copper and its alloys. The latter fulfill not only all the required con-

ditions, but when once erected, cost no more than lines heretofore used.

VARIATION OF CONDUCTIVITY WITH CHANGE OF TEMPERATURE.

The influence of temperature on electric conductivity is well known. Heat expands bodies, and thus separates their molecules by spaces of greater or less width.

It would seem to follow therefore that heat should modify the conducting power in a manner depending upon whether the molecule or the intervening space is the better conductor.

In the case of metals, elevation of temperature diminishes the conductivity. Silver sulphide, on the other hand, according to Faraday, gains in conducting power as its temperature is raised.

Non-metallic elements are unlike the elements in this respect. A familiar example is afforded by the incandescent lamps. The conductivity of the filament is notably greatest at the time of incandescence than when it is cold.

Becquerel made researches upon this subject in 1846. Without considering the details of his method, we will limit ourselves to mentioning the fact and to giving some of his results. The coefficient of increase of resistance is given in the following table, for each degree rise in temperature:

The coefficient is the value of k in the formula: $R=r(1+kt)$; in which r is the resistance at 0° and R that at $t°$.

Mercury	0.001040
Platinum	0.001861
Gold	0.003397
Zinc	0.003675
Silver	0 004022
Cadmium	0.004040
Copper	0 004097
Lead	0.004349
Iron	0 004726
Tin possibly containing lead.	0.005042
Tin, pure	0.006188

The change of volume due to the expansion has in reality no effect. The augmentation of conductivity, when this is the consequence, is nearly balanced by the diminution resulting from the increase in length, which is a part of this same expansion.

The relative conductivity of several metals (the bodies of most interest in this relation) are given in the following table, which should be compared with that by the same physicist previously given for the mean temperature of 12.75°:

	Conductivity at 0°.	at 100°.
Silver, pure, annealed	100.	71.316
Copper, pure, annealed	91.517	64.919
Gold, " "	64.960	48.489
Cadmium	24 579	17.506
Zinc	24.063	17.596
Tin	14.014	8.657
Iron, annealed	12.350	8.387
Lead	8.277	5.761
Platinum, annealed	7.933	6.688
Mercury, distilled	1.7387	1.5749

We have at present no similar experiments of our own with which to compare these. We may therefore accept these results for the present, with the reservation before expressed, relating to the purity of the metals and the precision of the methods. We shall have occasion to refer to this subject in the next communication.

ELECTRIC COMPARED WITH THERMIC CONDUCTIVITY.

The prevalent notions regarding the identity of divers physical phenomena have led physicists to seek to determine the relation between heat, light, and electricity.

Without repeating the theories bearing upon this subject, I may recall the opinions expressed in relation to the analogy between electric and thermic propagation, by giving the figures previously referred to, and specify the experiments, the results of which will form the subject of a later communication

The analogy referred to, has been supposed to exist since it was known that some metals appeared in the same order when classified either by their electric or their thermic properties. Also metals are good conductors of heat and electicity, while dialectrics are generally poor conductors of heat. Furthermore, the coefficients of conduction for heat and electricity are for many metals nearly the same.

The researches of Wiedmann and Franz yielded the following results:

	Conductivity. Thermic.	Electric.
Silver	100	100
Copper	74	73
Gold	53	59
Brass	24	22
Tin	15	23
Iron	12	13
Lead	9	11
Platinum	8	10
German silver	6	6
Bismuth	2	2

These results are certainly very interesting, and at first view tend to mislead those experimenters who seek to simplify the explanations of physical phenomena, and who are always seeking to unify the theoretical conceptions of elementary laws.

Meanwhile, we do not find it possible to accept unreservedly these results, nor the conclusions which it is desired should be drawn from them. We know that the table of electric conductivities needs revision. Copper, for example, appears in the table of Wiedmann and Franz with a coefficient of 73 for electric conductivity, whereas it is well known to be entitled to rank with silver at the top of the scale.

We find, also, some remarkable divergences in the following list:

	Conductivity. Thermic.	Electric.
Copper	73.6	100.
Gold	53.2	78.
Zinc	19.1	29.9
Aluminum	19.6	54.2
Silicious bronze	68.	98.
Antimony	21.5	3.88
Tin with sodium	13.34	46.9

The whole question clearly deserves to be restudied without the embarrassments of preconceived theories.

We have undertaken this new study, and we will limit ourselves here to describing our preliminary experiments. These researches have been carried on with the valuable assistance of M. Jannetaz; *Maitre de Conférences* of the Sorbonne, who has willingly brought to our aid his profound knowledge of the subject, and the apparatus with which he prosecuted his previous labors in determining the conduction of heat in minerals.

The process which he employed and which we have adopted is as follows:

The apparatus as devised by M. Jannetaz, consists essentially of a plate upon which is placed the mineral, whose conducting power is to be tested. This body has been previously prepared by coating a plane face of it with a thin layer of wax or grease. In contact with this is a small bead of platinum about the size of the head of a pin, which has been welded to the apex of a triangle of platinum wire. A current being sent through the wire heats it; the bead fusing the wax over a circular or elliptical space, whose dimensions, taking into account the temperature of the wire and the time of contact, afford data for determining the conducting power of the specimen.

To complete the description, it is only necessary to add that the wire is brought through a copper casing which is supplied with a current of cold water, and that the wire is also supported by a carrier, which facilities making contact with or withdrawal from the specimen.

It is with this apparatus that we have begun our researches. Unfortunately, it has not been possible in the time available, to prepare thin plates of the metals and alloys. We have been obliged to content ourselves with half-cylinder sections, cut from the little bars employed in the previously mentioned experiments. Under these circumstances, the external conditions may have modified the results, and we prefer at present not to exhibit numerical results which may require correction.

It is sufficient now to say that some of the results appear to invalidate the law of identity above referred to, and others lead to the interesting conclusion that there may be a difference of conductivity in the same metal depending upon the method of preparation, whether it was cast, drawn, or rolled.

You will notice upon the specimens submitted to you, which bear traces of these experiments, that the elliptical form of the curved spaces takes a direction corresponding to the lamination of the specimen.

We have completed the observations which we set out to present to you, and which, as you see, form a programme of researches to be made quite as much as of results accomplished.

We wish especially before closing, to call your attention to the double interest attaching to the subject which has enlisted your attention. From the practical point of view alone, the subject is of the highest importance, as has been shown by the results obtained, bearing as they do upon the vast problem of transmission of electric energy.

But we do not insist upon this point. The estimate of this subject from the scientific standpoint is no less great, involving as it does the nature of electricity itself.

Among the facts which we have noted, we will refer again to the strange anoma-

ly exhibited by the alloys as conductors of electricity, when we consider them as compound substances. This might serve as an occasion to review the theories promulgated by certain eminent physicists; Clerk-Maxwell among others. But these theories are yet such mere hypotheses, and their interpretation appear to me so abstract that I fear to venture upon this ground.

But there is no rashness in affirming that in the resistance offered by a body to the electric current, the magnetic orientation of the molecules of each body plays an important part. Each of them, influenced by the passage of the current to a degree probably depending upon its constitution, modifies the resistance.

The question may be asked whether the compound molecules of the alloys do not behave as other elementary electric couples, having a special electromotive force that determines the passage of the current, and which produces in the total mass a series of currents analogous to the so-called Foucault currents, the effect of which is contrary to that of the primary current.

We are tempted to believe that in the alloys the chains of molecules of the same kind are not able to act like a cable made of wires of different metals. As the alloy of equal parts of gold and silver has already been referred to, let us determine the resistances under the different conditions. If the current be transmitted through two wires each a kilometer long and a millimeter in diameter, one being of gold and one of silver, the formula for derived currents enables us to determine the total resistance. The formula is:

$$x=\frac{R_1\ R_2}{R_1+R_2}$$

The resistance of the silver being 19 ohms, and of the gold 24.36 ohms, the total is by the above formula, 13.38 ohms.

But a wire made of the alloy in these proportions, having the same length and a double cross-section, offers a resistance of 58.50 ohms.

This result shows that the law governing derived currents does not apply in the case of conduction through alloys.

Some other cause intervenes; possibly such an one as is mentioned above; perhaps an unknown cause which essentially modifies the flow of electricity.

We may be permitted to recommend this investigation to physicists, and we are ready to devote our efforts to the same end.

If this task, to the accomplishment of which, we wish to dedicate ourselves, is crowned with success, we shall be sufficiently recompensed by the satisfaction which labor affords to those who seek thereby to advance science.

SECONDARY BATTERIES.*

By FRANK GEERE HOWARD, Stud. Inst. C. E.

From Selected Papers of the Institution of Civil Engineers.

Among the most important discoveries of recent years few have received so much attention, and had such bright prospects, as the accumulators of Planté and their various modifications. All sorts of wild schemes were set afloat for their utilization, the principal one being to talk about the time when electricity by means of these accumulators could be delivered every morning from house to house like milk. The result of this would have been much the same as if a water company attempted to supply water to its customers by sending round so many gallons every day in carts, or better still a gas company delivering bags full of gas ready for use, one being as impracticable as the other. These ideas were soon dispelled, and then followed disappointment, and of course general condemnation of secondary batteries.

But all this time inventors had been steadily turning their attention in this direction. Many improvements have been introduced, and numbers of difficulties, before considered fatal to them, have been

* This communication was read and discussed at a meeting of the students on the 16th of January, 1885.

overcome. They are still far from perfect, though it may yet be expected that accumulators or secondary batteries will play the most important part in all extended systems of electric lighting, transmission of power, and the satisfactory solution of the utilization of wind and water power.

The earliest form of accumulator was a voltameter worked backwards, and was first observed in 1801, by the French chemist, Gautherot. Two years later, Ritter, in Germany, carried out a number of experiments in the same direction, and endeavored to utilize the reaction; he employed for this purpose two plates of gold, separated from each other by flannel kept moistened with acid, and charged by an ordinary Volta's pile. If two pieces of platinum be immersed in dilute sulphuric acid and connected to the two poles of a battery, so that a current be sent through them, the liquid will be decomposed, hydrogen being driven off at one plate and oxygen at the other. If, then, the battery is disconnected and the plates circuited through a galvanometer, it will be seen that they give out a current in the opposite direction to the charging current.

Becquerel first pointed out the real cause of the returned current, upsetting the original theory that the plates absorbed the current during the charging, and gave it up again during the discharge. He showed that the returned current was not due to the storage of electricity, but to the presence of substances having chemical affinities for each other, derived from the decomposition going on during the charge.

If two sheets of ordinary lead be placed in dilute sulphuric acid, and a current be sent through them, a dark brown deposit will form upon the plate connected to the positive pole of the charging battery, due to the formation of peroxide of lead on the surface of that plate, whilst the other will be reduced slightly to spongy lead. Upon disconnecting these plates from the charging source, and circuiting them through a galvanometer, a current will flow from the brown or peroxidized plate to the other reduced plate.

The result is the reduction of the peroxide of lead on the one plate to oxide, and then to sulphate of lead from the presence of the sulphuric acid in the solution; and of the spongy lead to sulphate of lead. Upon recharging, the anode or brown plate, attached to the positive pole of the charging source, will again become peroxidized, and the cathode or other plate reduced to spongy lead. The sulphate of lead will thus disappear from both plates. The plates are thus continually oxidized and deoxidized as they are charged and discharged.

This is a second battery in its simplest form, and all accumulators are based upon this principle. Lead has proved by far the best metal for the purpose, because it forms an almost insoluble oxide; a few other metals are nearly insoluble, but lead has the advantage. It is better than silver or manganese for the reason that its oxide is less soluble than either of them.

The initial electromotive force of a freshly prepared cell when charged is 2.25 volts, but this drops after the first few minutes of the discharge to a little less than 2 volts, which may be taken as the normal electromotive force of a cell made up of peroxide of lead, spongy lead and dilute sulphuric acid. The constancy of the electromotive force of a cell varies considerably with the rate of discharge. If it is run down, or discharged, at a very rapid rate it falls off very quickly; whereas, if only a small current be taken out, the electromotive force will remain constant for a long time, though the loss on the negative plate or anode from local action will be greater. The strength of the electrolyte or solution varies considerably during the charging or discharging of the cell. When the cell is fully charged the solution is strongest, and when discharged weakest. From this it follows that the internal resistance of a battery is lowest when the electromotive force is at its highest, and the converse. Dr. Frankland has endeavored to utilize this change in the specific gravity of the solution, to indicate the condition of the cell, and has found that a change of 0.005 in the specific gravity was equal to a storage of 20 ampere hours. This method of determining the state of the cell does not in practice give very good results, on account of the difficulty of getting the solution in the cell to circulate freely, so as to be of the same density throughout.

The first practicable form of accumulator was that of Planté, made in 1860. He took two sheets of lead, and placing them

in dilute sulphuric acid, passed a current through them for some time. Then disconnecting the cell from the charging source, he discharged it and again charged it, but this time in a reverse direction; so that the lead plate, formerly connected to the positive pole of the charging source, was this time connected to the negative pole. A number of charges and discharges were thus alternately effected until the plates were rendered sufficiently porous to give a satisfactory storage capacity. Planté found that it was necessary to continue this process for some months before a battery was fit for use. Of course when the battery was used the current was sent through it always in the same direction. The reverse charges were only employed to "form" the plates, or render them sufficiently porous to hold a charge. Unfortunately, this form of cell, simple as it is, does not store much electrical energy, except when very thin plates are employed, and these soon become brittle and liable to fall to pieces through being entirely converted into peroxide of lead and thereby losing their stability.

According to Géraldy, a Planté cell containing 1.445 kilogram of lead, is capable storing 4,983 kilogrammeters of energy, or 11,329 foot-pounds per lb. of lead. De Meritens modified the Planté cell by making the plates out of a number of thin sheets of lead laid one on top of the other, and soldering their edges together, thus obtaining plates composed of a number of layers of thin sheet lead.

Kabath again modified this by taking long thin strips of lead which he corrugated by passing them through grooved rollers, and then interleaving them with plain strips, or ribbons of lead, folding them backwards and forwards, until a plate was formed of the required size. The drawback to this form is that the lead used is of necessity very thin, and thus soon breaks down and renders the cell useless. In both these cases the same tedious process of forming as in the Planté cell has to be gone through.

In 1880, Faure devised the plan of pasting red lead, or minium, upon plates of plain lead; this paste was rapidly peroxidized and reduced in one charge, thus overcoming and doing away with the long process of forming the Planté plates. The paste had to be held on the plates by wrapping them in flannel or some such porous material. This increased the internal resistance of the cell, and after a time the flannel began to get rotten and fall off, causing the cell to fail. A much higher storage capacity, however, was obtained, it being 18,000 foot-pounds per lb. of lead and minium. Many improvements have been introduced by Messrs. Sellon and Volckmar, culminating in the form now manufactured by the Electrical Power and Storage Company, which consists of grids of cast-lead with holes from ¼ to ⅜ inch square.

The grid used for the anode is made thicker than that for the cathode. These castings are filled with a paste of minium and dilute sulphuric acid. After being dried they are placed in long troughs, filled with dilute sulphuric acid, and a current is sent through them to form them. The amount of current necessary to convert 1 lb. of minium to peroxide is 70 ampere hours, and 140 ampere hours to reduce it to metallic lead. But as it only needs 110 ampere hours to reduce 1 lb. of litharge, this is generally employed for the cathode plate. After the current has been sent through the plates, they are taken out, washed, and stacked until required for use, when they are mounted in their respective cells, and employed for lighting or other purposes. No felt is put between these plates; they are separated from each other by india-rubber bands, pieces of wood, or some similar device.

The storage capacity of this cell is considerably higher than that of the Faure type, being 48,000 foot-pounds per lb. of lead. But since the storage capacity of a theoretically perfect secondary battery is 360,000 foot-pounds per pound of lead, even the best cell falls far short of this. The figures in Table I., against Sellon-Volckmar, are the results obtained from cells specially constructed for driving electric launches, tramcars, &c. Those employed ordinarily for lighting purposes yield about 36,000 foot-pounds per lb. of lead.

It has been proposed by Mr. Fitzgerald, to substitute carbon for the lead employed for establishing contact with the active material of the cathodes, where oxides of lead are used, and also to protect parts of the anodes by means of some insulating material, so as to arrest the process

TABLE I.—STORAGE CAPACITY OF THE VARIOUS SECONDARY BATTERIES AS REGARDS RATIO OF WORK TO WEIGHT.

NAME.	Storage Capacity in foot-lbs. per lb. of Lead.	Percentage of Efficiency.
Theoretically perfect cell.	360,000	100.00
Sellon-Volckmar.........	48,000	13.13
Crompton-Fitzgerald	24,000	6.66
Faure	18,000	4.99
Planté..................	12,000	3.33

of peroxidation over the whole of the plates, and thus always to preserve a good contact throughout the anode. A large number of experiments have been carried out in this direction. The best results obtained were from a battery made up of cathodes composed of minium and carbon fragments, held in a punctured lead envelope, and anodes composed of spongy or finely-divided lead, also held in a punctured lead envelope, and protected by a network of insulating material, generally known under the name of Prout's glue. This form had a storage capacity of 24,000 foot-pounds per lb. of lead. Messrs. Beaumont and Biggs' battery is made up of plates of compressed spongy lead. The lead is obtained in this state by precipitation from a solution of acetate, or sugar of lead, in which solution is placed a plate of zinc. Lead in a state of very fine subdivision is deposited upon the zinc, from which it is scraped off, put into moulds, and subjected to hydraulic pressure. By this means plates of great porosity are obtained, which should have as high a storage capacity as any known form of battery. Of the numerous batteries in the market, nearly all are modifications of the well-known Planté type, which seems to be the best form for durablity, if not for storage capacity; and the "life" of a battery is of far more importance than its storage capacity per lb. weight. The most notable battery of this class, and one coming prominently into notice, is manufactured by the Wolverhampton Electric Light Co., called the Elwell-Parker accumulator. The plates of this battery are made of sheet-lead only, but before being formed they are immersed in a strong solution of nitric acid, which attacks the surface of the lead, and honeycombs it to a certain extent. Thus, according to Planté, who first patented this process in 1881, the long process of forming is to a great extent done away with. There seems, however to be considerable doubt whether this is the case, or if the nitric acid simply dissolves the lead. In all probability the acid attacks the impurities in the lead, and thus prepares the plate in the best possible manner to receive the current. Another battery is the B. T. K., so called from the names of the inventors, Messrs. Beeman, Taylor, and King. This is a modification of the Kabath accumulator, consisting of plates made of alternate corrugated and plain strips of lead, wound round and round upon themselves. Batteries of this description are being largely used in the district lighting at Colchester.

An improvement by Dr. Frankland is claimed upon the Sellon-Volkmar type of battery, by hardening the paste of minium, or other oxide of lead used in the plates, by immersing the plates in solutions of sulphuric acid of various densities during different periods. He also claims for the plates the capacity of receiving a higher rate of charge. This hardening mixture may be cast into plates or cylinders of any desired form or size. There are also many other batteries of the Planté type by various inventors. Mr. Joel has patented a plate having a considerable storage capacity. He makes the plates of lead-wool, or perhaps more properly speaking, lead-fiber, mixed with minium, and then pressed upon a cast-lead plate, which acts both as a conductor and support.

A battery differing in many ways from those already mentioned, was that proposed by Sutton. He took two plates, one of lead and the other of copper, and immersed them in a solution of sulphate of copper. The lead plate became peroxidized, and the copper was alternately dissolved and deposited from the solution of sulphate of copper. This battery, though giving good results upon a small scale failed when used to a large extent. The electromotive force is much lower than that of a cell composed of lead only.

The advantages of accumulators made after the Planté type over those of the Faure type are that they afford a far higher rate of discharge. This property is invaluable for many purposes, as, for

instance, where numerous lamps are required to be run for only a short time. But then, again, accumulators of the Faure type take in a higher rate of charge than the Planté, because they have a greater thickness of working material, and also store a large amount of electrical energy in a given weight.

The durability, or life, of a secondary battery, has never been satisfactorily settled. Manufacturers assert that their accumulators will last for a number of years, but this has not yet been proved. Those of the Sellon-Volkmar type cannot, the author believes, be depended upon to last more than five or six months at the outside, when they are worked regularly every day up to their full capacity. But when overcharged continuously, or otherwise improperly used, a few days will often suffice to put them out of order.

The Planté accumulator should last longer than this, but it must be borne in mind that when this battery has arrived at its maximum of efficiency, it is on the point of falling to pieces. Where water or steam power is available, a rough but serviceable battery may be made by taking sheets of thick lead, cutting them into plates of any required size, and mounting them in tanks filled with dilute sulphuric acid. Of course, during the first few charges the storage capacity will be very small, but this will improve every day; at the same time the cost of forming them is next to nothing. Eventually it will prove to be a most efficient battery.

The first and most important point in setting up a secondary battery is to carefully insulate the cells from each other, and from any moisture. This is best effected by arranging them on wooden shelves raised about 8 inches off the floor, and at an equal distance from the walls. The boxes should be left with a space of at least an inch between them, and, wherever possible, placed in single rows, and not upon shelves one on top of the other, so that free access may be had to each box. In large installations the cells are often of considerable size, weighing from 5 to 6 cwt. each, thus rendering it extremely difficult to move them. For these reasons, they should be so arranged that, once put up, they can be easily got at for repair without being removed. The importance of keeping the cells perfectly free from moisture can hardly be overrated. If they should be placed upon a damp floor, or in any position where moisture can get to them, there will be a considerable leakage of electricity, the cells will rapidly run down (*i. e.*, become discharged), and will be in all probability condemned as unworkable and useless.

All secondary batteries, when first set up, should be tested for insulation. This may easily be done by connecting one pole of the battery (after it has been charged) to one terminal of an ordinary galvanometer, and the other terminal of the galvanometer to earth. The best way to obtain a good earth is to connect the wire to a water or gas pipe; or, if neither of these should be available, to drive a piece of iron into the ground and make a connection in that way. If the needle is deflected it indicates leakage, which should at once be remedied. The leakage will in all probability be found to arise from an escape of the solution from one of the cells, the place where they are standing being damp, or one of the leads "making earth." The poles of the cells should be connected together by stout bars of metal. It is inadvisable to use copper on the positive pole of a cell, on account of the rapidity with which it corrodes. Plain lead connections, or a mixture of lead and antimony, are the most trustworthy. The positive pole of one cell is joined to the negative of the next, and so on all through the series. Then the positive pole of the dynamo is connected to the last positive of the battery, and the negative to the negative. The contacts must be properly made between each cell, as a bad contact will produce a great deal of heat in the cells, thereby introducing resistance into the circuit, and reducing materially the rate of charge or discharge. Care should be taken not to charge the battery at too rapid a rate, and if at any time during the charging the cells become warm, the charging should be stopped, and the battery allowed to cool down. The heating of the plates causes them to buckle, often resulting in the short circuiting of the cell, and thus putting it out of use until the defect has been remedied.

During the charging, and also whilst they are at rest, it is advisable to test

the cells from time to time. This may be readily done by taking a short length of stout wire, holding one end upon one pole of the cell to be tested, and striking the opposite pole with the other end of the wire. If the cell is in good condition it should give a bright, crisp spark. If it gives no spark at all it shows that there is a short circuit in the cell, arising, in all likelihood, from two of the plates touching each other, or from a small piece of some conducting substance having fallen down between the plates. This should be at once remedied, otherwise the cell should be cut out from the circuit and taken to pieces. It is very important, when working batteries, to keep all the cells in as even a condition as possible. For, if a cell be empty, or is discharged before the others, it will then become recharged in the reverse direction, and thus set up an opposing electro-motive force to the rest of the battery, besides there is the risk of spoiling that particular cell. Keeping all the cells in the same condition is best effected by observing them when charging, and cutting out of the circuit all those cells that give off gas freely, and continuing to charge the others until they are all fully charged. If this is done about once in two months, the working efficiency of the battery will be maintained at a very high point. Of course, this entails extra labor, but the author contends that secondary batteries do, and always will, require skilled supervision. Care must be taken not to cut out too many cells whilst charging the battery without introducing a corresponding resistance, or the dynamo will be burnt.

The ordinary strength of the electrolyte is 1 part of sulphuric acid to 9 parts of water, or of a specific gravity of 1.2, that of sulphuric acid being 1.84. The water should be as pure as possible. When working a battery it is necessary to keep all the plates immersed in the electrolyte. If it becomes necessary to add to the electrolyte, to keep the cell full, water only should be used, otherwise the solution will become too strong. When batteries are left out of use for any length of time, it is advisable either to remove the solution and wash the plates and boxes with water, or else to leave them fully charged. This latter method is perhaps best adopted only when the battery is not to be out of use for very long. It is found that, when batteries are left idle and full of the solution, sulphate of lead will form all over the plates, and have a serious effect upon their future working.

By discharging a cell too rapidly, as much harm can be done to it as by charging it too rapidly.

The best dynamos for charging accumulators are shunt dynamos. Both the series and compound machines are liable to have their poles reversed by the batteries discharging through them, which is likely to arise from the speed of the dynamo decreasing, and thereby having its electromotive force overpowed by that of the battery. This difficulty might be overcome by specially-designed automatic switches. The most economical way is to charge each battery in two series in parallel, though, of course, while charging them in this manner, no lights can be run at the same time. The circuit should be so arranged that the dynamo is charging the battery in two series in parallel, or in one series with the lamps in parallel, or dynamo, lamps and battery all in parallel. In order to charge an accumulator, it is necessary to employ a dynamo giving an electromotive force greater than that of the cells, and greater in proportion to the rate of charge required; but as all excess of electromotive force is lost energy, a slow charge is the most economical where time is of little moment.

The uses to which accumulators may be put are almost endless. They are serviceable for every application of the electric current. Lighting being at present the most in need of them, by employing an accumulator in any installation, the failure of the light should be rendered almost impossible. The chief causes of break-downs are the slipping or breaking of belts, the heating of the bearings, or some trivial mishap with the engine which may be set right in a few minutes, but which is sufficient to plunge the place so lighted into darkness. Now, by employing accumulators, if the engine has to be stopped, the batteries immediately come into action, and should run the lights long enough for the necessary repairs to be effected. In nine cases out of ten the failure of the electric light can be traced to some minor breakage or mishap

that can be remedied in half an hour, or even less; but, of however short duration, a total extinction of the lights must follow when working direct from the dynamo. Again, in almost all factories and workshops where steam power is used, there is a surplus of from 2 to 4 HP. This surplus, if employed to drive a small dynamo and to charge batteries all day, would be sufficient to run enough incandescent lamps to light up the whole of the shops in the evening. Accumulators may also be used advantageously in steadying the light when the dynamo is worked by a gas-engine, or any irregular source of power. The only method of satisfactorily solving the problem of the utilization of water power is by employing storage batteries, so that the water, instead of running to waste day and night, could be made to store electricity, which, in its turn, could either be used for motive power or for ordinary lighting purposes, and be drawn off at will. This applies also to the possibility of utilizing wind power.

The employment of secondary batteries for driving trains, tramcars, boats, &c., has been a good deal discussed, but up to the present time, although many costly experiments have been undertaken, no practical work has been done.

The first experimental launch worked by accumulators was that fitted up by the Electrical Power Storage Co., and launched at Millwall in September, 1882. She was 25 feet long, 5 feet beam, with accommodation for about ten passengers. She was driven by two Siemens dynamos of the S D_3 type, coupled in parallel. The current was supplied by forty-five 1 HP. Faure Sellon-Volkmar cells. The two motors running together absorbed a current of 46 amperes, and developed abont 3.75 HP. The weight of each motor was 316 lbs., that of the countershaft, &c., was 180 lbs., and the total weight of the accumulators was 2,520 lbs., or 56 lbs. per cell.

The second launch was built by Messrs. Yarrow & Co., of Poplar, and was fitted with all the electrical arrangements by the Electrical Power Storage Co., for the Vienna Exhibition of 1883. She is 40 feet long, 5 feet beam, and will carry about forty passengers. In this boat the motor and batteries are placed under the floor. The motor used is a Siemens D_3 dynamo, which, with eighty Faure-Sellon-Volkmar accumulators (the number carried), will develop 7 HP. Its weight is 658 lbs., and the efficiency, when tested, was 78 per cent. In this boat no gearing is used between the motor and screw-shaft, the former being coupled directly to the latter. The average speed attained was 8 miles per hour. The advantages are, that whereas two men are necessary to work a steam launch, the electric launch needs but one man to look after it, as he can steer, stop, or regulate the speed at the same time. An electric launch will also carry more passengers, besides being free from smoke, smell, heat and noise.

The experiments carried out with tramcars have not yet advanced nearly so far. A trial trip was made on the Gunnersbury and Kew Tramway line on the 10th of March, 1883. The experimental car, as in the case of the launch, was fitted up with the Electrical Power Storage Co., and was of the ordinary type used upon the London tramways. It was driven by a Siemens dynamo, designed to work with an electromotive force of 100 volts, and a current of 60 amperes furnished by 50 accumulators placed under the seats of the car. The cells measured 13 inches by 11 inches by 7 inches, and weighed 80 lbs., their total weight being 4,000 lbs. The storage capacity of each cell was 560 ampere-hours. Apart from this experiment, which id not altogether end successfully, the problem of propelling tramcars has not been solved. The same company exhibited a tricycle at the Vienna Exhibition, worked by twenty-one small accumulators and a motor designed by Mr. Reckenzaun, of London. This would run for about one hour, and cover about 8 miles in that time. The main reason why electricity has not yet been employed for motive power for boats and tramcars is, the author thinks, undoubtedly to be traced to the still imperfect and unsatisfactory state of secondary batteries. When once they have been so far improved as to be looked upon as trustworthy, a great and lasting impetus will certainly be given to the utilization of electricity for motive power as well as for lighting purposes.

THE WATER SUPPLY OF ANCIENT ROMAN CITIES.*

BY PROF. W. H. CORFIELD, M.D., M.A.

From "The Building News."

As the supply of water to large population is one of the most important subjects in connection with sanitary matters, and one upon which the health of the populations to a very large extent depends, I propose to give a short account of some of the more important works carried out for this purpose by the ancient Romans—the great sanitary engineers of antiquity—more especially as I have had exceptional opportunities of examining many of those great works in Italy, in France, and along the north coast of Africa. Of the aqueducts constructed for the supply or Rome itself, we have an excellent detailed account in the work of Frontinus, who was the controller of the aqueducts under the Emperor Nerva, and who wrote his admirable work on them about A. D. 97. It may be interesting in passing to mention that Frontinus was a patrician, who had commanded with distinction in Britain under the Emperor Vespasian, before he was appointed by the Emperor Nerva as controller (or, we should say, surveyor) of the aqueducts. He was also an antiquarian, and in his work he not only describes the aqueducts as they were in his time, but also gives a very interesting history of them. He begins by telling us that for 441 years before the building of the city—that is to say, B. C. 312—there was no systematic supply of water to the city; that the water was got direct from the Tiber, from shallow wells, and from natural springs; but that these sources were found no longer to be sufficient, and the construction of the first aqueduct was undertaken during the consulship of Appius Claudius Crassus, from whom it took the name of the Appian aqueduct. This was, as may be expected from its being the first aqueduct, not a very long one; the source was about eight miles to the east of Rome, and the length of the aqueduct itself rather more than eleven miles, according to Mr. James Parker, to whose paper on the "Water Supply of Ancient Rome" I am indebted for many of the facts concerning the aqueducts of Rome itself. This aqueduct was carried underground throughout its whole length, winding round the heads of the valleys in its course, and not crossing them supported on arches, after the manner of more recent constructions; it was thus invisible until it got inside the city itself, a very important matter when we consider how liable Rome was, in those early times to hostile attacks. It was soon found that more water was required than was brought by this aqueduct, and it was no doubt considered desirable to have tanks at a higher level in the city than those supplied by the Appian aqueduct. It was determined, therefore, to bring water from a greater height, and from a greater distance, and the river Anio, above the falls at Tivoli, was selected for this purpose. The second aqueduct, the Anio Vetus, was no less than 42 miles in length, and was, like the Appian, entirely under the surface of the ground, except at its entrance into Rome, at a point about 60 ft. higher than the level of the Appian aqueduct. Little search has been made for the remains of this aqueduct, and its exact course is not known; but during my examination of the remains of the subsequent aqueducts at a place the Porta Furba, near Rome, where the ruins of five aqueducts are seen together, and at, or close to, which point the Anio Vetus must also have passed underground, I was rewarded for my search by discovering a hole, something like a fox's hole, leading into the ground, and on clearing away a few loose stones which had apparently been thrown into it, and putting my arm in I found that it led to the specus or channel of an underground aqueduct, and on relating this incident to the late Mr. John Henry Parker, the antiquarian, who was then in Rome, and showing him a sketch of the place, he said that he had no doubt that I had been fortunate enough to discover the exact position of the veritable Anio Vetus at that spot. These two aqueducts

* An address delivered before the Sanitary Institute of Great Britain.

sufficed for the supply of Rome with water for about 120 years, for Frontinus tells us that 127 years after the date at which the construction of the Anio Vetus was undertaken—that is to say, the 608th year after the foundation of the city—the increase of the city necessitated a more ample supply of water, and it was determined to bring it from a still greater distance. It was no longer considered necessary to conceal the aqueduct underground during the whole of its course, and so it was in part carried above ground on embankments, or supported upon arches of masonry. The water was brought from some pools in one of the valleys on the eastern side of the Anio, some miles further up than the point from which the Anio Vetus was supplied, and the new aqueduct, which was 54 miles in length, was called the Marcian, after the Præter Marcius, to whom the work was intrusted. Frontinus also tells us the history of the other six aqueducts which were in existence in his time, viz.: the Tepulan, the Julian, the Virgo, the Alsietine or Augustan, the Claudian, and the Anio Novus, the last two being commenced by the Emperor Caligula, and finished by Claudius, because "seven aqueducts seemed scarcely sufficient for public purposes and private amusements"; but it is not necessary for our purpose to give any detailed account of the course of these aqueducts, it is only necessary to mention one or two very interesting points in connection with them. In order to allow of the deposit of suspended matters, piscinæ, or settling reservoirs, were constructed in a very ingenious manner. Each had four compartments, two upper and two lower; the water was conducted into one of the upper compartments, and from this passed, probably, by what we should call a standing waste or overflow pipe, into the one below; from this it passed (probably through a grating) into the third compartment at the same level, and thence rose through a hole in the roof of this compartment into the fourth, which was above it, and in which the water, of course, attained the same level as in the first compartment, thence passing on along the aqueduct, having deposited a good deal of its suspended matter in the two lower compartments of the piscinæ. Arrangements were made by which these two lower compartments should be cleaned out from time to time. The specus, or channel, itself was, of course, constructed of masonry, generally of blocks of stone cemented together, and it was frequently, though not, it would appear, always, lined with cement inside. It was roofed over, and ventilating shafts were constructed at intervals; in order to encourage the aeration of the water, irregularities were occasionally introduced in the bed of the channel. The water supplied by the different aqueducts was of various qualities; thus, for instance, that of the Alsietine, which was taken from a lake about 18 miles from Rome, was of an inferior quality, and was chiefly used to supply a large naumachia, or reservoir, in which imitation sea fights were performed; while, on the other hand, the water of the Marcian was very clear and good, and was therefore used for domestic purposes. Frontinus gives the most accurate details as to the measurements of the amount of water supplied by the various aqueducts, and the quantities used for different purposes. From these details Mr. Parker computes the sectional area of the water at about 120 square feet, and says: "We can form some opinion of the vast quantity if we picture to ourselves a stream 20 ft. wide, by 6 ft. deep, constantly pouring into Rome at a fall 6 times as rapid as that of the River Thames." He considers that the amount was equivalent to about 332 million gallons a day, or 332 gallons per head per day, assuming the population of the city to be a million. When we consider that we in London have only 30 gallons a head daily, and that many other towns have less, we get some idea of the profusion with which water was supplied to ancient Rome. But the remains of Roman aqueducts are not only to be found near Rome. Almost every Roman city, whether in Italy or in the south of France, or along the north coast of Africa, can show the remains of its aqueduct, and almost the only things that are to be seen on the site of Carthage are the remains of the Roman water tanks and the ruins of the aqueducts which supplied them. The most beautiful aqueduct bridge in the world, on the course of the aqueduct which supplied the ancient Nemaucus, now Nismes, still stands, and is

called, from the name of the department in which it is, the Pont du Gard. It consists of a row of large arches crossing the valley over which the water had to be carried, surmounted by a series of smaller arches, and these again by a series of still smaller ones, carrying the specus of the aqueduct. This splendid bridge still stands perfect, so that one can walk through the channel along which the water flowed, and it might be again used for its original purpose. There was, however, one city which, from the fact that a great part of it was situated upon a hill, was more difficult to supply with water than any of the rest, and which, at the same time, from its size, its great importance, and the fact that it was the favorite summer residence of several of the Roman emperors, and notably of Claudius, who was born there, and who had a palace on the top of the hill, must, of necessity be supplied with plenty of water, and that, too, from a considerable height. I refer to Lugdunum (now Lyons), then the capital of Southern Gaul. This city was built by Lucius Munatius Plaucus, by order of the Senate, in A. U. C. 711. Augustus went there in A. U. C. 738, and afterwards lived there from 741 to 744. It was he who raised it to a very high rank among Roman cities. It had its Forum near the top of the hill now called Fourvières (probably a corruption of Forum Vetus), an Imperial palace on the summit of the same hill, public baths, an amphitheatre, a circus, and temples. In order to supply this city with water, standing as it did on the side of a hill at the junction of two great rivers (now Rhone and Saone), it was necessary to search for a source at a sufficient height, and this Plaucus found in the hills of Mont d'Or, near Lyons, where a plentiful supply of water was found at a sufficient height, viz., that of nearly 2,000 ft. above the sea. From this point an aqueduct, sometimes called, from its source, the aqueduct of Mont d'Or, and sometimes the aqueduct of Ecully, from the name of a large plain which it crossed, was constructed, or, rather, two subterranean aqueducts were made and joined together into one, which crossed the plain of Ecully in a straight line, still underground; but the ground around Lyons was not like the Campagna, near Rome, and it was necessary to cross the broad and deep valley now called La Grange Blanche. This, however, did not daunt the Roman engineers; making the aqueduct end in a reservoir in one side of the valley, they carried the water down into the valley, probably by means of lead pipes, in the manner which will be described more at length further on, across the stream at the bottom of the valley by means of an aqueduct bridge 650 ft. long 75 ft. high, and 28½ feet broad, and up the other side into another reservoir, from which the aqueduct was continued, along the top of a long series of arches to the reservoir in the city, after a course of about ten miles. In the time of Augustus, however, it was found that the water brought by this aqueduct was not sufficient, especially in summer, and as there was a large Roman camp which also requred to be supplied with water, situated at a short distance from the city, it was determined to construct a second aqueduct. For this purpose the springs at the head of a small river, called now the Brevenne, were tapped, and conveyed by means of an underground aqueduct (known as the aqueduct of Brevenne) which wound around the heads of the valleys, and, after a course of about 30 miles, is believed by some to have arrived at the city, but by others to have stopped at the Roman camp, and to have been constructed exclusively for its supply. I have here a diagram, after Flacheron, showing a section of this aqueduct, and this will give a very good general idea of the section of a Roman aqueduct where constructed underground. It will be seen that the specus, or channel, is 60 centimeters (or nearly 2 ft.) wide, and 1 m. 57 c. (or a little over 5 ft.) high, and that it is lined with a layer of 3 c. (or nearly 1¼ in.) of cement. It is constructed of quadrangular blocks of stone cemented together, and has an arched stone roof. It will be noticed also that the angles at the lower part of the channel are filled up with cement; it appears, also, that this aqueduct crossed a small valley by means of inverted siphons. But neither of these aqueducts came from a source sufficiently high to supply the Imperial palace on the top of Fourvières. Their sources are, in fact, according to Flacheron, at a height of nearly 50 ft. below the summit of Fourvières,

and it was, therefore, considered necessary by the Emperor Claudius to construct a third aqueduct. The sources of the stream now called the Gier, at the foot of Mont Pila, about a mile and a-half above St. Chamond, were chosen for this purpose, and from this point to the summit of Fourvières was constructed by far the most remarkable aqueduct of ancient times, an engineering work which, as will be seen from the following description—partly taken from Montfalcon's history of Lyons, partly from Flacheron's account of this aqueduct, and partly from my own observations on the spot—reflects the greatest possible credit on the Roman engineers, and shows that they were not, as has been frequently supposed by those who have only examined aqueducts at Rome, by any means ignorant of the elementary principles of hydraulics. To tap the sources of a river at a point over 50 miles from the city, and to bring the water across a most irregular country, crossing ten or twelve valleys, one being over 300 ft. deep and about two-thirds of a mile in width, was no easy task; but that it was performed, the remains of the aqueduct at various parts of its course show clearly enough. It commences, as I have said, about a mile and a-half from the present St. Chamond, a town on the river Gier, about 16 miles from St. Etienne. Here a dam appears to have been constructed across the bed of the river, forming a lake, from which the water entered the channel of the aqueduct, which passed along underground until it came to a small stream, which it crossed by a bridge, long since destroyed. After this it again became subterraneous for a time, and then crossed another stream on a bridge of nine arches, the ruins of some of the columns of which are still to be seen, and from these ruins it would appear that the bridge had, at some time or another, been destroyed, probably by the stream running under it having become torrential, and subsequently rebuilt; again it became concealed underground, to reappear in crossing a small valley and another small stream, when it was again concealed by the ground, and in one or two places channels were even cut for it through the solid rock, after which it reappeared on the surface at a point where now stands the village of Terre-Noire, and where it was necessary that it should, somehow or another, cross a broad and deep valley. It ended in a stone reservoir, from which eight lead pipes descending into the valley were carried across the stream at the bottom on an aqueduct bridge, about 25 ft. wide, and supported by twelve or thirteen arches, and then mounted the other side of the valley into another reservoir, of which scarcely any remains are now seen, from which the aqueduct started again, disappearing almost immediately under the surface of the ground to appear again from time to time crossing similar valleys and streams upon bridges, the remains of some of which may still be seen, until it reached Soucieu, on the edge of the valley of the Garonne, where are still seen the remains of a splendid bridge, the thirteenth on its course, nearly 1,600 ft. long, and attaining a height of 56 ft. at its highest point above the ground. The object of this bridge was to convey the channel of the aqueduct at a sufficient height into a reservoir on the edge of the valley. The remains of this bridge leave no doubt that it was purposely destroyed by barbarians; some of the arches near the end of it remain, while the rest have been thrown down, some on one side and some on the other, but happily the arches next to the reservoir, at the end of the bridge and on the edge of the valley, remain, and the reservoir itself is still, in part, intact, supported on a huge mass of masonry. Four holes are to be seen in that part of the front of the reservoir which is left, being the holes from which the lead pipes descended into the valley. There must have been nine of these pipes in all. These holes are elliptical in shape, being 12 in. high by 9½ in. wide, and the interior of the reservoir is still seen to be covered with cement. The walls of the reservoir were about 2 ft. 7 in. thick, and were strengthened by ties of iron; it had an arched stone roof in which there was an opening for access. From this the nine lead pipes descended the side of the valley, supported on a construction of masonry, crossed the river by an aqueduct bridge, and ascended into another reservoir on the other side, as seen on the plan, entering the reservoir at its upper part just below the spring of the arches of the roof. From this reservoir the aqueduct

passed to the next on the edge of the large and deep valley of Bonnan, being underground twice and having three bridges on its course, the last of which, the sixteenth on the course of the aqueduct, ends in a reservoir on the edge of the valley. Only one of the openings, by which the siphons, of which there were probably ten, started from the reservoir, is now left. The bridge across the valley below had thirty arches, and was about 880 ft. long by 24 ft. wide. A number of the arches still remain standing, and, as will be seen by the photograph, in some instances the pillars of the arches were constructed of transverse arches themselves. The work consisted of concrete, formed of Roman cement, so hard that it turns the points of pickaxes when ememployed against it, with layers of tiles at regular intervals. The surface of the concrete is covered with small cubical blocks of stone, placed so that their diagonals are horizontal and vertical, and forming what is known as *opus reticulatum*. After crossing the bridge the pipes were carried up the other side of the valley into a reservoir, of which little remains, and then the aqueduct was continued to the next valley, passing over three bridges in its course. This valley, that of St. Irenée, is much smaller than either of the others, but nevertheless it was deep enough to necessitate the construction of inverted siphons, of which there were eight. Leaving the reservoir on the other side of this valley, the aqueduct was carried on a long bridge (the twentieth on its course), which crossed the plateau on the top of Fourvières and opened into a large reservoir, the remains of which are still to be seen on the top of that hill. From this reservoir, which was 77 ft. long and 51 ft. wide, pipes of lead conveyed the water to the Imperial palace and to the other buildings near the top of the hill. Some of these lead pipes were found in a vineyard near the top of Fourvières at the beginning of the eighteenth century, and were described by Colonia in his history of Lyons. They are made of thick sheet lead, rolled round so as to form a tube, with the edges of the sheet turned upwards, and applied to one another in such a way as to leave a small space as shown in the diagram, which was probably filled with some kind of cement. These pipes, of which it is said that twenty or thirty, each from 15 ft. to 20 ft. long. were marked with the initial letters TI. CL. CAES. (Tiberius Claudius Cæsar), and afford positive evidence that the work was carried out under the Emperor Claudius. Lead pipes, constructed in a similar manner, have also been found at Bath, in this country, in connection with the Roman baths. The great difference between this aqueduct and those near Rome arises from the fact that, instead of being carried across a nearly flat country, it was carried across one intersected with deep ravines, and that it was, therefore, necessary to have recourse to the system of inverted siphons. There can be no doubt that the inverted siphons were made of lead, although no remains of them have been found, for we know that the Romans used lead largely, and, as we have seen, pieces of the lead distribution pipes have been found. It is possible, and even likely, that strong cords of hemp were wound around the pipes forming the siphons, as is related by Delorme in describing a similar Roman aqueduct siphon near Constantinople; Delorme also describes, in the aqueduct last mentioned, a pipe for the escape of air from the lowest part of a siphon carried up against a tower, which was higher than the aqueduct, and it is certain there must have been some such contrivance on the siphons of the aqueduct at Lyons. Flacheron supposes that they consisted of small pipes carried from the lowest part of the siphons up along the side of the valley and above the reservoirs, or, in some instances, of taps fixed at the lowest part of the siphons. The Romans have been blamed for not using inverted siphons in the aqueducts at Rome, and it has been said that this is a sufficient proof that they did not understand the simplest principles of hydraulics; but the remains of the aqueducts at Lyons negative this assumption altogether. The Romans were not so foolish as to construct underground siphons, many miles long, for the supply of Rome, but where it was necessary to construct them for the purpose of crossing deep valleys they did so. The same Emperor Claudius who built the aqueduct at Rome, known by his name, built the aqueduct of Mont Pila, at Lyons, and it is quite clear, therefore, that his engineers were prac-

tically well acquainted with the principles of hydraulics. It is thus seen that the ancient Romans spared no pains to obtain a supply of pure water for their cities, and I think it is high time that we followed their example, and went to the trouble and expense of obtaining drinking water from unimpeachable sources, instead of, as is too often the case, taking water which we know perfectly well has been polluted, and then attempting to purify it for domestic purposes.

THE PHYSICAL BASIS OF PHENOMENA.

By H. H. BATES.

From the Bulletin of the Philosophical Society of Washington.

If there is anything entirely disheartening, it is to see the few landmarks of human achievement disappear before the shifting current of opinion, as headlands disappear under the ceaseless buffeting of the ocean. It is no doubt a matter of poignant regret to the cherisher of ardent theological convictions to see the bulwarks of faith slowly undermined by controversy. So, also, to him who has built his convictions on supposed demonstrable and irrefragable fact, to find nothing unassailable, not even the axioms and postulates conceded for ages as first principles, on which the fabric of science was reared, nor the sublime inductions of Galileo and Newton, on which the modern philosophy called natural—the only fruitful philosophy which man has produced—has been founded.

But the course of criticism shows that there are no first principles. Nothing is unquestionable. Even the mathematic joins hands with the metaphysic. I propose briefly to examine the fundamental grounds of mechanical philosophy, in view of the wide divergence of basal hypotheses in recent years, and especially on account of the importance conferred upon certain speculations by their admission into works of standard reference and authority.*

To do this aright it is necessary to go behind the mere sub-science of mechanics to the essence and substance of things, as did the eighteenth-century philosophers succeeding Newton. The observational data which have accumulated since that time by the splendid efforts of the molecular physicists enable us to review and recast, with some promise, the primary dogmas regarding the physical basis of phenomena. It is legitimate to frame hypotheses on subjects which are still unfathomed, but which confessedly do not belong to the domain of the unknowable. The distinguished example of the authors of the vortex atom would alone justify such a conclusion.

No entirely satisfactory hypothesis of the atom has yet been found. I do not design to discuss the vortex atom here at length; for, although it is the most successful form of the Cartesian doctrine of vortical substance, it has not been perfected, and is generally regarded rather as an example of remarkable speculative and mathematical ingenuity, than as a discovery corresponding with any facts of objective physics. It has insuperable difficulties, some of which have been pointed out by Clifford, and others by Clerk-Maxwell. Moreover, unparticled or continuous substance, the necessary postulate in this hypothesis, is something we not only have no experience of, but find full of inconsistencies with experience, when we gain a clear conception of what it implies. Such a conception fulfills Hegel's paradox that being and non-being are the same, since it forbids all mobility, all differentiation, as was perceived by the followers of Democritus. It simply affords an inviting basis for analytical discussion, on account of the elimination of the very conditions of objective existence which make the mathematical difficulty.

There are some postulates regarding substance which we may probably be permitted to assume at the outset. We may postulate its objectivity, and also its dis-

* Encyclopædia Britannica, 9th ed., articles "Mechanics," "Measurement," etc.

continuity. I have no space to review here the time-worn controversy between continuous and discontinuous substance. The arguments, which are exhaustive from the metaphysical side, are as old at least as Democritus and Anaxagoras. Suffice it to say that modern experiential philosophy has decided the battle experimentally in favor of the discontinuity of matter. The dispute only lingers in the region of the atom, where observation cannot penetrate or has not penetrated. The inability to conceive which attaches to all non-experiential affairs is encountered here, coupled with the too great facility of conceiving what is superficially observed, but will not bear analysis. Thus our first impressions of substance are in favor of its continuity. It is only after much reflection that we get the idea of necessary discontinuity, as bound up with the exhibition of existing phenomena. But the wonderful development of the Cartesian mathematics, in conjunction with the infinitesimal calculus, and its great facility in dealing with geometrical continuities, has tacitly revived the Cartesian idea regarding the nature of matter, as synonymous with space relations, which never reached intelligible development at the hands of its author, and wholly declined and disappeared after the establishment of the Newtonian philosophy, and the discovery of the discrete character of substance.

In point of fact, experience would point to extreme porosity or discreteness as characteristic of substance, rather than to its opposite—perfect continuity. The infinite divisibility of space has nothing in the world to do with the question, though this is a confusion often fallen into. On the contrary, there is an infinite distinction between the infinitesimal discrete units of substance, occupying extension by their interactivity, and the passive infinitesimal resolvability of space continuity. This is the antipodean difference between the Epicurean and the Cartesian conceptions; the former admitting of the operations of force, the free exhibition of motion, the organization of material phenomena, which are phenomena of mobility; the latter constituting a plenum, with only ideal divisions, and phenomenally as necessarily barren a negation as space itself.

Substance is purely experiential. In its essence it is still incomprehensible, because experience has not yet reached down to those recesses. We know nothing of substance except by its manifestations. These manifestations are cognized by us through sense impressions, weighed, compared, adjusted, and analyzed in the mysterious alembic of the mind. First impressions have enormous predominance, and are intensified by heredity of cerebral predisposition and function.

We cognize substance only in bulk by direct perception, and these vast aggregations stand in thought for matter. A drop of water contains incomparably more molecules than the ocean contains drops; a grain of sand more particles than the earth contains grains; and it is this vast mesh of complicated forces that forms the integrated concept of matter to our apprehension. The child, before he can walk, encounters obstacles to movement, reaction to his every muscular effort, of equal measure to his own; and thus his first and profoundest convictions of objective existence are associated with resistance, opposition, repulsion. This impression of matter is so early that it remains with us as its most natural and obvious characteristic.

The idea of weight is also one of the earliest experiences. This idea would not be conceivable to a denizen of the deep sea, for our first ancestor who emerged from the water gained the experience at the cost of great struggle and enterprise. By the natural development of muscle and function the child rears itself very early against the constant pull of our pedestal, triumphs over it with new-found energies, dances on tiptoe, and spurns the ground, but is soon content to draw the battle, to wander around a few weary years on equal terms, at length to call in the aid of a stick or crutch, and, finally, to resign the unequal contest, and sink, vanquished and satisfied, to rest in its bosom. Weight thus seemed a natural characteristic of matter until identified and generalized by Newton as a universal and especially a reciprocal property. This generalization transferred the property, in conception, from the naturally heavy body to a cause outside thereof, namely, the earth itself. Here the human mind relucted, for, unlike repulsion, attraction is not an observational fact. All forms of tension, stress, con-

straint—by whatever name called—are attended in the child's experience with an intermediary connection. The string is necessary to pull the cart, and the action of the magnet upon the iron particles is viewed with astonishment and awe. The sense of mystery does not proceed so far in his case as to contemplate the equally mysterious power which makes his string differ from a rope of sand. The most profound attention of the human mind has not yet fathomed this mystery.

Inertia or mass is a less obvious property, being in early observation and in common apprehension bound up with weight. It was not recognized in philosophy till Galileo's time, nor is it now by the common perception, except after training. A lady makes no scruple of asking to have a loaded car or train or vessel stopped at a given point on the instant, and reinvested with motion any number of times; and would-be inventors often contrive theoretical machines having numerous heavy reciprocating parts timed to velocities impossible of execution. With beings under other conditions it is wholly different. The sword-fish, *e. g.*, can have no conception of gravity, as he has no perception of it, but his apprehension of inertia is finely cultivated, through the muscular sense, in setting up and modifying the rapid movements in which his existence delights, as well as through his vivid realization of momentum, in the piercing of a whale or a vessel, by which his function is so powerfully exhibited. When once realized by human perception, however, inertia becomes identified with substance as its most primary characteristic.

The old scholastic property of impenetrability. also, is one of the superficial notions of experience, gained in the same way as that of repulsion. It seems to pertain to solids—the typical matter—with approximate accuracy, though calcined plaster of Paris and water, *e. g.*, will occupy a good share of each other's volume, and still form a highly porous solid. But a quart receiver full of hydrogen can have a quart of carbonic acid gas deftly introduced into it as into a void space; and so can a quart of water, at ordinary temperature and pressure, according to Gmelin, without increase of volume, although water is the type of material continuity. As to impenetrability in the molecule, we can predicate nothing. The evolution of heat in chemical combinations indicates penetration of volume, with reorganization of the molecule in less space; and there is no reason, except a scholastic one, why two or more molecules or even atoms, should not occupy the same place, as admitted by the highest authority—James Clerk-Maxwell.

Dimension is also a common notion, derived similarly from superficial and early experience. Solids alone have figure and assignable dimension, though liquids have fixed volume, and gases variable volume, in inverse ratio to constraint; but even solids are of varying and fluctuating dimensions, according to temperature, density, etc. Solidity and liquidity are, it is well known, but mere transitory conditions of material aggregation, for all matter is capable, by sufficient accession of molecular motion, of assuming that hyperbolic or expansive condition which we call gaseous, and in this state dimension and impenetrability are meaningless terms. Concerning dimension, as a necessary attribute of the unit of mass, Clerk-Maxwell says (Encyclopædia Britannica, 9th Ed., Vol. 3, p. 37): "Many persons cannot get rid of the opinion that all matter is extended in length, breadth, and depth. This is a prejudice . . . arising from our experience of bodies consisting of immense multitudes of atoms." That there is no necessary relation between mass and volume as there is, *e. g.*, between mass and weight, is shown to common experience by the notably different masses of a buckshot and a pith-ball of the same dimensions, or of a cannon ball and a child's hydrogen balloon. A pellet of iridium equivalent in mass to the pith-ball might be microscopic, and, by extreme supposition, infinitesimal. We are not forced, however, to deny to the unit of mass finite magnitude, as this would be an experiential fact when ascertained.

The remaining so-called properties of matter are too obviously transitory, accidental, or derivative to require attention. Color, luminosity, opacity, transparency, sapidity, sonority, odor, texture, temperature, diathermancy, plasticity, hardness, brittleness, density, compressibility, conductivity, malleability, fusibility, solubility, and many others, are too clearly but conditions of aggregation, or else mere

subjective states due to the way the complicated interactions of the primary qualities affect our senses. What are the primary qualities?

Here is where the modern method of philosophy flags, by the disappearance one by one of the experimental means of approach, as we eliminate the non-essentials. But though the substance is thus elusory, we cannot yet believe it to be illusory.

Chemical and molecular physics have already gone marvelously beyond the ordinary range of sense-perception, by strictly scientific methods. Not only is the discrete character of matter established, but many data of the differentia and organization of the molecule are discovered. Here is a vast field of science in itself. From the ideal molecule, or simple couple, up through the 70 actual organized molcules of our provisional elements, then the chemical molecules of their combinations in vast numbers, discovered and undiscovered, and, lastly, the enormously complex organic molecule in infinite variety, the domain transcends in area for classification that of biologic science. The simple molecule has not yet been discovered, much less the molecular constituent, the atom, or the *indivisible.* It is evident, however, that the properties of matter which are essential, not differential, must reside in the atom. The philosophers succeeding Newton, treated the atom and the elementary molecule as one, from lack of sufficient chemical knowledge. We are on a higher plane of information, but their method is not necessarily vitiated by such lack of distinction.

We cannot, as before said, attribute *a priori* to the atom dimension or figure, though we postulate it to aid conception. As the atom is an absolute unit, there is incongruity in finally assigning to it such relative attributes, which are but matters of comparison and degree. There are properties, however, which are inseparable from an absolute essence. These are the properties by which the essence is manifested to us. We know them provisionally as forces, in the Newtonian nomenclature. Had gaseous matter neither weight nor mass, we could not know of its existence. But these attributes are so constant in matter that we estimate its quantity in terms of them and have no other exact terms. Weight is the statical measure; mass the dynamical measure. And since weight and mass correspond for all substances, under all transformations, we judge that the correspondence identifies them alike with the essence. They cannot be the mere result of organization. They must belong to the ultimate atom.

At this point it would seem proper to attend to a question of definition. Definitions are essential to clearness, on the one hand, and a source of entanglement on the other, if we fall into the scholastic error of regarding a mere word as the coextensive symbol of an idea. Words are evolved during the imperfection of ideas, and language is still a most imperfect medium of expression. Hence, logic is not a science in the sense that mathematics is. I have used the term force. This is a word of much ambiguity of meaning. We may use it as a convenient mathematical expression for a mere rate of change of momentum, or we may go farther and *define* it as that which changes a body's state of rest or of uniform motion in a straight line; either of which uses restricts it to only a portion of phenomena, and ignores the whole science of statics, dealing with forces in equilibrium and the phenomena of balanced stress. If we give it a more general signification, as that which changes or tends to change, or conserve, the state of motion of particles, or systems of such, either in quantity or direction, we embrace statics as well as kinematics, and get a measurably philosophical definition, if we bear in mind the proviso that we do not thereby postulate force as an entity apart from substance.

And since the compound variable space and time condition, which we call motion (of which rest is but a phase), is the sensible resultant of the interaction of such discrete substance by constant rearrangement where readjustment is free, or the potential resultant where confined, we may admit that the observed tension and persistence, of whatever form, is that which effects the phenomenon (though masked by infinite variety and composition), and always across the discontinuity; not as separate entities, but as modes of manifestation of the interacting and pervasive substance itself and its only manifestations. This we call *force*—the inscrutable agent of phenomena—and this I take to be the true Newtonian conception, as evinced by his maturest conclu-

sions, expressed in query 31 appended to his "Optics."

So far as weight goes, it was generalized by Newton to be a reciprocal force or stress, operative without limit on the law which inheres in radial space relations —the inverse square of the distance. The term operative means effective upon mass, namely, bridging the discontinuity. Gravity is the typical attractive force— *vis centripeta*. The relation is mutual by the law of action and reaction, and amounts to a universal tention among particles, controlling all matter everywhere into orderly movements and relations. This is what we postulate from observation, on the Newtonian plan of naming simply what we see. The notion, however, of action at a distance has encountered a metaphysical difficulty in many minds, from the preconception derived from ordinary experience that all affections or stresses must proceed through an intermediary connection, deemed continuous. Even Newton made concession to this prejudice in his oft-quoted letter to Bentley. That there is really no such continuity in any mode of connection known is demonstrable, and the notion itself that the fancied continuity of some rare effluvium could in any way aid the mechanics of the problem is chimerical. Clerk-Maxwell, moreover, has shown (Encyclopædia Britannica, Vol. 3, p. 63) that action at a distance is as necessarily implied in repulsion as in attraction, so that theories of repulsion do not aid conception. Ability or inability to conceive, furthermore, is not held even by the metaphysicians to be a criterion of objective truth. Such truths exist independent of the conceiving mind. The conceiving organ was evolved by experience, and conception develops with attention. The first law of motion was wholly inconceivable to the contemporaries of Galileo, and we find such instances even now. Thus, while plain truths are inconceivable until established, some utter absurdities have been deemed conceivable, as, for instance, vacuity of two dimensions. State of mind, then, is no measure of external truth.*

The second force or manifestation of the atom, inertia—or mass—unlike gravity, is not unlimited in range of action. As to this property matter is discrete. Mass has both a *locus* and a limit (being apparently dependent for dimension on multiplicity), and amounts to that incomprehensible property by which conservation of motion is maintained. Under gravity, quantity of motion varies according to relations of contiguity, but under inertia motion is conserved in direction and quantity, is modified in direction and quantity by interaction of mass with gravity, and is redistributed by interaction with repulsive force upon an indefinitely near approach of particles, upon conservative principles. Its discreteness gives matter its numerical and finite character, and admits of that interplay which constitutes phenomena. Its reality and primary character, when once apprehended, have proved more acceptable to the imagination than has the conception of central force, and under appulsion hypotheses (with the aid of that other readily accepted property, repulsion, and certain highly artificial hypothetical media), it has been made to do duty in providing so-called explanations of gravity, under its form of *vis viva*.

It has always seemed to me that the mode of approach adopted by Boscovich was the most philosophical and rigorous of any. He viewed matter for the purposes of mathematical treatment and for investigation of its essentials, as divested of accidental and fugitive properties; and as the analytical calculus had not then become so developed as to wholly fasci-

* In this connection, to illustrate how entirely a matter of opinion or prejudice or culture is this notion of conceivability, I quote from a letter written by Faraday to Dr. Playfair, in response to some inquiries of the latter about his atomic opinions:

. . . "I believe in matter and its atoms as freely as most people—at least, I think so. As to the little solid particles which are by some supposed to exist independent of the forces of matter, and which in different substances are imagined to have different amounts of these forces associated with or conferred upon them, . . . as I cannot form any idea of them apart from the forces, so I neither admit nor deny them. They do not afford me the least help in my endeavor to form an idea of a particle of matter. On the contrary, they greatly embarrass me: for, after taking an account of all the properties of matter, and allowing in my consideration for them, then these nuclei remain on the mind, and I cannot tell what to do with them. The notion of a solid nucleus without properties is a natural figure or stepping-stone to the mind at its first entrance on the consideration of natural phenomena; but when it has become instructed, the like notion of a solid nucleus apart from the repulsion, which gives our only notion of solidity, or the gravity, which gives our notion of weight, is to me too difficult for comprehension; so the notion becomes to me hypothetical, and, what is more, a very clumsy hypothesis." (Playfair's works, vol. 4, p. 84.)

Here we see a difficulty opposite to that usually encountered, for, while many people express an infirmity of conception of the forces apart from the imaginary vehicle, Faraday finds the vehicle of no use as a carrier of the properties, but a positive impediment.

nate the attention of geometers with abstract and ideal relations, he proceeded from prime physical data. He thus identified matter by those apparently general and characteristic properties recognized by Newton as the basis of mechanical philosophy in conjunction with the laws of motion. These properties are, as before said, gravity, inertia, and repulsion; or, as characterized by function, attraction, conservation, distribution. In this view, matter consists of certain *loci* of central forces, mutually attractive by the first property according to a variable law in the duplicate inverse ratio of distance without limit, but restricted in manifestation as to the second property to the infinitesimal *locus*, thereby excluding unitary dimension. Contemplating matter under this aspect alone, a dilemma arose. For gravity waxing by the law of inverse squares of the distance up to the focus or origin, involves the consideration of infinite force and apparently of infinite velocity in the limit, in the supposable case of rectilinear approach, at which point the equations become unexplainable. While Euler and La Place differ in their interpretations of the result, Boscovich sought to solve the apparent absurdity and inconceivability by the invention of his ingenious and complex system of alternate spheres of attraction and repulsion, or change of sign, on a very near approach, with infinite repulsion at the focus, which so loaded down and vitiated his hypothesis as to cause its rejection. This result was similar to that of Le Sage's speculations and those of the Ptolemaic astronomers, each thus working out the falsity of his respective scheme by superadded complications to readjust the theory to the progress of criticizm or of observed fact.

By attributing finite magnitude to the atomic mass, however, Boscovich's difficulty disappears, as I had the honor of pointing out before this Society some ten years ago. This may be deemed a violent hypothesis in regard to a positive discrete simple absolute, as the atom is presumed to be, but parallel difficulties inhere in any other finite supposition, as, *e. g.*, a sphere of repulsion. Under my provisional assumption, the way out follows from an elementary proposition of Newton's, and it does not demand the gratuitous change of law or of continuity involved in the resort of Boscovich. The movement of a gravitating particle under stress of a center of gravitative force would be in all respects as the great 18th-century mathematicians have demonstrated, until the margin of the particle reached the attracting center, where, if we suppose the attractive virtue to pervade the particle equally throughout a certain finite volume of mass, however minute, as gravity does the mass of a sphere, the maximum of attractive force would be attained; for, as Newton has shown, homogeneous spheres are controlled under gravity by a law of force varying directly as the mass and inversely as the squares of a distance between their center of mass and the attracting center, at all points *beyond* the surface, and directly as the distance between the said centers *within* the surface; so that after passing the surface, the attractive center must proceed onwards to the gravitating center of mass (relatively), not by a force increasing to infinity, but by a force decreasing to zero, after passing the maximum, since it is balanced at the center by opposing stresses.

A similar law of attraction prevails between two gravitative particles when both are similarly endowed with finite spherical volume and mass, excluding the idea of impenetrability (which is not a necessary attribute of mass), the Newtonian law being the product of the masses divided by the product of the distances $\left(\frac{Mm}{dd}\right)$* for outside positions.

For positions of encroachment the law

* I write the formula this way because it is possible that we have been in error all along in regarding the denominator as a radial space relation, as implied when we write it $\frac{Mm}{d^2}$. In discussing the deflection of the particle under gravity, Newton, for matematical simplicity, treated it as governed by a fixed attracting central force, and in testing various relations found that the radial space relation gave the true path of the planetary bodies under the immense preponderating influence of the sun's mass. The fixed center of attraction is, however, a mathematical, not a physical condition, and can only be realized by making $M=\infty$, when we get a form of expression which does not give a law of force. I think it possible that the relation is a mere reciprocal distance relation, since the stress is mutual for the masses and each is equally distant from the other. The inverse form of the relation, moreover, may arise from our subjective way of viewing distance, as measured outwardly from ourselves, since we have to go from here to yonder. It is possible to look upon the relation as really one of contiguity or nearness, and by placing $\frac{1}{d}=c$ we get the cosmical law of gravitation as $Mcmc$. This, however, would not be a useful formula, since we are not accustomed to expressions which attain maximum value with minimum magnitude.

is more complicated, and forms an interesting field for mathematical discussion. Where three or more atoms are superimposed the problem becomes too complex for discussion. It is noted, however, that such compound atom, if quiescent from extreme abstraction of heat, would be in a condition of elastic equilibrium, ready to respond like a bell to the slightest disturbances. In all these cases of interpenetration the law of stress would be finite and diminishing, and if the line of encounter should chance to be a right line through their centers (a condition infinitely rare in actual occurrence), they would continue on or repeat according to energy of approach; while upon any other lines of approach orbital relations would supervene, in modified curves of the second order, either hyperbolic, parabolic, or elliptic, according to velocity, and with or without partial penetration, according to nearness of approach.

Boscovich, however, did not adopt this solution, although within his reach. The problem of the action of a gravitative particle as controlled by an attractive center has several aspects of statement, which may be confined to four, for practical investigation. In the first, where the particle is assumed to be without mass, no discussion is possible, for the two suppositious points instantly assume the same locality, and end the relation. In the second, where the particle is endowed with inertia but not magnitude (and the attractive *locus* fixed by postulate), the element of motion enters, but infinite terms appear in the equations in the limit, forbidding interpretation. Th'rdly, when we attribute finite magnitude to the gravitative particle for gravitative pervasion, as in actual spherical masses, no infinite terms appear, and we get an intelligible mathematical discussion, with planetary results for exterior positions, and pendulum results for interior positions, as I have heretofore demonstrated; and lastly, when both the gravitating *loci* are invested with similar attributes of volume and of mass (excluding extraneous notions of ordinary collision and repulsion from the problem), the results are similar to those of the third hypothesis. I do not introduce any of the mathematical discussions here, as the dynamics of the particle have been fully treated by mathematicians, though I am not aware that any of them have pursued it to physical conclusions.

It is not likely, however, that there is any matter so simple as this modified Boscovichian atom; that is, which can be identified. All the matter we know of is already compounded and highly organized. The ideal simple molecule would consist of a single pair of such atoms, bound to each other in orbital relations of more or less eccentricity, including the extreme rectilinear form of simple pedulum-like oscillation through one another's centers; and it is a most significant fact that spectroscopic observation of all incandescent matter shows atomic matter to be in this state of transverse or orbital oscillation with inconceivable but synchronous rapidity without regard to range, according to the pendulum law of stress varying directly as the range of occillation, discovered by Galileo. Any theory of the simple molecule must take cognizance of this observed fact. Another cognate fact is that the law of elastic cohesion manifest in all elastic tensile action—"*ut tensio sic vis*"—is a parallel law of stress, as illustrated in the spring balance weighing scale, the spring dynamometer, the isochronous spring governor, etc., and is a function of molecular and ultimately of atomic force and distance.

If the atom is really thus characterized, the repulsion or resistant property experienced in matter becomes worthy of investigation, since it drops out as the primitive affection or disaffection postulated by Boscovich. I have shown that it is not necessary to oscillatory motion. We must admit that the notion of rebound or recoil, in the ordinary sense, between simple atoms possesses difficulties. No less does the idea of plasticity or destruction of momenta. Consider what is involved in the hypothesis of two absolutely hard, rigid, unparticled, homogeneous spherical bodies of any magnitude at all, if possessed of mass, meeting on a rectilineal central line of motion. We know what would happen in case of ordinary spherical elastic masses or aggregations of molecules. Such merely undergo, first, apparent contact, then compression, deformation, strain, accumulation of stress, retardation of velocity, momentary arrest, acceleration on new lines of departure, relief of strain, recovery of form, redistribution of momenta, and final resumption

of uniform velocities, with relative motion inverted and aggregate energy of motion unimpaired, unless permanent distortion and heat have absorbed a portion. All this complex action is involved in the term elasticity. None of this could take place with simple undifferentiated particles, unless we invent for them a mystic atmosphere or cushion of repulsive capacity surrounding the *locus*, as Boscovich was forced to do by logical conclusions. Without this, contact would be absolute and instantaneous at first impact. As hardness involves impenetrability, absolute destruction of motion on the instant must ensue; that is, motion and no motion at consecutive instants of time; a discontinuity unknown to experience, and known to be inconsistent with the nature of motion and of time. This argument from breach of continuity is due to Leibnitz. Conversion into heat motion is excluded, heat being a mode of motion of the entire atom. Moreover, the destroyed motion has to be recreated instantaneously in new directions, for destruction of energy cannot be postulated. This geometrically angular motion is also unknown to experience, for all deflected bodies pass by continuity from motion in one direction into a new direction, and so far as we can see, must do so. These discontinuities in translatory relations are therefore put aside, not because they are inconceivable, but as illogical and non-experiential. Simple repulsions by contact without occult intervention is a false suggestion, and we find that we get the pseudo-conception from our false observation of what occurs in the collision of sensible masses, somewhat as we make a false observation and generalization about material continuity, or about tension, from a superficial perception of matter; thus creating concepts from supposed experience which can have no true objective counterparts. I shall recur later to a possible derivative basis for repulsion.

It is remarkable that to Newton we owe the final establishment of the majority of those fundamental and universal truths which by simplicity and generality seem to touch the absolute; that is, more than to any and all other philosophers combined. Thus, of the six ultimate generalizations, four were formulated and placed on an impregnable basis by Newton: the three laws of motion and the law of gravitation. All of these were inconceivable when first promulgated, were hotly controverted on the metaphysical plan, were finally established experientially, and are now generally accepted as axiomatic by the modern mind, except for sporadic reversions which appear now and then to deny their actuality and reassert their inconceivability. The remaining two universal inductions are the collective group of axioms formulating the relations of extension—the only enduring remnant of the Greek philosophy—and the law of the conservation and unity of energy, unperceived in Newton's time in its generality, though taught as a dogma by the Cartesians. These also are still held to be inconceivable by certain disciples of metaphysical methods and axiomatic by others. Such mental attitudes should lead us to believe that simplicity has been arrived at in all these cases and the boundaries of explainable knowledge reached, where inconceivability necessarily begins.

It has been said that paradox is born either of confusion of thought, or of knowledge, or confusion of statement arising out of the imperfection or subtlety of the verbal vehicle of thought. Thus, as Clerk-Maxwell points out, the celebrated arguments of Zeno of Elea, establishing the inconceivability of motion, represented in the paradox of Achilles and the tortoise, were unanswerable and unanswered until Aristotle showed, some half-century later, that duration is continuous and incommensurable by numerical methods in the same sense that extension is. The old logical dilemma of the irresistible force encountering the immovable body was insoluble to the Greek mind, both from lack of physical knowledge and lack of verbal clearness of statement. The acute sophist knew not the nature of force, the constitution of bodies, the conservation, transformation, and dissipation of energy, and consequently knew not the refuge and escape from the dilemma contained in the perception of the conversion of molar energy into heat energy, expansion, and dissipation. The resources of verbal subtlety and of inner consciousness failed, as they always do. Something of the same difficulty remains in modern problems, where observation and strict verification are, from the nature of the problem, inappli-

cable, or where the confusion arises from the still-existing imperfection of language, or, again, where generalizations, both clearly made out and clearly formulated, have not passed into the instinctive popular apprehension. The modern dilemma of the inconceivability of infinite or finite space is, I take it, due to the metaphysical form of the statement. For when we reflect that the ideas of immensity and of infinitesimal resolvability are but abstract generalizations of the merely relative continuities, extension, distance, and dimension, which are in their turn but abstractions of the sense-perceptions, form, translation, and volume, the statement becomes intelligible and entirely conceivable, and I think, though, with deference, saves geometry; that is, the universality of that system of inductive postulates regarding the relations of extension and inferences therefrom, known as geometry to the Greek philosophy, but now named Euclidean by certain analysts whose so-called geometry is symbolic. Geometry is therefore able to deal with all aspects of extension, without regard to limit, in spite of some infirmity in the Greek methed, for scale cannot affect the generality of extension relations, and abstract unconditioned space is not an entity, but a mere negation, concerning which relative propositions are unintelligible. A false philosophy regarding space is at the root of all modern heresies concerning geometry and mensuration, founded in misapprehension of the Euclidean inductions or generalizations.*

The first law of motion is but the formulated recognition of inertia, which is only manifest in conjunction with motion, actively or passively. It was known to Galileo, and laid down by Descartes as a law in his "Principia." It is a cosmical truth, bound up with the absolute nature of mass and the true relations of extension, which correlates the whole fabric of dynamical knowledge with rectilinear geometry, curvilinear motion being demonstrably not a simple state of conservation under inertia, but a resultant of multiple forces. The simple action of mass under the first law of motion, if undisturbed, furnishes the absolute unreturning rectilineal path which overthrows all speculation about possible ideal spaces. I here recall a book written by a learned American of Philadelphia—learned, that is, according to the medieval standard of the colleges—and published only during the past year, entitled "An Examination of the Philosophy of the Unknowable, as expounded by Herbert Spencer," wherein he naively lays down the first law of motion as unintelligible except by appulsion. Motion, he says, in the absence of propulsion is inconceivable. I have no space here to reproduce the explanation evolved out of consciousness by this reasoner to account for the action of a ball struck by a bat after leaving the bat. It resembles in ingenuity and gratuity some of the inventions devised to explain gravity. The notable thing about it is that here, at this date, is a mind of good caliber, informed in the higher schools of learning, which is still of the mental period of Aristotle; a mind which has evidently never apprehended inertia, nor heard of the great contributions to knowledge made by Galileo and Newton, by which philosophy was entirely revolutionized.

The second law of motion, regarding the independence and coexistence of motions, on which we occasionally see comments in the metaphysical vein controverting its possibility, has long been established experientially. Its early experimental proof is attributed to Galileo. Yet I recall a pamphlet written and published only during the last year by a learned German at Leipzig, the theme of which was that "the sun changes its position in space, therefore it cannot be regarded as being in a condition of rest." This, he concludes, overthrows the entire fabric of Copernicus, because the planetary orbits in such case cannot be closed.

The third law of motion is but formulated reciprocal stress, in its modes of

* There are two opposite though similar forms of error in the assumptions regarding space. The first is that space is a specific or perhaps generic entity or objectivity *per se*, possessed of conditions and attributes, like substance, such as dimension (in several), differentia in locality, figure, as curvature, etc. (hence necessarily finite), and only uncognizable by us simply for lack of perceptive faculties to correspond. This is the fundamental error, as it seems to me, of Riemann and Lobatschewsky. The second is that of the older Cartesians, who viewed space as but the mere attribute or synonym of substance, and inconceivable apart from it, so that bodies separated by void space would be absolutely in contact without regard to distance. Both of these speculations are purely metaphysical, and non-experiential, the latter resulting from the old scholastic method of syllogistic deduction from primary postulates of verbal definition, and the former from similar inferences from the forms of the analytical logic of symbols, the use of which is still in the scholastic stage. Like Zeno's paradox, these merely intellectual difficulties should be removable by intellectual processes.

compulsion and repulsion, through which mass acts on mass to redistribute motion by what appears to be necessary law. The stress is necessarily reciprocal, since there is no *point d' appui*, or fixed fulcrum in the universe.

We have thus been brought to the boundary of the absolute, where all is inconceivable until found out, and where the simple data are unexplainable. All examination seems to continue to point to mass and weight as the ineffable simple insignia of substance standing on this limit. We must accept something as elementary fact; what shall we find more elementary? Repulsion is still debatable; for, if we make an issue between repulsion and compulsion as contradictory primary attributes of the same essence, or untenable in conjunction for artificiality, by far the greater difficulties attach to the former, some of which I have already alluded to. The profound mind of Boscovich was forced to accept repulsion as a primal quality, but in deference to the physical hypotheses of his time, he overloaded it with complication. This has been weighed in the balance of the philosophical judgment and found wanting. I have intimated that there are possible grounds for surmising that it may not be a simple property of the atom, but a mere mode of distribution of energy dependent on composition of motion of atomic mass after change of sign, *i. e.*, a mode of *vis impressa* after exhaustion of the space relation; for, mathematically, the hyperbolic lines of approach and recession of two atoms under high proper motion characteristic of the atom, and on lines not directly central, would be similar, at sensible distances, in their asymtotes (which would be the practical paths), whether the deflection were due to attractive or repulsive stress, though acceleration and retardation at the passage of the infinitesimal focus would be inverted.

It therefore seems to me immaterial to result which of the two modes of passing the infinitesimal focus is the true one. In either case the distance at passage is infinitesimal, and the force may be as near infinity as the facts require it to be assigned. The normal or rectilineal encounter is here excluded from supposition. In that case, under repulsive stress, as postulated by Boscovich, the recoil would be rectilineal and opposite, without breach of continuity. Under attractive stress, with finite volume of the atomic mass, penetration would ensue as before shown; but without dimension or repulsion we have an insoluble condition, although the occurrence would be infinitely rare. Only one pair of elements is here considered. In all real encounters, whether of masses or molecules, the effect is a vast resultant, but should not be different in kind from that of the elements; that is hyperbolic or expansive between alien systems under motion. As the number of elements ordinarily engaged could not be represented by any numerical places of arabic notation for which we have names, we see the hopelessness of stating the problem mathematically. I therefore do not presume to offer this as an explanation of repulsion, and I confess that to me repulsion is in its mechanism incomprehensible. We know the result experimentally, and that is resistance to penetration, and reaction at insensible distances on an undefined boundary which begins prior to contact and increases in a high exponential ratio as approximation progresses. The contact boundary of any solid—even the smoothest and hardest—resembles the astronomical limb of Jupiter is geometrical indefiniteness. The contact transmitter in the telephone, the whole range of whose phenomena occurs under pressure and so-called contact of varying degrees, illustrates how relative a thing is contact. Under high velocities the distinction between solids, liquids, and even aeriform bodies entirely disappears in repect to repulsive reaction, though this is the most sensible distinction between them under low velocities.

We may, therefore, adopt the conclusion that if any of the apparently simple properties of the atom are to be thrown out as derivative and secondary, presumption points to repulsion as the complex one. We could possibly account for phenomena in a universe bound together by purely tensile stress, but most of the sensible phenomena of solids—cohesion, affinity, tenacity, etc., including nearly all of statics—remain hopelessly unattackable problems under a hypothesis of pure repulsion, like that of Le Sage, or Preston. It is to be noted that the kinetists who freely postulate repulsion and appulsion, without analysis, as a primordial fact, but relect against compulsion or tension, are

forced to the invention of the most complicated and gratuitous mechanism and media to explain the phenomena of gravity, and then without attainment of result. Le Sage's atom is too complicated, even without his suppositious or extra mundane operative machinery; and the vortex atom is but a mere analytical expression for an unproducible condition in a figmentary mathematical plenum.

The thesis that conservation is the characteristic by which we identify objective existence will not bear the test of examination. It is only in the most recent times that such a quality has been known or imagined, and its establishment, both as to matter and energy, is justly viewed as the triumph of modern philosophy. The evocation of matter from nothing and its relegation to nothing, even by the finite will of a wizard, was ever a common and universal notion, which did not at all impair the belief in its present reality and substantiality. We have only to go to Apuleius for this. and it is doubtful if even now the notion of the indestructibility of matter is anything but a scientific conviction, for do we not see numbers of our contemporary fellow-citizens meeting together frequently in our midst to witness feats of materialization out of nonentity by powers akin to those of the sorcerer, without an idea of incongruity? Nor has the essentially modern doctrine of the conservation of energy anything to do with the belief in its reality. Few people apprehend it even now. No philosopher understood it a hundred years ago. Its verity rests on a sufficiently general inductive basis, from the refined and exhaustive experiments of Joule, and the theoretical conclusions of Mayer and Clausius, and it is accepted in the same sense that the law of gravitation is accepted. But the duality of matter and energy to the exclusion of force is a verbal shift, the assumption of which removes no difficulty. Matter, the object, remains unexplained; and energy, the phenomenon, becomes segregated and unintelligible. Energy, in fact, is but mass in phenomenal manifestation, being a product of triple factors, two of which—translation and speed—are not things, but variable and evanescent conditions, and, taken together, constitute motion. Mass is the absolute or persistent factor, but the evanescent character of the variable component—motion—would render the entire phenomenon—energy apparitional, were it not for the distance relation involved in motion, which, under the same inscrutable agency which modifies and saps the motion, renders it potential upon change of sign. This agency, the dynamical source of the manifestation, being central to mass and likewise pesistent and constant, renders the positive and negative potentialities of movement constantly equal, and the actual and potential energies consequently complimentary, from which energy gets its character of conservation.

Energy cannot therefore be that other reality of existence (besides matter), since force is clearly the one reality at the bottom of the manifestation of both, to whose persistence and resistance to change, except through tranformation, the conservation of both is due. This one reality is, in its triple aspect of causation, (1) attraction—the source and modifier of motion; (2) inertia—the conserver of motion; and (3) repulsion—the distributor of motion; or, more correctly, in its aspect of quality: (1) *vis centripeta*—the power of mutual control across distance; (2) *vis insita*—the power of persistence in state of motion impressed; and (3) the distributive power of imparting and acquiring motion by transfer, at minimum distance, which may be called *vis partitiva*, the result of which is Newton's *vis impressa*. Matter thus comes into the world of phenomena by the simple presence of other matter, permitting the exhibition of these comparisons and interactions, involving the conditions of contiguity, distance, position, translation, direction, succession or sequence, and time-rate for the continuous increments, decrements, successions, and uniformities, all bound up in the compound variable continuity—motion. With motion and distance comes the dependent phenomenon—energy—active and potential, which should be a constant, the numerical units of mass being constant throughout immensity, provided the sum of the motions, potential and actual, be constant. This the dynamical theory deduces from the fact of central force (for without force potential motion is ridiculous), and the thesis of the conservation of energy is a dynamical truth or nothing. It is therefore all the more

extraordinary that certain kinetists, who reluct against central force, should have selected, out of all the manifestations of the universe, the variable and conditional product—energy—to be the one reality or objectivity, aside from the undefined hypostasis—matter—as a primordial simple fact at the basis of phenomena. It has been mathematically demonstrated by Mr. Walter R. Browne that the conservation of energy is true if the material system is a system of central forces, and is not true if the system is anything but a system of central forces. In fact, the ordinary theoretical proof of the principle of the conservation of energy assumes the forces acting to be central forces, *i.e.*, reciprocal stresses between units of mass, as recognized by Clausius in his "Mechanical Theory of Heat." Moreover, the entire body of kinetists, who have aimed to supersede gravity or central force, have freely assumed an extramundane supply of motion and energy without regard to conservation, and it is notable that every hypothesis for this purpose yet broached involves the constant expenditure of work without recovery, and postulates the accession of energy in infinite influx from some occult source, of which only a small portion relatively is available or manifest in observable phenomena, thus violating all three of the canons of philosphical ascription—true cause, sufficient cause, and least cause. Such is the power of conception of the unknown in endeavor to explain the inconceivable known.

If the dynamic hypothesis of perpetual transformation of energy could be established as a universal induction, with as much generality *e. g.*, as the statement of the law of gravitation, it would establish and confirm that law, by Mr. Browne's demonstration, as something more than a law, to wit, the necessary constitution of matter as a system of central forces and nothing more, substantially as conceived by Newton and elaborated by Boscovich. At present it is but a dynamic induction, but the theory of gravity is no more. Our appliances are material, and we can deal with molar forces, but only indirectly and inferentially with those which are atomic Conservation is indubitably true of energy in the mechanical and molar sense, under the laws of dynamics and the persistence of force. It is, also, experimentally true, so far as we can trace it, of those less understood forms of energy which are molecular or atomic, the establishment of which was the great glory of Benjamin Thompson, Clausius, and Joule as to heat, and of a multitude of observers as to electrical energy. We infer it as a general truth of these energies (formerly known as imponderables, since they are not manifestations of matter in the concrete), from the fact of their convertibility with other modes of energy which are undoubtedly dynamical, and also from the intimate connection of electrical energy with one of the specific exhibitions of central atomic force—magnetism. Such clews create a warrantable presumption that the phenomena in question will all ultimately be classified among the modes of atomic mass and motion, inductively as well as hypothetically. Possibly in the investigation of these evanescent modes of energy the missing simple particle may come to light. Provisionally, we are entitled to rank them among the mechanical modes of energy, as products of the same material forces, assuming, until the contrary is proved, that some form of matter is concerned in manifestations so correlated by conservation with undoubted material activities.

In including the imponderables within the general dynamical law of conservation, we have to take account of the phenomenon of dissipation, first pointed out by Sir William Thomson. It is true that heat (as well as electrical energy) is strictly correlated with and interconvertible with energy of mass motion, as before stated, but in its final form energy seems to take leave of matter altogether, so far as our perceptions can follow it, and disappear as a material phenomenon (though liable to reappear wherever matter is encountered whose particles are deficient in a like species of atomic motion with that which disappeared; which fact indicates that atomic mass is still a factor, with its inherent property of persistence and transference). The earth and all upon it is radiating heat energy away into space at the constant rate of 500° F. of absolute temperature, more or less; the sun and the visible stars at the rate of many millions of degrees. Much energy also passes off in the luminous form. Of electrical and actinic energies

we know less, and of some we doubtless know nothing. This amounts to a constant drain of the dynamical supply of energy. These final forms, the radiant energies, have a remarkable specific high cosmical velocity of their own, which is a function of something not material, or at least not molar. It is supposable that, in addition to the dynamical source of motion from central forces, and the contraction of systems in dimension which supplies dissipation, there may be an inherent and primordial store of atomic motion. The high proper motion of some of the stars, beyond what can be accounted for on dynamical principles, and the inexhaustible and enormous supply of radiant energy from the visible stars, have afforded grounds for such a surmise, but these speculations do not belong to the domain of mechanics.

And here we must bear in mind that the dynamical theory, in placing these assumed agencies and modes of interaction in causal relation to phenomenal motion, by no means predicates or can predicate anything concerning absolute motion or its cause. The lack of this distinction may have proved a stumbling block to some in comprehending the idea of force. Were it not for the observed dissipation of energy no system could become contracted in dimensions a particle by the interactions of material forces, nor is there now any known way by which the material system can be expanded in dimensions except by the accession of motion from extra-mundane sources, which there is no scientific mode of ascertaining. The sum of motions under the action of forces remains the same, and any change would imply creation or annihilation, which is not ascribable to a material agency. Primordial dimension remains as inscrutable a fact as ever, and primordial motion an unsolved problem.

In conclusion, I know nothing of force except as a manifestation of matter, and nothing of matter except through its manifestations. It is substance that interacts with substance, so far as we know, always reciprocally, and force is but the convenient translation of the terminology invented by Newton to designate these several species or modes of action, in the word *vis*, with its appropriate adjective. He was arraigned by the Cartesians (and virtually is by their modern representatives) as the reintroducer of occult qualities into philosophy, but his statement was "*hypotheses non fingo*," and to a similar charge brought against him by Leibnitz he pertinently replied that it was a misuse of words to call those things occult qualities whose *causes* are occult though the qualities themselves be manifest.

I have adopted gravity as the type of central inherent force—*vis centripeta*—but I would not thereby be understood as excluding from the category of material forces any and all other modes of tensile or constraining force which may be hereafter made out as specific, by the elucidation of such phenomena as affinity, cohesion, tenacity, elasticity, ductility, viscosity, capillarity, polarity, magnetism, etc., now so little understood, any more than I would exclude any form or mode of energy which may be observed, from the category of material phenomena. The Newtonian doctrine of force would not be impaired by such discovery, and its strength lies in the fact that it as readily includes static phenomena—that despair of the kinetist, who has no imaginable hypothesis by which to range them under a form of motion—as it does kinematical phenomena. Statical force (Newton's *vis mortua*) cannot be ignored in a theory of force. The straw that breaks the camel's back—the very lightning that crashes through the sky—are familiar examples of its power made manifest. Its reality may be exemplified by suspending two heavy balls of equal weight at equal heights—one by an elastic cord, and the other by a tense string. The difference of effort required to displace the two vertically upwards, which can be measured, makes sensible the difference between the two forms of balanced statical forces. In the one case the antagonizing force is suddenly withdrawn, and in the other gradually. Wherever strain exists—and it is everywhere—there force is as certainly present as when it becomes manifested in a stress relieved by motion and measurable in terms of energy.

Let us, then, give up the standard of *a priori* conceivability, in view of its many historical failures, and adopt as possible that which is provisionally ascertained. The "ego" and the "cogito"—Cartesian starting points—have proved barren and

irrelevant in philosophy. True philosophy is concerned with objectivity. The data of consciousness, mainly acquired in infancy or in the womb, are blind guides. Many an ego, whose brain was his cosmos, has run through his brief subjectivity, but the order of nature endures. The same facts are continually observed, verified, recorded, and rectified, but the observers change. Their intelligent observations add to the sum of knowledge. This is all the proof we need of objectivity, and all we will get. The insoluble difficulties of philosophy have disappeared one by one since the happy thought of eliminating them by observation entered. The immortals are those who have successfully applied this method. It is only where observation fails that insolubility lingers. Beyond the sphere of the knowable it will continue, in spite of introspection. How masterful is fact in the presence of the most intricate mental subtleties. The ball leaves the bat, in spite of the inconceivability. Galileo's plummet dropped from the moving mast strikes the deck and not the water, in spite of the inconceivability. The Earth returns in its orbit, to the second, in spite of the sun's rapid fall through space, and of the inconceivability. Two opposed horses can pull no more than one, in spite of the inconceivability. The guinea and the feather dropped in the exhausted receiver strike the plate together, in spite of the inconceivability. The isochronous pendulum swings through the widest arc in the same time as through the smallest, in spite of the inconceivability. The minute hand overtakes the hour hand, in spite of the inconceivability. The magnet draws the iron with undiminished force through all possible interpositions, in spite of the inconceivability. Could an exception be found, the perpetual-motion "crank" would work a greater inconceivability, by the instant contrivance of a power-generating machine.

We need not aspire, therefore, to remove any of the inconceivabilities of the external world. We must accept them as natural to the finite comprehension, as necessary to faculties which act by comparison, and above all as evidences of objectivity. On the other hand we should avoid that opposite error of the introspective school, of deeming that probable, or in any way connected with fact, which merely seems conceivable. I have shown that while the simplest truths have generally proved inconceivable until found out and established by genius, the greatest absurdities have had ready currency without a doubt of their conceivability. This all mythology shows. Such rubbish as "a thing cannot act where it is not," and 'a body cannot move where it is not," or "a cause cannot precede in its effect"—mere metaphysical assertions or subtleties in face of every-day fact—were stumbling blocks for ages. Such assumptions formed the basis of deduction in lieu of observation, and blocked the possibility of advance. And even yet, rigid deduction from the most hare-brained premise, if the chain of deduction is sufficiently intricate, seems to possess fascinations over a verifiable induction, with many minds.

And now, if any ask, "*cui bono*," to the scientist these philosophical inquiries and intricacies, when he has the vast field of unexplored data still before him to occupy him, I answer, the queries of philosophy are not only the main-spring and final cause of science (her first fruitful daughter and handmaid), but they conciously, or unconciously, dominate the methods and results of science herself. Each investigator, even though in the domain of the most abstract of the sciences, postulates more philosophy than he is aware of; and with so much the more danger to final accomplishment if he assumes his philosophical basis without examination. It is the errors of giant minds that are dangerous, by their ponderosity. The infallibility of the master, Aristotle, seemed to make investigation useless, until the rise of parallel giants, like Galileo, and Copernicus, stimulated a new conflict of opinion. And Descartes, though harmless from all his productions within the metaphysical domain, is dangerous by his very eminence and originality in science, which gives vogue and currency to his monumental errors. Although acquainted with the true law of motion, his scheme of matter evolved from conciousness would forbid all exhibition thereof. A grand geometer, he erected a scaffold for scaling immensity, and with unparalleled penetration perceived how a purely ideal logic, if general, would represent truth in a wholly dis-

similar realm of deduction, if equally general. Strange to say, this grand and useful discovery has become the engine, in nihilistic hands, for overthrowing all the positive knowledge we possess—the achievements of two thousand years of human effort. Not only geometry—all that has survived to us of philosophical value from the antique world—but the basis of positive dynamics, as handed down from Galileo and Newton, and Boscovich and Dalton, are apparently undermined, for all that gives them intellectual value—their certainty—unless an effort be made in the neglected field of philosophy. With strange inconsistency these advocates *par excellence* of the experiential origin of knowledge are found in the same breath promulgating as possible truth matters not only non-experiential, but not representable in ideas derived from or verifiable by experience, and avowedly originating, not from inductive generalizations—the only source of knowledge—but in purely deductive processes in the old scholastic way, from logical premises of bald assumption. In a similar way, in the hands of the Greek sophist, language, a good servant, became a vicious master, and made a chaos of all ethical achievement. A remnant of knowledge, fortunately expressed, not in verbal, but diagrammatic logic—geometry—was left, but only to fall now by the hands of similar iconoclasts, armed with more potent destructiveness, in its full flower and fruit of twenty centuries of unmolested growth.

It is time, therefore, to get back to Baconian ground, and while using for its legitimate purposes the magnificent modern machinery of analytical investigation in the field of abstract continuity—extension, motion, duration—not attempt to conjure with it as a source of objective revelation, which no mere machinery can be. A scaffold of n dimensions is as useless to the geometer as to the architect. To assume matter as continuous, simply because of the possession of a potent engine for the investigation of continuities, is to repeat the practice of certain quack specialists, who are prone to diagnose nearly every form of disease as a variety of their own peculiar speciality. And to interview the symbols of a mathematical logic for the prime definition of a fundamental objectivity, like force, is to revert to a barren source of knowledge, by an obsolete process in philosophy, and bar all progress in anything but abstract technique.

A COMPARISON OF BRITISH AND METRIC MEASURES FOR ENGINEERING PURPOSES.

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II.

Mr. W. H. Preece said he happened to be a member of the Committee of the British Association which had succeeded in establishing a system of electrical units based on the metrical system, and that system was now in use by every nation throughout the world, so that electricians in every country could speak amongst themselves in a language understood by all. There was not a laborer in any electrical engineering establishment who did not speak in accordance with a metric system which was completely understood. The advantages of the metric system were not to be met by ridicule. It would be an easy thing to turn the British system into ridicule. It might be said, for example, that Englishmen were so consistent that they measured the depth of the ocean by fathoms, and the height of mountains by feet; they measured their own height by feet, and the height of their horses by hands; they measured the surface of the land in miles of 1,760 yards, and the surface of the sea in miles of 2,025 yards; ale and beer were measured by barrels, kilderkins, hogsheads, pipes, and the like; they weighed their fuel in chaldrons, tons, or loads, according as they used coke, coal,

or wood; then there was Troy weight, avoirdupois, and apothecaries' weight, with scruples, ounces, pounds, and so on. But ridicule would not convert any one. The only valid objection raised to the decimal system was that it could not be divided by one third. For two years he had been trying to find out any circumstances that had necessitated his dividing by one-third, and the only thing that had come under his notice was the bill of his lawyer, who divided the pound by one-third and charged 6s. 8d. It had been stated that the metric system was invented by the French; but with reference to the decimal system, it was worth knowing that on the 14th of November, 1783, before the system was proposed in France, James Watt wrote a letter to Mr. Kirwan, in which he fully developed a decimal system based upon the pound.* He agreed with previous speakers that the decimal system and the metric system should be kept separate. The metric system was a system of measurements based on uniformity and simplicity, a system that would bring the whole civilized world together under one roof, and enable men to speak to each other in one language. He did not like to prophecy, but he thought the time was not far distant when the engineering profession would, like one of its branches, the electrical, seize upon the decimal and metric system as a labor-saving machine. It was a mistake to say that the metric system was not a binary system; it was essentially a binary system. Reference had been made by other speakers to the use in France of half a kilogram and half a millimeter. All men reasoned by experience. The man who had been accustomed all his life to the one-foot rule could not understand the meter, just as the man who had been used to the meter could not understand the one-foot rule. The English system had the merit of being ancient. It was established in 1266 by Henry III., who ordered that for the future 1 dwt. should be thirty-two grains of wheat, and that 20 dwt. should be 1 oz., and 12 oz. 1 lb. Edward II., in 1324, ordered that three barley-corns should be 1 inch, and 12 inches 1 foot. The British system, therefore, rested upon the scientific basis of the weight of a grain of wheat and the dimensions of a barleycorn.

Mr. Hamilton-Smythe, in reply upon the discussion, said he regretted that criticisms of the particular calculation in metric, and in decimalized British measures which he had, perhaps somewhat unadvisedly, selected, appeared to have diverted a good deal of attention from the broader question of the general comparative merits of British and metric measures for engineering purposes. By ingenious sleights of mind, when calculating British measures by vulgar fractions, and by great proficiency in mental arithmetic, some English engineers were able to evolve results in engineering calculations which excited the admiration of simpler arithmeticians. The capacity for such feats had no doubt been fostered, as was suggested by Mr. De Salis, by long practice and experience in battling with the complexities of British measures; and the difficulties which English boys had to contend with in learning British arithmetic and mensuration had, in many cases, developed mathematical capacities similar to those literary capacities which were supposed to have been developed by long and arduous studies of ancient Greek literature. Literary men, however, were able to escape from the Greek particles and irregular verbs after leaving a school or college; whereas, British engineers could never evade the wholesome discipline of those rather irksome educators, the compound rules of arithmetic and the British tables of weights and measures. It had once been urged that the introduction of machinery would destroy the individual capacity of the handicraftsman. Possibly this had been the case, yet the general result of the introduction of labor-saving machinery could hardly be alleged to have been unsatisfactory, and it might be asked whether the time now devoted to mastering the complexities of British arithmetic and mensuration might not be more usefully employed in overcoming technical difficulties, the study of which might develop equal mental capacities with more directly remunerative results. As time went on, and England became more fully equipped with engineering works, British engineers would probably find themselves more dependent on foreign work, and brought

* "The Origin and Progress of the Mechanical Inventions of James Watt," by J. P. Muirhead, vol. ii., p 179.

more into competition with foreign engineers in securing it. A considerable portion of this work would lie in countries using the metric system, so that familiarity with the system which, to quote Mr. De Salis, "appeared to commend itself to their clients," would probably be of ever-increasing importance to British engineers; while, if they were always to be burdening their memories with a double set of standard engineering dimensions and quantities, for use alternately at home and abroad, they would carry a load that would handicap the brains of most in the race for foreign work. If engineers wanted to get the full advantage out of a system of measures, they must accustom themselves to think in it. Mr. De Salis had deprecated any alteration in the size of the brick generally used in England; but bricks measuring 22 centimeters by 11 centimeters by 7.5 centimeters, would probably bond in sufficiently well with standard English brickwork. At any rate, the quality of the compound brickwork would depend, most likely, more upon other considerations than on the minute difference in size between the two kinds of bricks. He agreed with Mr. Walton Williams that a leveling-staff divided into separate meters was most inconvenient, and productive of error when read through a level at short distances; but if Mr. Williams would try the leveling-staff mentioned in the Paper, divided throughout into decimeters and centimeters only, omitting meter marks, his difficulty would disappear. Such leveling-staves had long been in common use, and he had used one in Austria for several years. The metric system afforded an ample supply of drawing scales to suit the convenience of the draftsman and the sizes of his drawing paper; but it was hardly to be expected that conveniently finite metric scale would coincide exactly with the various existing British scales, which had been adopted on account of their finite relation to the various British measures. If such had happened to be the case, one of the reasons for discarding British measures in favor of metric measures for the sake of facility for international comparison of maps and drawings would not exist. He thought no practical disadvantage would arise, as regarded the convenient size of drawings, if scales of $\frac{1}{50}$ and $\frac{1}{100}$ were used with metric measures instead of scales of $\frac{1}{48}$ and $\frac{1}{96}$ with British measures. It should be remembered that metric scales were not meant to be used to represent feet and inches, and conversely that British scales were not suited to represent meters and their decimal submultiples. As Mr. Thelwall had objected to the waste of figures involved by writing 200 millimeters and 13,000 kilograms, he might economize in them by writing the synonyms 2 decimeters and 13 tonnes. Mr. Airy seemed to have implied that the feet sometimes used in France and Norway were of the same length as the British foot; whereas he understood these feet differed about 1 per cent. and 3 per cent. respectively from the British foot, and if so would be of little value for international purposes. The adoption by Russia of the British foot was an argument, though of limited value, in favor of British measures which had been overlooked in the paper; but it was probably merely the correction of the length of a foot that had been used in Russia ever since Peter the Great carried home an English carpenters' foot-rule, and imposed it on his subjects as a standard measure. Mr. Hanson had joined Mr. Walton Williams in a defence of the $\frac{3}{16}$-inch to a foot scale; but the advantage claimed for it of suiting the existing divisions on a carpenters' rule would disappear if the carpenters' rule became metric, and the carpenter would then find that the natural scales of $\frac{1}{50}$ and $\frac{1}{100}$ would suit the metric rule quite as well. So far as he was aware, there had never been any question of the metric system affecting the British standard railway gauge, which was practically the same as the standard railway gauges of most countries using the metric system; and he thought Mr. Hanson was hardly justified in assuming that if the metric system were introduced into England, bricks would necessarily be made $\frac{1}{4}$ meter long. He regretted that Mr. Hanson had not stated what the young artisans in France happened to be measuring with a two-foot rule. If, for instance, they were measuring British-made machinery, it might be more convenient to use a two-foot rule for that purpose, on account of the various dimensions of what they were measuring having been

constructed to finite British measurements. Many of the inconveniences attributed to the metric system would be found to vanish on investigation. If computers preferred expressing metric measures in vulgar, instead of in decimal fractions, they were at liberty to do so, nor was there anything to prevent a man speaking of ¼ meter instead of 25 centimeters. It had been urged by Mr. Vernon-Harcourt that if metric measures were allowed to be used in Parliament, both witnesses and the legal profession would be much puzzled. He doubted, however, whether the average witness could be as easily puzzled with metric measures after a short experience of them, as he could always be with British measures, while the members of the Parliamentary Bar had certainly sufficient intelligence to enable them to master the metric system in addition to their briefs. He was struck with Mr. Barry's quotation from Lord Melbourne. Lord Melbourne, having attained to great eminence in his own line of life, apparently became disposed to think that changes must necessarily be for the worse. But there were many things, including the methods of measuring used in England, which had not yet been brought to annular perfection, and it appeared, from the remarks of Mr. Preece and others, there were some eminent members of the Institution of Civil Engineers who thought our time-honored British measures for engineering purposes might be improved upon with advantage. Almost every large general question resolved itself into a choice between evils, or into a balance of advantages, and the question between British and metric measures was no exception. He contended that, on the whole, the net balance of advantages was so much in favor of the metric system, that all impediments to its use in England should be removed as far as possible; and that it would be to the ultimate interest of English engineers to begin to make use of it for professional purposes. He believed continental experience, as he had pointed out in the paper, had proved this to be much more easy than many members would have been disposed to think, and the temporary translating of the comparatively few measurements which specially concerned the outside public in the plans of engineering schemes, had not been found so laborious as might have been supposed. In Ireland, land purchased for engineering works had to be computed in Irish land measures, the conversions into which were at least as troublesome as the conversion of hectares into statute acres. Sub-contracts for fencing, walling, and even some other kinds of masonry, were in Ireland set in Irish lineal perches, and various local measures had to be used there for purchasing lime, road-metaling, and some other engineering materials. Any one taking the trouble to investigate the approximate relations existing between metric and British measures, would find rules for approximate and rapid interconversion, which were at least as easily remembered and applied as the many ingenious mnemonics so freely used for abridging British fractional arithmetic and mensuration. Take for instances of the former—

10 lineal meters approximately equal to 11 lineal yards.
10 square meters approximately equal to 12 square yards.
10 cubic meters approximately equal to 13 cubic yards.

When once, however, metric measures had supplanted British standard and local measures, all such approximate and other converters would be discarded and give place to more remunerative engineering knowledge.

Sir. Frederick Bramwell, President, said he wished, before the close of the meeting, to ask the indulgence of the members for 0.0833, or it might be 0.1666 of an hour. What was the object of a system of arithmetic? He supposed that it was to be able to make calculations in the easiest manner, and to arrive at sufficiently accurate results. The question, therefore, was whether the system in use in England, or that in use on the Continent, was the more likely to satisfy these conditions. He felt inclined to say, from many years' consideration and experience, that the English system was the more likely to give this satisfaction. Of course a mere statement of opinion, if unsupported by facts, was of no value, and he therefore desired to give one or two reasons for the faith that was in him. He wished to be allowed to use the decimal system with English weights and measures when he liked, and to use vulgar

fractions when he liked. At the present time the metric system was permissive in England, so that any one could use it when he pleased; but he presumed that the object of the author was to have an Act—a compulsory Act—which should forbid the use of the present English weights and measures. What would be thought by the advocates of the metric system, if those who preferred English weights and measures were to introduce a bill for the purpose of prohibiting the metric and the decimal method. They would, no doubt, look upon such a measure as being very wrong and improper, and he must be permitted, on the other hand, to regard the introduction of a bill to compel the use of the metric system as being equally wrong and improper. His hearers must not suppose that the advocates of the metric system were not amenable to the charge of seeking to make the continued use of the existing system a crime. The bill brought in by Messrs. Ewart, Bazley, Baines, Smith and Graves on the 24th of February, 1868, contained the following penal clauses:

"10. From and after the expiration of ——— years from the passing of this Act, the Imperial and all local or customary weights and measures shall be abolished, and every person who shall sell by any denomination of weights and measures other than those of the standard metric weights and measures, or such decimal multiples or decimal parts thereof as are authorized by this Act, shall on conviction be liable to a penalty not exceeding the sum of forty shillings for every such sale.

"11. From and after the expiration of ——— years after the passing of this Act, if any person or persons shall print, or if the clerk of any market or other person shall make any return, price list, price current, or any journal or other paper containing price list or price current, in which the denomination of weights and measures quoted or referred to shall denote or imply a greater or less weight or measure than is denoted or implied by the same denomination of the metric weights and measures under and according to the provisions of this Act, such person or persons or clerk of the market shall forfeit and pay any sum not exceeding ten shillings for every copy of every such return, price list, price current, journal, or other paper which he or they shall publish."

There must indeed be an extreme superiority of one system over the other, to justify an enactment that would cause a man to be considered a breaker of the law and liable to penalty simply because he chose to make his calculations by the old method instead of by the new one. All that he asked was, that liberty should be left to people to make their own selection, and he thought if that liberty were continued it would be easy to foretell the result. The permission to use the metric system as a legal measure had existed in England for some years, and in the United States for a still greater number of years, but it had not been adopted; whereupon the advocates of the metric system, not content with leaving it to the selection of the people to use that which was most convenient, wanted to force their particular mode upon them by means of penalties. To come now to a consideration of the relative advantages and disadvantages of the two systems, What were the facts? The author had spoken of "stones," "drams," "scruples," and so on; but such measures were not used by engineers. He had ridiculed the scale of three-sixteenths of an inch, but, as had already been pointed out, that was one sixty-fourth of a foot, and he believed that his mind was as capable of grasping the idea of one thing being sixty-four times the size of another as it was of being impressed by the notion that the relative dimensions of two objects were 1 and 100. The author had brought forward a pair of calculations to illustrate the general superiority of the decimal-metric system above the existing English system; but in one sense, and in one sense only, was this calculation that had been placed before them typical of the metrical and decimal system—in the enlarged copy on the wall the decimal point appeared in the wrong place, as it always did. Some years ago he went into the workshops of the Paris and Lyons railway, where he was shown a drawing of a locomotive, with a variable blast-pipe, and he asked what was the maximum and what the minimum area. One of the engineers took a sheet of foolscap, covered it with figures from top

to bottom, and then gave him a dimension rather bigger than that of the cylinder. Sir Frederick Bramwell had a two-foot rule in his pocket, and, finding that the drawing was made on the scale of one tenth, he applied the English inch tenth, and so got out the area and translated it into French measures, which he did in one-fifth part of the time occupied by the man figuring on the paper. He did not displace the decimal point, because he had not got one. Reverting to the calculations on the wall, he wished to show how utterly misleading they were. The author had placed before the members two comparative calculations, employed to ascertain the weight in tons and decimals of tons of the water contained in a given sized vessel. In consequence of the bulk of water representing the weight in French measures he was enabled to stop in his calculation on arriving at the cubic contents, and to say, "The whole thing is done; there is the weight of the water; but if you do it in English measurements you will have all these additional figures to use before you can get the weight of the water." Assume for the moment the difference in the length of the two calculations existed, what did it prove as regarded the general question? Nothing whatever. To what did it apply? To fresh water at a particular temperature, and to nothing else. There was no other liquid on the face of the earth, from ether to mercury, for which it would be true. It was not true for salt water, nor would it even do for fresh water at a different temperature. In any other case a multiplier must be used to get the weight, which would make the metric calculation as complex as the author's English example. But who but one whose mind was warped by the metric system would have thought of turning inches into decimals of feet prior to calculation? Would not anyone else have worked the sum thus?: The annexed sum showed how any one not saturated with decimals dealing with 10 feet 6 inches by 6 feet 2 inches by 1 foot 1 inch would have treated it. There, in twenty-five figures, was the answer, as regarded the cubic contents, while the metric system, to reach the same point, had needed thirty-five figures. Mr. Percy Fowler had spoken, to his great astonishment, of the way in which workmen in Spain used

10	6	
6	2	
63	0	
1	9	
64	9	
1	1	
64	9	
5	4	9
70	1	9

the metric system, and made calculations which persons of the same class could not make in England with the ordinary English measures. He had never traveled in Spain, but he had traveled in France, Italy and Germany, and had made it his business to ascertain what the facts were with regard to the powers of the people to do anything in the way of mental arithmetic, and he said unhesitatingly that in those three countries it was the rarest possible thing to meet a person who could make a mental calculation, not because they were wanting in intelligence or ability, for they were quite equal to the English in those respects, but because they dealt with a system so cumbrous that it absolutely precluded mental calculation. Let any one go to a French railway station, and ask for three tickets from A to B, and it would generally be found that the man (or, as was commonly the case, a woman, with a man to look after her) could not tell the amount without taking a piece of chalk or a pencil and making a calculation. The clerk would have no more idea of what three times the single fare was than a child would have. Compare such a person's power of calculation with that of an English butcher's wife or daughter who was in the habit of dealing with pounds and ounces and pence. Let him test the question nearer home. Could many of those present mentally square 3.25? He believed very few. But there was no difficulty in squaring $3\frac{1}{4} = 10\frac{9}{16}$. $10\frac{9}{16}$ was a sum easily appreciated and easily expressed, while in 10.5625, the decimal equivalent of the vulgar fraction was much more cumbrous, and he ventured to think did not form any impression on the mind, except that it was a little more than one-half. Again, which of them could square 4.125? That to the majority would be almost impossible, but with $4\frac{1}{8}$ there was no difficulty.

$17\frac{1}{64}$ compared with 17.015625. It appeared to him that a system like ours, which enabled mental calculations to be made rapidly and accurately was enough for all practical purposes. It did not lie in the mouth of decimalists to insist upon absolute accuracy, because they were content with approximation, and must of necessity be so in many cases. With regard to the statement as to the meter being the ten-millionth part of a quadrant of the meridian, Mr. Hawksley had fully exposed this in his presidential address, to which he would refer the members.* The whole thing had been a failure, and Frenchmen themselves did not use the notation, nor did they use the unit of weight. It had been intended, if it were desired to express the thousandth part of a meter, it should be done by writing the word meter, and putting below it a decimal point followed by two 0's and a 1, thus $\overset{\text{Meter}}{0.001}$; but what was the fact? A millimeter was expressed by m/m.* So with the kilogram. It was intended that should be the unit, and the lesser divisions were to be indicated by a decimal point and the regular number of ciphers; but the fact was the unit was too large, and accordingly the French bought and sold by the half-kilogram, and they called it the $\frac{1}{2}$ kilo, and not the 0.500 kilo, or even 500 grams, these modes were too roundabout as compared with "half a kilogram," and humanity would say half a kilogram in spite of penalties. Centimes, again, were often replaced by sous. The "tonne," also, was not a part of the metric system, but was an invention to cure a failure in the system. People did not and would not deal with thousands of figures when they could adopt another mode of expressing the number compendiously by a single figure. As regards the facilities afforded by English weights and measures, he desired to be permitted to refer to the numberless short cuts in mental calculations given by the present system. How much they would have to forego if they were forbidden to say that plate-iron was 5 lbs. to the foot super, and cast-iron 4 inches to the pound, adding $\frac{1}{20}$ to make the necessary correction. Round-iron, by the present system could have its weight calculated in a moment, squaring the diameter in eighths and dividing by twenty-five, or multiplying by four, gave the weight in lbs. per foot at once; for example, $1\frac{3}{8}=11$, $11^2=121$, $121\times4=484$ lbs. per foot. Water had been referred to as an instance of the wonderful use of the metric system. He would take a water illustration of a simple calculation by the English system. If engineers wanted to know how much a pump would lift, what did they do? They squared the diameter and multiplied by the stroke of the piston in yards, and at once obtained the amount in lbs., as every yard upon the circular inch was a pound. Taking an 8-inch pump, the square was 64; it was making 10 yards a minute—64 gallons a minute—there they had it in a moment. Another instance, 1 inch of rain to the acre was 100 tons, or, to be more accurate, 101 tons. In the United States, where, as here, they had the option to use legally the metric system, they had not used it. Mr. Sellers, one of the best authorities, had said that the thing was not fit to be used. He would refer the Members to one who was not a bad engineering authority, Rankine. He would read to them the last verse of Rankine's song in praise of the three-foot rule:

"Here's a health to every learned man that goes by common sense;
And would not plague the workman on any vain pretence;
But as for those philanthropists who'd send us back to school,
Oh, bless their eyes, if ever they tries to put down the three-foot rule."

He would also read an extract from a speech of Mr. Beresford Hope on the same subject, made in the debate on Mr. Ewart's Bill:

"Decimalization is a process of calculation for the benefit of the calculator. Metricalization is not a process, but a system of measures, so called from its unit or base, which happens accidentally to be facilitated by the ease with which its details may be worked out through means of the decimal notation. The metrical system in itself is an abtruse and philosophic one, founded upon the fancy of some French men of science at the time of the Revolution, who adopted as the starting-point of the system the measurement of the earth's circumference, and by the way of a unit, measured the 10,000,000th part of a quadrant of a

* Minutes of Proceedings Inst. C. E., vol. xxxiii. p. 342.

meridian through Paris (about $39\frac{5}{13}$ in.) which they termed a 'meter.' No doubt those multiples and aliquot parts of the meter which form the French measures of length are adjusted to meet the decimal system, as are also the measures of area, capacity and weight, which are by a further process built upon the meter. But decimal notation is equally applicable for the man who finds that it helps his calculations whenever he has to work out his sum in our old weights and measures; for decimals are really not a system, but, as I said, a process for easily reaching a certain practical result, like logarithms or algebraical symbols. I grant all the advantages which their friends urge in behalf of decimals for the purpose of calculation; but it requires no Act of Parliament to enable those who appreciate them to make their own calculations by way of decimals. Least of all, is legislation needed for the merchant princes—the men of enormous means and gigantic transactions—whose advocate my hon. friend the Member for Liverpool (Mr. Graves) has made himself. They have but to keep a calculating clerk—an employé whose one duty is to manipulate the decimals—and they have got what they want. The sufferers will be the little people—the small buyers and sellers, the hucksters and the marketers—who will be compelled under the penalties of a compulsory Act of Parliament, to learn and to use a system which is, in its outward type, as non-natural as it is novel. I will, in order to prove my point, take the most familiar instance, and show that although a great deal has been said about the advantages of the French subdivisions, yet, after all, our subdivisions are more natural for the ordinary purposes of life. If a boy has to divide an apple, does he ever think anything about the circumference of the earth and its aliquot parts, or about the decimal system and its unrivaled facilities for calculation? No; but he takes his apple, and cuts it into two parts if he wants to halve it, and those halves into quarters if he wants to make four parts of it. In the same way, if a housewife has to cut up the loaf for her family, she divides it into two, into four, into eight, or into sixteen parts, and the sixteen people share their bread naturally. Supposing the loaf to weigh originally a pound, each of these sixteen divisions comes out an ounce. Such is the *rationale* of our system of measuring—binary system so-called—founded on continual halving, and proved, by the common sense of mankind, before the great era of enlightenment inaugurated in 1789, to be the most convenient and natural one. But I may be told—Halve away, but then express your halvings in decimals. This is very easy for the merchant prince to do when he is totting up his large transactions in 'centals,' or for the Chancellor of the Exchequer when dealing with a nation's finances; but how will it suit the little transactions of daily life? I come back to my loaf. How are ordinary people to represent halves and quarters by decimal points? The symbol of a half is the figure 'five,' with a dot to its left hand; the symbol of half that quantity, that is of a quarter, is the sum twenty-five, also with a dot to its left hand. Arithmeticians understand how this can come about, and the symbols have grown natural in their eyes; but in what—even the most infinitesimal—degree do they tell their own story to the unlearned? What palpable relations towards each other can be disentangled out of these most frequently recurring symbols? What is there in the nature of things to show that the dotted five means a half, and the dotted twenty-five a half of that half, and a quarter of the 'one,' with no dot on either side, which stands for unity? Decimal notation is then, after all, as I have been arguing, a process, and not a system. It is a process good for the schools, and good for the bustling counting-house and the large sum, but the poor man would be completely thrown out if he had to employ—under penal legislation, too—decimal points for the purpose of measuring his little purchases by halves and quarters. With permissive means, such as now exist, the system will come in where it is wanted, and be kept out where it is not wanted; but under a compulsory enactment it will intrude itself everywhere, and show itself in its real colors as nothing less than a public nuisance. But the more we examine the bill of the hon. Member of Dumfries, the more inapplicable do its provisions seem for the purposes of practical life. I have touched upon the principle of the metric system, let me now call the attention of

the House to the language in which (after the French model) it is proposed to clothe that system. The new unit of weight is to be the 'gram' or 'gramme,' which is attained by providing a square vessel, whose capacity is the cube of the hundredth part of a meter (centimeter, to wit), and then weighing the amount of water which it will hold at a certain temperature. One-tenth part of this gram is to be a de*ci*gram, and ten times a gram is to be a de*ka*gram, for the reformers decreed that aliquot parts were to be named after the Latin, and multiples after the Greek numerals. How in the name of common sense can we make poor people understand that because there are the letters 'ci' in the one word it means the tenth of a gram, and that because there are the letters 'ka' in the other it means 10 grams, or 100 decigrams? My hon. friends the Member for Dumfries and the Member for Liverpool come to this House representing great commercial transactions; but I stand up for the poor man. Only imagine an honest housewife going into a shop and asking for a decigram of pepper, and a dekagram of tea; imagine, too, the milkmaid selling her fluid by the liter. The Member for Liverpool is a kind-hearted man; is he then prepared, with all the stringent force of a penal statute, to enact that, when one of his youthful constituents may desire to effect a commercial transaction in a manufacture for which one portion of that great borough is famous, he should be bound to go to the shop and tender his 'dime' for three decigrams of Everton toffee? Fancy the farmer who has been accustomed ever since he entered on his farm to cultivate the 'ten' or 'twelve-acre field,' having to consult the steward about liming the seventeen *are* field, or be a criminal and a contemner of the laws of his country. Fancy the bumpkin who was prepared to boast that he was within a decimeter of catching the fox as he crept through a gap about a dekameter from the white gate. If the theorists and the men of wealth—men of brains, it may be, but as certainly men of self-assurance—have worked out this system for themselves, there are poor men, who form the majority of mankind, for whom it will never answer, and there are men of brains at least equal who are decidedly opposed to its adoption. Is it not possible that our present system is not only quite as convenient and useful as the metric system, but a little more philosophical also? Why should a standard founded on the quadrant of the earth's circumference passing through the meridian of Paris be a better one than ours? No doubt it looks very solemn, from the grand nomenclature with which it is propped, but all those odd names for the French weights and measures were adopted at the first heat of the great Revolution, when the pedantic aping after ancient Greek and Latin terms led to their being applied to everything novel and French—from the scanty proportions of a lady's dress to the most intricate principles of jurisprudence and moral philosophy. Moreover, they have taken root in nations whose vernacular languages are themselves derived from the old classical tongues. May it not, I repeat, be just possib e that our unit is as good as that of the French, even upon the most abstract grounds."*

Mr. Beresford Hope, having spoken in that common-sense way, had quoted a letter from Sir John Herschel, which was very well worth reading, but he would not detain the members with it. He would only refer to one other matter, and that was the coinage. It had been said, you may decimalize the pound without difficulty. This was one part of the case which had not been considered. Once let the pound be decimalized, and there would be an end of the guinea. He did not know whether there were any physicians or barristers present, but if there were he would remind them that by the new system they would lose 0.047619 of their incomes—in other words $\frac{1}{21}$. When the pound was decimalized no one would be prepared to pay the next division, which would be $\frac{1}{10}$ instead of $\frac{1}{20}$. In conclusion, he would tell them of a ready way which he always used for mental calculation when turning millimeters into inches. Mr. Rapier had said, multiply by 4, but this needed a correction, which was, deduct $\frac{1}{64}$ of the product; this would give an answer as near the truth as the 3 .. 3$\frac{3}{8}$ commonly taken; for example, 16 millimeters $\times 4 = 64 - \frac{1}{64} = 63 = 6.3$ inches.

* Hansard's Parliamentary Debates, vol. cxcii., May 13th, 1868.

CORRESPONDENCE.

Mr. A. Barclay remarked that there was a convenient mode of graduating and figuring a rule which was applicable to any arbitrary unit and subdivision. In the ordinary fitters' and joiners' English two-foot rule, folded in the middle on a compass-joint, the inches, figured on each face from left to right, 1 to 24, were usually divided into $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, and $\frac{1}{16}$. If any work had to be centered, the unit end of the rule was made to correspond with one limit, and the reading was taken at the other limit (say 21$\frac{3}{4}$ $\frac{1}{16}$ inches bare), it was then necessary, mentally, to halve this quantity, represented by two numerals, two fractions, and the difference to the nearest division, and having by this process obtained the new half quantity (10$\frac{7}{8}$ $\frac{1}{32}$ inches, scant, which had also two numerals, two fractions and a difference), to note the corresponding reading on the rule, and against it make a trial center mark, which was very likely to require adjustment. Next, the accuracy of its position must be checked by an equal half measurement to the other limit. The rule which he preferred had the usual divisions and figures, and also on one face next the outer edge were two scales of half inches, figured 1 to 24, from the joint to each end, the half inches having the same number of subdivisions as the scale of inches. With this rule, having ascertained the cross measure (say, as before, 21$\frac{3}{4}$ $\frac{1}{16}$ inches bare), and found the corresponding figures and divisions on the two half-inch scales, they would, when laid on the work, exactly coincide with the two opposite limits, and the center would be marked off with confidence at the joint zero. It was only necessary to remember the first measurement as a comparative length, calculation was unnecessary, and halving a measurement by this method of substituting the appropriate scales was accomplished with accuracy and dispatch. The Japanese artizans, now at work at the Exhibition at South Kensington, used a bamboo rule of native make, like a drawing scale, the unit being the English foot divided into 10, 100, $\frac{1}{80}$.

Mr. H. Bauerman, as the result of long experience in the use of various metrological systems in different countries, was unable to agree with the author's conclusions as to the desirability of internationalizing the metrical system to the exclusion of all others. He thought that the weights and measures used at any particular time by any people fairly corresponded to their local and temporary necessities, and as new necessities arose, they would be met by the adoption of new, or by the modification of old, units. Take, for instance, the progress in the use of the ton as a business unit. For many centuries commercial requirements were satisfied by the hundredweight or quintal, and indeed the latter, until lately, if not now, did survive in the Newfoundland and Labrador cod fisheries. Increase in the scale of operations, however, led to the adoption of the twenty-fold larger unit, first in England, and at longer intervals in foreign countries. Strangely enough in Germany, where the cwt. unit lingered longest, the ton had been supplemented by a ten-fold larger unit, the wagon, corresponding to the contents of a ten-ton railway wagon, which was now commonly used in the coal and iron districts of the Rhineland. These were examples of change of use due to commercial necessities, and which had been effected without inconvenience; but it would be very different to attempt to impose an entirely new metrological system upon a people without regard to local usages. Those who advocated the universality of the metric system seemed to consider arithmetical convenience as synonymous with convenience of every kind, which was not necessarily true. For instance, when a large number of similar articles had to be divided into small packages, there was a distinct saving of packing materials by making them up in twelves rather than in tens. Here the convenience of the packer was in sharp contrast to that of the computer, so that while the latter might prefer to consider twelve as 1.2 decade, the former would regard ten as five-sixths of a dozen, and it would only depend upon the relative strength of the two parties which view should prevail, if one were to be used to the exclusion of the other. The author's arithmetical illustration did not appear to have much bearing upon the question, for although he had used a large number of figures to arrive at an incorrect result in determining the contents of the English tank, as contrasted with a smaller

number required for the determination of the equivalent metrical volume, the former was due to the use of incorrect factors, and an inappropriate arithmetical method, which, again, was conditioned by the assumption that civilized arithmetic must necessarily be decimal. In other words, he preferred multiplying by 0.167, which was inexact, to dividing by 6, which was right. The author's suggestion that English engineers would be precluded from using foreign technical literature, because, in the latter, dimensions were, as a rule, expressed in millimeters, did not seem to accord with the experience of the Institution, which for some years past had supplied abstracts of the more important papers in foreign journals, in English, but in many cases with the original dimensions. These had been so far appreciated as to be largely reproduced in English and American journals when of special interest, which would scarcely have been done had the editors considered them as a matter unintelligible to their subscribers; and, in the case of one contributor, at any rate, such abstracts had led to correspondence sometimes of an inquiring and sometimes of a mildly controversial character, but in neither case had there been any misapprehension as to dimensions. The author's views as to decimalizing English money were also contrary to experience. The physical decimalization of the sovereign had been attempted in England by the issue of double shillings, which were intended to supersede half-crowns, and the coinage of the latter was discontinued for many years; but when they became scarce the public asked for a further supply, and got them, whereby the decimal system, pure and simple, was tacitly abandoned, but at the same time public convenience was enhanced by having the two kinds of coin instead of only one. The Latin Monetary convention, in its present form, was rather intended to reduce the circulation of a token coin, the franc of $\frac{835}{1000}$ fine, than to facilitate its international currency.

Mr. F. Briffault pointed out two instances that had come under his notice in connection with two foreign waterworks where the decimal and metric systems alone were used. The Brazilian Government had been supplied with 95,000 tons of pipes and connections from this country for the waterworks of Rio de Janeiro, this quantity being divided between four manufactories. The castings were all weighed in kilograms, the weighing machines having been made expressly for the purpose. At first the men did not take kindly to the innovation, but at the end of a fortnight, after finding out the great saving of trouble thereby effected, they much preferred this system to the British. Having only to deal with one unit of weight, the kilogram, the numbers were at once read off the weighing machine, and at the conclusion of the day's work the columns were added up, and three figures pointed off to the left gave, of course, the total in metric tons. Only simple addition being necessary, the class of men entrusted with these operations could do their work without error, whereas, had the weight been of of several denominations, a hopeless muddle would have arisen. At the termination of the contract there was only a discrepancy in weight between the Government authorities and the contractors of under three tons. Considering the magnitude of the undertaking, such a result would scarcely have been attained had British measures been used. There had also been a great saving of time and money in clerks' salaries by the adoption of this system, in the two and a-half years employed on the work. In the case of the Constantinople waterworks, similar satisfactory results had been obtained by the use of the decimal system; but in Turkey the advantage of it over the British system was even more marked. The number of pipes arriving damaged and cracked in transport up country was very great; they had, consequently, to be cut on the spot in order to be utilized, and there was thus a great variety of of lengths to deal with. To weigh the pipes after cutting was impossible, for many reasons, so the reduced lengths were measured in meters and centimeters, and the maximum weight allowed per lineal meter being fixed, the two had but to be multiplied together, and the result was metric tons and decimal parts of the same. With feet and inches, and tons, cwts., qrs., and lbs., a vast amount of tedious work would have been necessary, with a doubtful result as to its correctness.

Mr. W. D. Chapman remarked that in

the *Standard* of the 16th of December, 1884, there appeared a leading article to the effect that a Dutch company could sell Dutch milk in London "at the price of 2¾d. per liter—or, in other words, at a little over 2d. per quart." On proceeding to check this calculation, he found that 2¾d. per liter was 3⅛d. per quart—a difference which completely disposed of the arguments in the *Standard*. He thought that many practical English farmers, interested, as he was, in English dairy farming, had been misled by having to assume the accuracy of the erroneous conversion of measures, and had needlessly admitted the hopelessness of competing with the Dutch company, simply because the want of an international language of measurement deceived them as to the facts of an enterprise affecting their business. This was merely one recent example of many cases where English business had been fettered and English business men deceived, in questions of international competition where measures were concerned.

Mr. John Craig observed that most of the advantages and disadvantages of substituting the metric system of weights and measures for the British system had, there was no doubt, been fully entered upon and discussed. He wished only briefly to add his experience of the working of both the above systems, and also of other systems in various countries. Great weight must be attached to the remark in the paper as to the position held by England and the United States of America in the manufacture of engineering machinery. It was generally admitted by engineers that any violent change of the measures now in use would be practically impossible, as far as this branch of mechanical engineering was concerned; besides, a strong feeling existed among many mechanical engineers against any change; they even asserted that British measures were, for their purposes, better than any others in use. No such feeling existed amongst civil engineers; nor would interests be affected in anything like the same degree if the metric system were at once generally adopted in connection with all field and office work undertaken by them, for which the consent of Parliament had to be obtained. I have never met a civil engineer who had used the metric system in field and office, on railway or other public work, who did not admit its great superiority to the British. If his experience agreed with that of the majority of the civil branch of the profession, he submitted that steps should be taken to have the metric system allowed, or enforced, on all public works; its use would thus become known, and gradually extend, and the way prepared for the time when the metric system, or other system based on somewhat similar principles, should be adopted by all English-speaking peoples. Literature, in connection with all branches of the profession, would soon adapt itself to a new order of things. It had been said, "To acquire a new language is to enter upon a new world." The advantages a British engineer would gain by a literature written in terms of an almost universal measure with which he was familiar, would be great, indeed. The advantages derived from the nomenclature of the metric system, although great were, he thought, of secondary importance; that was, if the nomenclature should in any way prove a bar to the system coming into general use. He would prove a benefactor to his country who would suggest some good plan whereby he could retain many familiar names for certain of the metric dimensions. The adoption of the metric system in India, on the Government and public works, so far as might be required for engineering purposes, would not cause much inconvenience, especially if time were given. In most, if not in all our colonies, the change would be hailed with joy, and would form a not unimportant link in the chain which was gradually being forged, to bind them in one mighty federation with the mother country. In some of our colonies its adoption would do away with the difficulty of introducing British measures amongst peoples under our rule, whose system of weights and measures might be as good as ours, and whose likes and dislikes were quite as strong. At the Cape, for example, all railways and public works were carried out by the use of British measures. Land was, however, surveyed, bought and sold by Dutch measurement. The colonial ton, of 2,000 lbs., and cwt., 100 lbs., could not, he thought, with advantage be replaced by the British ton and cwt. In

the markets of the country the measures were most confusing, and there was certainly great need for reform. To have a decimal system of coinage would no doubt facilitate the use and introduction generally of the metric system. The great convenience of a decimal system of coinage could only be fully realized by those of our countrymen who had resided for a time in a country where, such a system was in use. He feared, however, the opposition to change would be very great. Time could not not be given for it to come gradually into use, and a change, when made, must be compulsory on all. He did not think our accounts would ever be kept in pounds sterling and mils. The pound sterling must always remain, but florins and cents could easily be substituted for shillings and pence, and the British penny might still remain a force in the land, in name and appearance. Accounts kept in the above way would effect a saving of 20 per cent. in the number of figures required on a page of a written-up ledger, to say nothing of the advantages in other ways.

Professor J. D. Everett agreed with the author in thinking that the adoption of the metric system of measures and weights would be a great benefit, by effecting a saving of a vast amount of useless labor. The case had, he thought, been fairly and clearly stated in the paper. He was glad to find one point insisted on which had often been overlooked, namely the facility which a decimal system afforded for logarithmic calculation. The remark must be extended not only to the use of tables of logarithms, but to the use of logarithmic scales, such as the slide rule in its ordinary form, or in the circular form, or in the parallel-column form employed in his "Proportion Table," or in the helical form which, in Professor Fuller's "Spiral Slide Rule," admitted of working to five places of figures. He concurred in the importance of speedily decimalizing the British money reckoning and coinage. Any partial decimalization of the coinage, not carried far enough to admit of keeping accounts decimally, was of no advantage. In his opinion the arrangement that would least disturb existing prices would be to decimalize upwards from the farthing, because every sum of money, according to present reckoning, would have an exact equivalent in the new reckoning, whereas the present penny and halfpenny would have no exact equivalents in a system decimalized downwards from the pound sterling. But a minimum of disturbance was not the only consideration.

He was in favor of neither of the systems above compared, but had for many years held the opinion that the best system to adopt would be the American system of dollars and cents. When traveling in America many years ago he was greatly struck, not only with the facility afforded for calculation, but with the extreme convenience of the coinage. An abundance of change for all sorts of purchases could be carried without burdening the pocket, and though a stranger, he could count his change with as much ease as in his own country, and with much more ease than in France, notwithstanding that the coinage there was likewise decimal. He claimed that the American system was more economical of space in account books than either the French system or a decimalized system founded on the pound sterling. It started from the cent (about a halfpenny) as the unit in the right-hand column. A decimalized system, founded on the pound must start from the mil as the unit in the right-hand column; for the hundredth of a pound (2½d., nearly) was too large for less sums to be ignored. The pound would therefore be the unit in the fourth column from the right hand, and space must be left for three columns to the right of it. In the American system only two columns were wanted to the right of the dollar column, so that the unit in the fourth column from the right hand was ten dollars. This was rather more than two pounds sterling, and thus with a given number of columns twice as large a sum could be expressed in the American system as in the pound and mil system. The disturbance of prices would be much less than in the pound and mil system, for 100 cents were 4s. 1½d., and 100 halfpence were 4s. 2d. The difference between a cent and a halfpenny was, therefore 1 part in 100, whereas the difference between a mil and a farthing was 4 parts in 100. He thought the importance of adhering to the pound sterling

in its exactitude had been greatly exaggerated, and he believed that no confusion would be introduced in foreign relations by making payments in five-dollar gold pieces, each worth £1 0s. 7½d., instead of in sovereigns, as hitherto.

A SUGGESTION IN SEWAGE PURIFICATION.

From "The Builder."

An experiment has recently been made by a scientific man at Buxton, which may possibly exert an important influence with regard to the disposal of sewage. Nor is it a theoretic discovery alone. Works have been erected, at the cost of £4,000, for the treatment of the sewage of Buxton (which varies in quantity from 200,000 to 1,000,000 gallons per diem), by the process to be described; and on the 9th of April the Buxton Sewage Works at Ashwood Dale were formally opened, and the occasion was duly celebrated by a public dinner.

The Rivers Pollution Commissioners have for some time past insisted that measures should be taken by the Local Board of Buxton to rid the Wye of the poisonous contents of the drains. In the case of a town chiefly known as a health resort, the subject assumed even more than usual importance, and a deputation of the Local Board took the wise step of visiting various sewage works, of which the principles had been recommended for their adoption. They went to Birmingham, where they found a sewage farm, and also a long series of tanks, and pronounced the system a failure. No fish would live in the water. They went to Bilston where the filtration system has been adopted. It was not, however, satisfactory. They also visited Coventry, where they saw the operation of the grinding machines on what is called the black-ash system, in which sulphuric acid is used. That they considered the best system they had seen. At Leamington they saw an immense sewage farm, for the effluent of which, produced at a cost of £1,100, £400 was received. This they regarded as a heavy loss, and a plan that would not answer at all. They visited Hertford, where phosphate of alumina is produced by the Phosphate Manure Company. That plan, they found, would cost 8d. or 9d. in the pound on the rates, if applied at Buxton. It does not appear from the report under what conditions Dr. Thresh was called in to advise; but this gentleman, in the course of experiments made from time to time as to the defecation of sewage, was led to examine the water flowing from an old coal pit, which ran to waste in the Wye, disfiguring the public gardens of Buxton by its course through them. This water is, in fact, a strong chalybeate spring. It contains salts of iron, aluminium, sodium, calcium, and magnesium, chiefly as sulphates, but a considerable portion of the iron is in the form of a carbonate held in solution by carbonic acid. From 1.2 to 2.4 grains of metallic iron occur in the gallon of this water.

The effect of this iron water on the sewage is remarkably prompt. The method adopted has been to mix with this natural water a certain proportion of milk of lime, and then to allow it to mix with two or three times its volume of sewage. By agitating gently, a flocculent precipitate forms and rapidly settles, leaving the supernatant fluid beautifully clear. Analyses made by Sir Henry Roscoe at Owens College are appended to the report of Dr. Thresh, from which we abstract these particulars.

Additional interest attaches to this method, from the fact that its principle is much the same as that first applied at Antwerp by Professor Bischof to the purification of the waters of the River Nethe by passing them through a mixture of spongy iron and gravel. The effect of iron in the destruction of organic matter suspended in water has thus not only been previously known, but the plan has been acted on, on a considerable scale, at the Antwerp Waterworks. In 1878 Mr. Bischof, as appears from the proceedings of the Royal Society, advocated the use of finely-divided or spongy

iron as a medium for the filtration of water. It was demonstrated, according to Dr. Frankland, that filtration through spongy iron destroyed much of the organic impurity, removed color, precipitated finely-suspended solid matter, and, above all, destroyed the germs of putrefaction, and probably those of all kinds of epidemic disease. In 1879 a filtering apparatus was erected at Waelhem for the filtration of the water of the Nethe. A cast-iron tank, 18 ft. 6 in. square and 11 ft. deep, was coated at the bottom with cement concrete, covered with bricks on edge. On the bricks was laid a mixture of three parts of gravel with one part of spongy iron, 3 ft. thick, which was covered with 18 in. of fine sand from the Meuse. A second filter of a similar kind was placed at a lower level, so as to receive the water that had passed through the first. The results were so satisfactory that large works were undertaken, a description of which will be found in Vol. 72 of the Proceedings of the Institution of Civil Engineers. After eighteen months' experience it was stated at the Conference on Water Supply, held at the International Health Exhibition of 1884, by Mr. Anderson, M. Inst. C. E., that, as far as the purification of the water went, Professor Bischof's process left little to be desired, but that the working of the system had been costly.

The increasing demand for water rendering extension of these works necessary, Mr. Anderson, M. Inst. C. E., whose duty it became to advise the directors of the waterworks, made an experiment, suggested by Sir F. Abel, on the principle of passing iron through the water, instead of passing water over the spongy iron. Mr. Anderson constructed a revolving cylinder, 4 ft. 6 in. in diameter, and 5 ft. 6 in. long, which was furnished with inlet and outlet pipes, and also contained shelves or ledges for scooping up the iron used, raising it to the top of the cylinder by the rotary motion, and thus letting it fall through the water. Running water through this cylinder at 12 gallons per minute, which gives a contact of about 45 minutes, Mr. Anderson found the water to be very heavily charged with iron. At a flow of 30 gallons per minute, 1.20 grains of iron were dissolved per gallon, which was twelve times as much as the experience at Antwerp had shown to be necessary. At 60 gallons per minute 0.9 grain per gallon was dissolved. The result of the trial proving thus successful, the revolver was sent to Antwerp, fitted with large pipes, which sent 166 gallons per minute through it, and has been at work there ever since.

Thus the history of the application of iron to the purifiation of water comprises a number of independent experiments and discoveries, made by different men. More than twenty-five years ago Dr. Medlock and Mr. Quick, C. E., made a number of experiments on the purification of Thames water by metallic iron. The water of the river at Battersea was left in contact with iron wire and plates in a large tank, for twenty-four hours, and the improvement in quality was very marked. It is well known to naval officers (and has been mentioned in the columns of the *Builder*) that water stored in iron tanks that have been whitewashed inside becomes remarkably pure and sparkling, and that the vapidity of distilled water is removed by such storage. The Antwerp filters represent a further step in the same direction, although the propriety of the mixture of gravel with spongy iron has been called in question. Sir F. Abel's suggestion is marked by extreme elegance; as the weak point of all filters, that of becoming choked by their own action, is avoided by the very ingenious reversal of the usual method of producing contact with the metal. A no less original step has been taken by Dr. Thresh, and the review of the advance made in twenty-five years leads to the conclusion that much yet may be done towards the perfecting of the use of iron as a purifier of water.

Dr. Frankland, an unquestionable authority on the point, states that bacteria, which are indestructible by an atmosphere of pure oxygen, of carbonic acid, of nitrogen, of sulphurous acid and of cyanogen, are killed by a short contact with iron. As all the known forms of bacteria are affected in the same way, it is thus probable that all forms of bacterial life will be thus destroyed, and iron is the only known substance which produces this effect. Thus far, therefore, the progress of the application of iron may be taken as highly promising.

Several questions, however, remain for

solution. Mr. Anderson, Dr. Frankland, and other authorities, describe the action of iron as rapidly destructive of organic as well as of organized matter. In the case of .the water of the Nethe, which is very impure, a contact of nine minutes is enough to dissolve 0.9 grain of metallic iron per gallon, and a contact of three minutes and a-half, which presumably will not dissolve much more than 0.3 grain per gallon, is found to be more than adequate to effect purification from organic suspended matter. Indeed, the waste of iron during thirty-three days is stated at 0.176 grains per gallon of water run through the cylinder. Dr. Thresh, however, speaks not so much of destruction as of precipitation, and even says that 100 parts of dry residue from the Buxton tanks contains fifty per cent. of organic matter. The difference is cardinal. But the Burbage chalybeate water contains, together with from 1.2 to 2.4 grains of iron per gallon, fifty grains of mixed crystalline sulphates. It is thus evident that when to this heavy proportion of mineral matter is added the milk of lime thrown in to hasten precipitation, the total amount of sludge formed must be far in excess of the inorganic elements of the sewage. It is in the mass of sludge that has in some way to be got rid of that the essential weakness of all precipitation processes lie. In the present case it is intended simply to cart away the sludge in a moist state, and put it on land belonging to the Board. This mode of disposal, as Dr. Thresh justly remarks, cannot go on for ever, and sale of the sludge for manure is evidently looked forward to as a resource. Indeed, the presence of so large a portion of organic matter in the dry residue is evidently regarded as increasing the stimulating value of the manure. Now, it is on the rock, or rather the quicksand, of the profitable disposal of the *residua* of city life that most of the schemes for sewage disposal have hitherto foundered. The true chemical value of the contents of sewage is so low as hardly ever, if at all, to be worth the cost of extraction. And the other materials, put in as precipitants, or added to "fortify" the manure, can usually be applied much better in their natural state to agriculture than as constituents of the heavy and unmanageable sludge. As far, then, as precipitation of organic matter takes the place of destruction, and as the addition of lime to the heavy chemical charge of the chalybeate water is required the results which may be expected from the Buxton process by no means come up to what we think may yet be secured from the brilliant discovery of Dr. Thresh.

The works recently opened have been constructed by Mr. Joseph Hague, A. M. Inst. C. E., the Town Surveyor of Buxton. The chalybeate water is conveyed by gravitation through earthenware tubes, with joints of jute, spun yarn, and cement, from a disused colliery at the foot of the Axe Edge Hills for a distance of over two miles. It then enters a tank at the rear of the liming rooms adjoining the works, which are situated between the river Wye and the Midland Railway, in Ashwood Dale. A series of flushing chambers, supplied with penstocks, is introduced at suitable places along the route, with a view to supplying the carts for street watering.

The liming and mixing rooms are erected over the River Wye, on a semicircular stone arch, the liming-room floor being on a level with the adjoining highway, and connected with a siding on the Midland Railway by a tramway. One of Messrs. Bowes, Scott & Read's liming machines supplies a cistern of 800 gallons capacity, which is provided with an agitating apparatus. The machinery is driven by an overshot waterwheel, 16 ft. in diameter and 3 ft. wide, driven by water derived from the River Wye.

Outside the liming and machinery rooms are duplicate brick tanks, into which the main outfall sewer discharges. The tanks are furnished with wrought-iron screening-wagons, for the purpose of abstracting the solid and floating matter, which is estimated at 75 per cent. of the whole sediment. After passing through the screening wagons, the sewage runs through a brick conduit into a circular water chamber, furnished with horizontal paddles, where the iron, lime and sewage, are thoroughly mixed; thence the mixtures flows to the settling-tanks. the series of which is 266 ft. long by 73 ft. wide, built of brick in cement, with concrete bottoms. The bottom of each tank is an inclined plane, 3 ft. 6 in. lower at the entrance than at the exit

end, an arrangement that has been found fully adequate to retain the deposited sludge. After passing through the tanks the effluent water finally escapes over a weir, and so into the river. It is stated that the cost of the erection and maintenance of the works will be covered by a rate of 1¾d. in the pound.

The interest locally taken in this undertaking is very great, and the good example set by the Local Board of Buxton in visiting the sites of the various works suggested to them for imitation cannot be too well known. The plan may prove, however, to have much more than a local interest. Chalybeate water is of rare occurrence, and it is impossible that the exact conditions utilized by the skill of Dr. Thresh and Mr. Hague may be unique. But the attention that these works will cause to be given to the use of iron as a purifier has a wider scope.

THE SECOND LAW OF THERMODYNAMICS.

A VICE-PRESIDENTIAL ADDRESS BEFORE THE SECTION OF MECHANICAL SCIENCE AND ENGINEERING OF THE A. A. A. S., ANN ARBOR MEETING.

By J. BURKITT WEBB, Professor at Stevens Institute, Hoboken, N. J.

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THE second law of thermodynamics will be the subject of this address, and its object will be to make clear what that law is, and to show that Rankine's statement of it is the only correct one, and that it is entirely comprehensible and easily demonstrable. After calling your attention to some statements as to this law, and as to Rankine's way of putting it, no apology will be necessary for the choice of a subject, which ought already to have been fully and satisfactorily settled.

Professor Tait, quoting from Clerk-Maxwell, says: ". . . but when we come to Rankine's 'Second Law of Thermodynamics' we find that . . . its actual meaning is inscrutable.

"'The Second Law of Thermodynamics.—If the total actual heat of a homogeneous and uniformly hot substance be conceived to be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal.'

"We find it difficult, even in 1878, to attach any distinct meaning to the total actual heat of a body, and still more to conceive this heat divided into equal parts, and to study the action of each of these parts; but as if our powers of deglutition were not yet sufficiently strained, Rankine follows this up with another statement of the same law, in which we have to assert our intuitive belief that—

"'If the absolute temperature of any uniformly hot substance be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal.'

"The student who thinks that he can form any idea of the meaning of this sentence is quite capable of explaining, on thermodynamic principles, what Mr. Tennyson says of the great Duke—

'Whose eighty winters freeze with one rebuke
All great self-seekers trampling on the right.' " *

With all respect for the eminent physicist who gave this challenge, I would rather be responsible for these statements of the second law than for that with reference to his now celebrated "demon." "He will thus without expenditure of work raise the temperature of B and lower that of A, in contradiction to the second law of thermodynamics."†

Clausius gives as the "Second Fundamental Principle of the Mechanical Theory of Heat"—"Heat cannot of itself flow from a colder to a warmer body," and then applies it in thermodynamic investigations, and essentially

* "Miscellaneous Scientific Papers: with Memoir of Rankine," by P. G. Tait, M. A. London, 1881. Page 30 of Memoir.

† "Theory of Heat:" by J. Clerk-Maxwell, M. A. London, 1877. Page 328. (A and B stand for two portions of a uniformly hot and dense gas separated from each other by a thin wall.)

this statement, perhaps because of its inviting nature, is given in some of its many forms by numerous authors as the second law of thermodynamics; now it may be a law in the general theory of heat but not in thermodynamics, which treats of the relations between heat and mechanical energy.* This is a very different statement from Rankine's; it is simple and plausible—deceptively so—and amounts to a statement that the universe is running down, which is by no means axiomatic or easily demonstrable.

The formula for the maximum efficiency of a heat-engine is sometimes given as the second law of thermodynamics; but this is a consequence only of that law and of the form in which any continuously acting heat-engine must necessarily be built. Rankine calls this the "law of the efficiency of elementary thermodynamic engines," and the manner in which it results from his second law is simple and evident; it is, however, more frequently made to depend upon Clausius's fundamental principle, or its equivalent, by an argument based upon the fact that a perfect heat-engine is reversible, and a significant remark of Professor H. T. Eddy's is to be noted here. ". . . The necessity for the existence of the second law is to afford a basis for Carnot's principle, and any truth which affords such a basis is called the second law." † This is anything but a dignified rôle for a law of nature, which should never hold a position except in its own right, and the formulae of thermodynamics should not be made to depend upon truths chosen in this way.

A discussion of the various forms of the argument referred to would occupy too much space to form a part of this address; it may, however, be made the subject of a future paper. It is, however, to be remarked that the efficiency formula can be proved equally well on the supposition that heat will not flow of itself from a hot body to a cold one, or even that it will not flow in either direction,—we have only to reverse the pair of connected engines in the one case, or to run them in either direction in the other, to show the efficiency formula to be as correct as the hypothesis. It follows, therefore, that the truth of this formula is reflected in no degree upon the hypothesis of Clausius, and, further, this second fundamental principle, while it may be true, has nothing to do with the second law of thermodynamics, as given by Rankine, except in such way as all natural phenomena may be connected. We may add also that the necessity for this form of argument has greatly diminished, perhaps disappeared, since Carnot's time, the discovery of the nature of heat having furnished us with the means of deducing the second law directly from a consideration of its action.

The preference, too, should always be given to the proof which is direct, and which exhibits the mechanism of nature —which follows her straight path and reveals her steps. The *reductio ad absurdum* and other indirect arguments should only be used tentatively until the direct chain of evidence may be traced, and no scientific mind should be satisfied with a demonstration which would compel belief without removing apparent objections.

There is, however, much evidence, besides that furnished by Professor Tait, that Rankine is not understood; his statements are either copied verbatim, without explanation, or modified in a way to make this evident. But Rankine is not so much to blame; statements on well understood subjects are often in themselves quite unintelligible, and in new fields, words and phrases are not always at hand to completely express new ideas. Language is meant to excite and direct the imagination, and rarely does a form of words, when first heard, convey a definite and correct meaning unless the imagination meets it with pictures of all possible ways in which it may apply. Sometimes even it is necessary to understand the subject before statements in regard to it can be appreciated, and I confess that until I had worked up this subject myself Rankine's statements were troublesome; now, however, they seem so reasonable that I shall endeavor to lead you to the same opinion and to convince you that of the three statements to which reference has been made, only Rankine's is indeed the second law of thermodynamics.

Let us inquire first what we should

* See Rankine's "Steam Engine." Pages 223 and 299.

† "Thermodynamics," by H. T. Eddy, C. E., Ph. D. New York, 1879. Page 18.

expect for the second law after the first law has stated that heat and work are mutually convertible and convertible in a fixed ratio. It is manifestly appropriate that the second law state the agency by which such a conversion may be effected, and also the rate at which it will proceed.

To my classes in this subject I have for two or three years past given the following statement of the second law:—

A quantity of heat, W′, *may be employed as an instrument for the conversion of another quantity of heat,* W, *into work, or for the conversion of a quantity of work,* A, *into heat, and the converted quantity will be proportional to the convertor quantity for a given change of volume or of entropy.*

To realize the meaning of this statement, let us imagine the simplest physical air-engine;—we need no fly-wheel, valves, etc., but simply a vertical cylinder of infinite height and unit section with non-conducting walls; the cylinder bottom must be permeable to heat and the non-conducting piston must be loaded with a pressure varying so as to be always but a differential less than the gaseous pressure beneath it. If we fix a finite limit to the total amount of heat which the engine shall be capable of transforming into work, then the height of our cylinder need not be infinite.

But this is no more than the shell of the engine;—we will suppose the piston to be at such a point that the cylinder shall have a volume of one cubic unit, and we will now put in it, say one unit of mass of the molecules of a perfect gas;—the molecules may be supposed to be lying on the bottom, like dust, or they may be equally distributed through the space, as in any gas, but they will be *devoid of motion.* We have now added the muscles, perfect in all respects, except that they are dead, and our engine is no more capable of performing its functions than would be a dead crab or lobster. The engine is not capable of transforming heat into work, for the reason that *the agent* by which such a transformation may be effected *is not present;* the space exists through which the piston may move, but, inasmuch as the molecules exert no pressure against the piston, there can be no question of work. Work is the product of force into space, and one factor, the characteristic one, is here wanting.

To obtain the needed pressure we must heat the molecules of the gas to the absolute temperature τ, *i. e.*, we must store in the molecules an amount of kinetic energy proportional to τ, and calculation shows that the pressure per square unit, which will then result from the rebounding of the molecules from the piston and walls of the cylinder will be proportional to this amount. It will also, should the volume be increased by changing the position of the piston be inversely proportional to that volume. In the ordinary formula which connects the volume, pressure and temperature of a perfect gas,

$$pv = R\tau,$$

we have but to suitably change the value of R, say to R′, to be able to write

$$pv = R'W',$$

where p and v are the same pressure and volume as before, R′ a new constant and W′ the convertor quantity of energy, referred to in the law as given above, and which we have stored up in the mass of the molecules to act as the agent or instrument for the conversion of heat into work or *vice versa.*

I wish to emphasize this point:—the engine consists of mass and energy, and this mass is divided into two portions—the metallic parts of the engine, which are made in the machine shop, and the molecular masses, which we will suppose to be supplied in the physical laboratory. The animal is not perfect without life, it is not an animal and cannot perform the functions of one, and so it is this stored-up energy, which is the real agent in the engine, and without which the engine may be said not to exist.*

Let us now see what is Rankine's last and most general statement, in which he generalizes the second law. "The effect of the presence in a substance of a quantity of actual energy, in causing transformation of energy, is the sum of the effects of all its parts." Here he distinctly places energy before us as the agent for the transformation of energy, and I believe that *nothing but energy can act as the agent*, and that the gener-

* Rankine states somewhere that it is the working fluid which is the fundamental part of the engine, but I think that in this Rankine falls behind himself.

al law underlying all such transformations may be stated, perhaps, in the following form:

Every conversion of energy from a form A *into a form* B *can be effected only through the agency of a quantity of energy and* (I venture to add) *this agent, or convertor quantity, must possess at once the characteristics of* A *and* B.

In the air-engine the agent has temperature and pressure, respectively, characteristics of heat and work; in the dynamo the field is characterized by both electrical and mechanical tension. In fact, this seems to me to be the only way in which we can logically conceive of such a transformation, and the only way in which observed transformations occur.

Now, our agent quantity of thermal energy must act without expense of energy to itself, for this is the peculiarity of agents, that they always make themselves safe when they do business for others; therefore the expansion must be isothermal, and a source of heat at the temperature $\tau + d\tau$ must be applied to the conducting bottom, so that the gas will be caused to expand with no loss of its own energy. In reality, *there is no other expansion than isothermal for the conversion of heat into work;* as to adiabatic expansion, it is never used for that purpose, nor can it be; it furnishes, simply, a means of lowering the temperature of the gas if the engine is made to run in a cycle, and the temperature may be lowered equally well by regenerator plates, the only difference being in the form in which the energy taken away from the agent is stored up. Adiabatic expansion does not take heat from the source and transform it into work, and the only expansion that does is isothermal; and in any mixed expansion it is only the isothermal element that is of any value for this purpose.

Now, the gas, in expanding from the volume unity to the volume v, converts a quantity of heat into work, and this quantity will be infinite when v becomes infinite. Also for any given change of volume, say from v to v', the amount of heat changed into work will be proportional to the pressure, that is, proportional *to the agent energy;* but this is just what the second law states, and the proof is therefore seen to be of the utmost simplicity, depending simply on the mutual proportionality of work done, the pressure, the temperature, and the agent energy, or total actual heat (as Rankine calls it) of the working substance.

Let us look now at the behavior of the molecules; as each rebounds from the hot bottom its stock of agent energy receives into itself a portion of the heat W, which is to be changed into work. The molecule is driven away with increased velocity by the hotter, or more energetically vibrating, molecules of the bottom; it carries its added energy to the piston, and gives only this quantity up in the form of work. The reason that it gives it up is simply that, if the piston be moving up with the velocity V, the molecule will rebound with a velocity reduced by 2V. As the volume increases the pressure falls, on account of the diminished frequency of the rebounds per unit of surface, and therefore the conversion of heat into work must proceed more slowly, but for any and all particular changes of volume the speed of conversion may be increased to any degree by means of a proportional increase in the amount of the agent. It is interesting, also, to notice the thing in another light; the energy has to be carried over the distance from the hot bottom to the moving piston, which distance increases directly with the increase of volume, and the time required to carry a certain amount will be increased as this distance increases; the way to secure a more rapid conveyance throughout is to increase the speed of the carrying molecules, *i.e.*, the amount of agent energy. Note also that the amount carried per molecule will be at the same time increased.

The molecules have been spoken of as moving directly toward and away from the piston, whereas they should move in all possible directions; it is well known, however, that this involves no error in the result, and scarcely one in the conception.

Let us see now whether there are any real difficulties in Rankine's statements. To understand them most easily, commence with his "General Law for the Transformation of Energy," already quoted, and proceed backward; we come first, then, to a graphical representation of the second law. After explaining the quantities in his diagram and

stating the known relations between them, he requires us to suppose the temperature τ, to be divided into n equal parts. Now, we can divide nothing except quantity into equal parts, and therefore Rankine regards temperature as a quantity, and so it is. We have only to remember that τ is the temperature of the agent, and therefore τ is the amount of agent energy in terms of a suitable unit; in fact, the statement that a unit mass of gas is at a temperature τ is equivalent to saying that it possesses τ units of thermal energy, the unit used being the energy required to raise this amount of gas one degree in temperature.

It is the agent energy, then, which we are to divide by n, and we are afterward told that the parts will be "similar, and similarly circumstanced." Let us suppose a molecule heated from absolute zero to the temperature τ, by adding n equal increments of energy, these increments can differ only in the order in which they may be added, and once added this distinction vanishes, and they become merged into the quantity of energy contained by the molecule.

On the scale of the thermometer the degrees all have their places, and the last degree added is necessarily the first one subtracted; but no similar conception can be applied to the energy of the molecule, and we can imagine no connection between the last increment added and the first subtracted, or any difference whatever between the increments, whose sum is the total energy of the molecule.

"Similar and similarly circumstanced" mean, therefore, simply devoid of absolute and relative differences.

In this graphical treatment Rankine shows isothermal expansion, and his second law applies essentially to this expansion.

Rankine's next statement (proceeding backward) of his law is the second one criticised by Maxwell, and it supposes nothing more than the division of temperature already discussed. In some earlier editions the first formula under the statement is in error by the omission of τ; it should read

$$\tau\frac{d}{d\tau}=Q\frac{d}{dQ},$$

as it stands in the present edition.

We next come to a seemingly more general and comprehensible statement of the law which speaks of "the total actual heat." Now, Rankine has defined this, and it is simply the kinetic energy of the molecule, or that portion of the heat furnished to cause a rise of temperature from absolute zero to τ, which remains in the body as heat.

Inasmuch as Rankine's first statement of his law seems the more general, insomuch has it led, as I believe, to a false comprehension of its meaning. It may seem more general in this way: In the

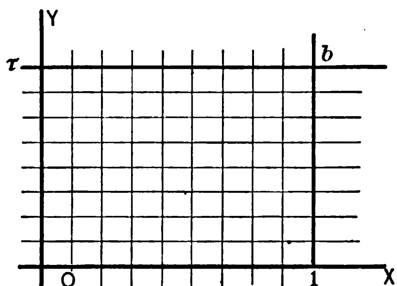

Fig. 1

figure let x represent mass, and O1 a unit of mass; also let y represent temperature; then, if a suitable unit of temperature has been chosen, the area O1$b\tau$ will represent the heat energy contained in the unit mass at the temperature τ. Now, we may cut this energy into n equal parts by means of equidistant vertical lines, and these lines will cut the mass also into n equal parts; in this case it requires no scientific imagination to see that the effect of the heat belonging to any one of the increments of mass must be the same as that belonging to any other, and that the heat of one increment is similar to that of another, though not quite similarly circumstanced, because the increments of mass have different positions in space.

Such a method of dividing the total actual heat is altogether too simple for the use to be made of it, and no such statement can pass for a law of thermodynamics; it is simply the law of homogeneousness. If the mass of a homogeneous body be divided into n equal parts, then the effects of these parts are the same in making up the total weight, value, density, etc., etc., of the body.

Rankine leads up to his statement in an unfortunate way, perhaps, in empha-

sizing the fact that every particle is equally hot without telling why; of course every particle must be so, or we could not speak of the body as having a temperature τ, and the right line τb in the figure would disappear; this would interfere with the argument, which depends essentially upon our dividing a temperature τ into equal parts.

Rankine intends no such division; he says, in fact, "Let unity of weight," and we may take a differential unit, and so put such a division out of the question. The division intended was by horizontal lines, and this makes the first statement of the law identical with the second, only in the one he says "total actual heat," and in the other "temperature." He commences by saying "heat," because heat is energy and is the real thing he refers to, and he changes to "temperature," because that is the praccal way of measuring the amount of this energy, being proportional to it.

I believe, then, that this is the one and only second law, and it follows that because our agent is a quantity of energy, which resides only in mass, and because different substances do not differ in mass —that being a common or fundamental property of all substances—therefore, the particular working substance used has no effect upon the result.

To further illustrate the division into equal parts, which Rankine intends, let us consider the pressure produced by a mass under the action of gravity.

The rectangle O1$b\tau$ will represent this pressure for a unit of mass if we lay off the acceleration of gravity on OY so that $\tau = g$. We shall have the same two possible divisions as before; that by vertical lines will correspond with the fact that each equal element of mass contributes its own equal share of the pressure, while for the horizontal division we must regard the force of gravity as made up of equal increments, each of which takes equal part in producing it. Now, it is a matter of no consequence whether we say "divide the acceleration g into n equal parts" or, "divide the force gm;" in the one case each increment will be represented by a portion of the line OY and will correspond to an increment of temperature, in the other each will be represented by a horizontal band of the area O1$b\tau$ and will correspond to an increment of total actual heat. There may be more difficulty in conceiving of a division of the acceleration or of the temperature into equal parts, than of a division of the energy or of the force, or weight, but this difficulty lies in conceiving of either acceleration or temperature as a *quantity* and not in the division of that quantity; both, however, must so be conceived before any mathematical or other exact treatment of them can be made. If we conceive of a force or weight, P, as ap-

Fig. 2

plied at a particular point (let it be attached, say, to the geometrical line rq) without our knowing the acceleration of gravity or the mass, but only their product P, then, while both divisions of P into equal parts are still possible, that into horizontal bands would seem to be the most natural; any variation in P would usually, I think, be considered as resulting from a change in the acceleration, in which change each equal part will have the same effect in producing the pressure P, and they will all be similar and similarly circumstanced.

If the camel's back does break, it is the first hair as much as the last that does it —this illustration, however, corresponds to the vertical sub-division, and we must suppose the fully-loaded camel to move through space in such a direction that gravity shall continually increase, in which case it will be the first increment of g as much as the last, which will tend to break his back.

Let us look again at the formula for efficiency, which flows directly from Rankine's law in a simple and evident manner. Our infinite-cylinder engine needs no such formula, for it works *at* a temperature τ and not between two temperatures τ and τ'; it transforms, also, all the heat into work. Mechanical considerations, however, require us to forego infinite cylinders and to build engines

that run in cycles, and for these this formula is needed. Every engine running in a cycle is a double engine and consists of an engine proper and a compressor. When the piston has risen as far as the practical height of the cylinder admits, during which rise it has converted all the heat used into work, some method must be taken to repeat the process with the same engine. The engine is therefore used as a compressor for re-compressing the gas, during which operation all the work done on the gas is transformed into heat, and we have only to cool the gas, by adiabatic expansion or a regenerator, before compression in order to get our gas back again to its original density and have left a margin of heat permanently transformed into work, in accordance with the efficiency formula.

It should be remarked that no special reference has been made in this address to anything but a perfect-gas engine because the theory of the action of such an ideal gas should be perfectly clear before the more complicated action of real substances, especially liquids and solids, is considered; Rankine's law, however, covers all such cases, and his definitions and formulæ are framed to include them.

Many other points have necessarily been left untouched, but if I have made clearer how heat and, therefore temperature, may be supposed to consist of a number of equal parts, and how these parts cannot differ in their situation or action, and convinced you that Rankine's is the real and only second law of thermodynamics, my object will have been accomplished.

Note.—A mathematical demonstration of the relation between the total actual heat of a perfect gas and its pressure upon the walls of the containing vessel will be discussed in a future number of this journal.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The transactions of the Society for July and August contain valuable reports upon preservation of timber.

The July issue contains the report of a committee presented at the last convention. The August number contains twenty appendices to the report.

A paper on the Preservation of Forests, by Francis Collingwood, with discussions on both of the above topics, make up the September number.

THE ENGINEERS' CLUB OF PHILADELPHIA.—The latest issue of the Proceedings contains:

Wrought Iron Stringers for the Substructure of Railroads. By William Lorenz.

The Pennsylvania Schuylkill Valley Railroad Tunnel at Phœnixville. By M. Van Harlingen.

Sources of Pollution in Storage Reservoirs. By A. Harvey Tyson. Discussion.

Strengthening the West Main Abutment of Chestnut Street Bridge. By J. Milton Titlow. Discussion.

The Construction of Street Railways in the United States. By William Wharton, Jr.

Pertaining to Isthmian Transits. By Preston C. F. West.

The Water Works of Columbus, Ga. By Jacob H. Yocum.

ENGINEERING NOTES.

ONE of the many odd callings which the production and pipe line transportation of petroleum have made necessary is one known as "chasing the scraper." Crude petroleum is run from the oil regions to the refineries at the sea board, a distance of 300 miles, in iron pipes. It is forced over the high hills that intervene by powerful pumps. Much of the way it runs by its own gravity. The pipes are constantly becoming clogged by sediment and paraffine. To clean them out an iron stem, 2½ feet long, to which are attached circular steel scrapers, fitting loosely in the pipes, is placed in the pipe at regular periods. This is forced along the line by the pressure of the oil behind it. It is necessary to keep track of this scraper, in order that its exact location may be constantly known, so that if it is stopped by any obstacle it may be readily discovered, and the obstacle removed. The noise made by the scraper against the iron pipes as it moves along their interior would not be heard by an untrained ear, but certain servants of the Pipe Line Company are able to follow it on its journey by the noise, and never lose its situation. These men are the scraper chasers. They are stationed in relays three or four miles apart along the line. One chaser will follow up and down mountains, across ravines and through streams and swamps until he reaches the end of his section, when another man takes up the chase and follows it until relief is reached, and so on until the course of the scraper is run. The work is one of hardship and danger, owing to the character of the country through which miles of the pipe line is laid. If a chaser by any mishap is thrown off the track of the scraper, and it becomes clogged before he can recover its position in the pipe, the cutting of the pipe for long distances is frequently made necessary that the missing object may be found, a work that is accompanied by much expense and labor.

THE FORTH BRIDGE.—The Forth bridge will connect Queensferry on the Fife shore of the estuary with South Queensferry on the Linlithgow shore, the total length of the bridge

being 2,700 yards, or 1,900 yards between high-water mark on the two sides. Beginning at the Queensferry side, the bridge is carried out 1,780 ft. on a series of nine piers of solid masonry, faced with cyclopean blocks of Aberdeen gray granite Each of these piers will be 130 ft. in height above high water and the span between them is 160 ft. The fourteenth pier from the shore is called the cantilever tower, as upon it will rest not only the end of the girders, but the end of the shoreward arm of the south cantilever. This tower has, in consequence, been made of great size—the basement or cut-water being 103 ft. in length and 52 ft. in thickness. The crucial part of the scheme is that which carries the railway over the deep water of the Forth—the free navigation of the channel for the largest ships having to be conserved. From three sets of granite piers, arranged in groups of four columns each—the central set at Inchgarvie, and the others about 1,800 ft. on each side—a superstructure of steel will be reared supporting bracket-like arms, two of which, with a separate girder in the center, will form an apparent arch with a span of 1,700 ft., or about one-third of a mile, and with a clear height above high water level of 150 ft. There will be two such spans and two half-spans of 680 ft. each thrown shoreward to cantilever towers on either shore. Foundations for three sets of cantilever piers were obtained by sinking iron and steel caissons, 70 ft. in diameter, to the solid rock at North Queensferry and Inchgarvie, and to the boulder clay at South Queensferry. These caissons were filled with concrete to low-water mark, and above that the piers have been continued to a height of 18 ft. above high water in solid masonry, faced with rough-hewn blocks of granite. These cylindrical masses of concrete and masonry from the base of 70 ft. gradually diminish in diameter to 50 ft. In the Inchgarvie pier, the central points of its quadruple members form an oblong, 270 ft. from north to south, and 120 ft. from east to west. In each of the other two great piers the oblong is only 155 ft. from north to south—the distance of 120 ft. from east to west being preserved. On each of these masses of stonework the steel superstructure forming the cantilever bridge will be erected, the three cantilevers being precisely the same in character. On the top of each of the four stone columns forming the pier is placed a skewback, constructed on the cellular principle of thick steel plates. Each skewback is firmly bolted to the masonry by means of a bed plate and tying-down bolts, carried deep down into the masonry of the column. Lengthwise these skewbacks are united by cylinders 12 ft. in diameter, while crosswise the binding is by lattice girders. From the center of each skewback rises a cylindrical column 12 ft. in diameter to a height of 340 ft., each member bending inwards. Viewed collectively along the length of the bridge, the four look like two great triangles with flattened tops. Their basal distance is 120 ft., center to center, but at 340 ft. up they have converged to 33 ft. On the inner side of each of these columns cylindrical shafts of the same type are carried from the foot of the one column to the top of the other, and form an X bracing; while atop the four main columns and the cross-bracing are knit together in a rigid framework by lattice girders of a maximum depth of 12 ft. From the outer ends of the skewbacks are thrown brackets, also cylindrical in shape. Each is 680 ft. in length, while from the top of the 340 ft. columns girders descend to within 40 ft. of the end of the brackets or arms. The cantilever bracket and sloping girder are firmly braced together by tubes and girders of diminishing sections, as the end of the arm is neared. A main support to the sloping girder is a great cylindrical shaft, which also rises from the skewback on the outer side of each of the main vertical columns. The two bracket arms connected with each pier being of the same dimensions in all respects, exactly balance each other. The various thrusts and strains—vertical, horizontal, and diagonal—involved in their own weight and the load they have to carry, all meet in the connected skewbacks, where they counterpoise each other. The cylindrical shape for the brackets and upright columns has been chosen as best fitted to resist compression, while the girders are used where it was necessary to provide for a tension strain. At the present time the number of men employed at the bridge is close on 2,500. At the South Queensferry end, out to 2,000 feet from the shore, the whole of the granite piers for carrying the girders have been founded and reared, with rounded cutwater, to a height of 18 ft. above high water. Each pier constitutes an oblong, placed east and west, with a base at low-water level of 52 ft., and with a width of 23 ft. The extreme or northernmost pier of this series is the cantilever tower already referred to, and it, too, is up to the same height above high-water level. The steel girders which will form the crown of this part of the structure are being constructed on the top of the stone caps of the piers at the level just mentioned. Strong staging with supports rising from the bottom of the shallow water is reared between each pier; on this the girders are put together, and, as each span is completed, the girder is dropped on the pier, and the staging removed farther out. The girders will be simultaneously raised to their permanent position at the level of 130 ft. above high water by means of hydraulic jacks, thereby avoiding the expense and inconvenience of high staging. When the jacks are in position, hydraulic power will be applied, and the whole of the girders, forming a viaduct nearly 600 yards long and weighing close on 2,000 tons, will be raised about a foot at a time. As it rises, the ironwork will be shored up and the masonry of all the piers built under it. From the new platform thus formed, another lift will be made, and the operation will be repeated until the necessary height is attained. On the north side the corresponding five piers of the viaduct—all, however, founded on dry land—are carried up to a minimum height of 38 ft. above high water; the girders put together upon them have been finished, and preparations are completed for lifting them into position in the same way. Of the south main pier, for carrying the large cantilevers, three of its group of four members have been founded and two reared to their height of 18 ft. above

high-water level, the third above water level. This is the full height of all the columns of the three great piers. At the fourth, the north-west member of this group, the caisson tilted over and was crushed. A timber casing is being constructed around it, which will practically be a coffer-dam; and when this is completed, the caisson will be pumped dry and the plates renewed. On Inchgarvie the works connected with the great central pier are also well advanced. At one time the island was considered a desirable resting-place for the central supports of the bridge, but it is curious to note that none of the four members of the central pier are actually on *terra firma*. The line of the bridge did not admit of that being done. Of this central pier, two columns are just beyond the west end of the island, and the other two 270 ft. further south. The former two columns are up to their full height above water; for the south-east member the caisson is now being sunk, while the caisson of the south-west member has just been floated into position. The depth of each caisson varies according to the distance it has to be sunk into the estuary bed. The deepest yet sunk in connection with the bridge is 71 ft. below low water. But all are filled solid with concrete and carried up to low-water level. Above this the column is built up of solid rubble masonry from Arbroath, faced with blocks of granite. At intervals iron belts are inserted in the masonry. On the Fife shore the four members of the main pier are all founded and finished. Two are on the land, and the other two are in the water, here of considerable depth. On this pier a portion of the permanent cylindrical tubing is stretched between its western members; and here, too, may be seen in position the bed plate on which the skewback will rest, with its holding-down bolts. On each column there are no fewer than 58 of such bolts each a bar of steel $2\frac{1}{2}$ in. in thickness, with a screw at the top 3 in. in diameter, secured by a nut. The bed-plates are 3 in. to 4 in. in thickness. The bottom of the skewback will be of similar construction. On three out of the four columns of each pier, the holes in the bottom of the skewback, through which the bolts will pass, will be cut of an oblong pattern, 9 in. or so in length by 4 in. across, so as to allow for a certain amount of movement of the cantilevers on the piers. On one column of every pier the bolt holes in the skewback will be circular, and just wide enough to admit the bolt, so that too much play in one direction may be prevented. The piers for carrying the girders inland to the ridge of the high ground between North Queensferry and Inverkeithing Bay are completed to a certain height, and everything is in readiness to lift the viaduct girders which are resting upon them. In the bridge workshops at Queensferry has sprung up an immense engineering establishment, where the whole operations connected with the rolling, bending, planing, and drilling of the steel work of the superstructure of the bridge are being proceeded with. Most of the machinery in the works had to be specially invented or adapted to meet requirements. Almost every machine in the place has some ingenious feature for saving labor, of which Mr. Arrol, one of the contractors, is the author. Among other processes going on is the drilling of rivet holes by multiple machines, working simultaneously a large number of spindles. In all, something like 150 spindles are at work mostly night and day, and from first to last something like 5,000,000 of rivet holes have to be drilled. 45,000 tons of steel will be used in the construction of the bridge, exclusive of 3,000 tons in the viaduct girders, and that of the tubes for the cantilevers about $5\frac{1}{2}$ miles, have to be made. Of the steel work nearly 6,000 tons are ready for erection. The total amount of building done up to the present time is:—Granite set, 220,000 cubic feet; concrete, 43,000 yards; rubble masonry, 25,000 yards. The engineers for the bridge are Mr. John Fowler (engineer-in-chief) and Mr. B. Baker; the contractors, Sir Thomas Tancred, Bart., and Messrs. W. Arrol, Falkiner, and Phillips. The chief of the engineers' staff at Westminster is Mr. A. D. Stewart, and at Queensferry Mr. P. W. Meik, with five or six assistants and four outdoor inspectors; while the contractors are represented at the works by Messrs. R. E. Middleton, W. Westhofen, A. Biggart, and others.

IRON AND STEEL NOTES.

M. P. Gabriel gives the following method of tempering steel, in the *Revue Chronometrique*:—Cyanide of potassium is dissolved and red heated in a metallic or earthen crucible; the pieces of steel are then immersed in the liquid until red, and afterward plunged in water. This process is said to give great satisfaction, and many advantages are claimed for it. The temper is said to be harder, and if a finished piece is under treatment the polish is not lost. It will show a grayish tint, but the original polish will reappear immediately, if a piece of polished wood with the finest rouge is passed over it. It is also said that if the steel has been well annealed, and not put out of shape by the file or the hammer, it will come from the crucible perfectly straight; arbors 4 or 5 centimeters long are not deformed, if tempered by this method. It is recommended as particularly advantageous for tempering escapement springs.

Some curious statements on tempering steel are made in a paper published in *Dingler's Polytechnic Journal*, vol. 225, by Herr A. Jarolimek, "On the Influence of the Annealing Temperature upon the Strength and the Constitution of Steel" Hitherto it has been generally considered that to obtain a specified degree of softness it is necessary to heat the hard steel to a particular annealing color—that is to say, to a definite temperature—and then allow it to rapidly cool. Thus, for example, that steel might anneal—be tempered—yellow, it had to be heated to 540 deg., and the supposition was formed and acted upon that it must be allowed only a momentary subjection to this temperature. Herr Jarolimek says the requisite temper which is obtained by momentarily raising the temperature to a particular degree, can also be acquired by subjecting the steel for a longer

time to a much lower temperature. For example, the temper which the annealing color—yellow—indicates can be obtained by exposing the hard steel for ten hours to 260 deg. of heat; in other words, by placing it in water rather above the boiling point.

A paper was read recently before the Royal Society "On Magnetization of Iron," by Dr. Hopkinson. It contained an account of the results of experiments which have been made on a considerable number of samples of iron and steel of known composition, including samples of cast iron, malleable cast iron, wrought iron, ordinary steels, manganese, chromium, tungsten, and silicon steels. The electrical resistance and the magnetic properties are determined in absolute measure. Amongst the electrical resistances the most noteworthy fact is the very high resistance of cast iron—as much as ten times that of wrought iron. The fact that manganese steel is almost non-magnetic is verified, and its actual permeability measured. The action of manganese appears to be to reduce the maximum magnetization of steel, and in a still greater ratio the residual magnetism, but not to affect the coercive force materially. It is shown that the observed permeability of manganese steel containing 12 per cent. of manganese would be accounted for by assuming that this material consists of a perfectly non-magnetic material, in which are scattered about one-tenth part of isolated particles of pure iron. Some practical applications of the results are discussed.

RAILWAY NOTES.

Railroads in Central Asia.—The Central Asiatic Railway will now be pushed forward very energetically, in pursuance of the recent imperial order. Gen. Annenkoff, charged with the direction of the necessary works, has already started for the Trans-Caspian, and will be there before you receive this letter. Great attention is paid in Russian circles to this railway, which will be of the utmost importance, not only strategically, but also in its relation to commerce. The line will go in a direct southeastern direction to Kakhka, which place is about half-way between Askabad and Sarakhs, but then the road will take a northeastern direction to Merv and further to Bourdalik, on Amu-Daria. In this way the railway, which has hitherto closely followed the Persian frontier, will suddenly branch off at an angle and be continued to a distance of about 50 miles from the new Russo-Afghan boundary, which is thought will be settled by the negotiations between England and Russia. The considerations which have determined the government to choose this direction for the railway are commercial ones, and the line now will be connected with the principal fluvial communications in the Russian possessions in Central Asia, also with the great caravan roads. The expenses of keeping up the railway will be serious. They are calculated at nearly 2,000,000 roubles a year, and during the first year or two the receipts will scarcely amount to 200,000. After that time, when the Central Asiatic merchants have learned to appreciate the new means of communication, the traffic may be expected to largely increase. It is said on good authority that the Central Asiatic Railway will be continued next year across the Bokharian territory to Samarkand, and from thence to Tashkend, the residence of the Turkestan Governor-General. The distance between the two towns is 260 English miles.—*St. Petersburg Letter to the London Daily News.*

Railways of the United States.—The total number of miles of railroad in the United States at the close of 1884, was 125,379, of which 3,977 miles were constructed during the year—the rate of increase being 3.17 per cent. The number of miles making returns of their share capital and funded and floating debts equalled 125,152, against 120,552 for 1883, the increase being 4,598, the rate of increase being 3.8 per cent.

The share capital of the mileage in operation in 1884 equaled $3,762,616,686, against $3,708,060,583 in 1883, the increase equaling $54,556,103, the rate of increase being about 1.4 per cent.

The funded debts of all the lines at the close of the year aggregated $3,669,115,772, a sum $168,235,858 in excess of the total of 1883 ($3,500,879,914), an increase of nearly 5 per cent.

The other forms of indebtedness of the several companies at the close of the year equaled $244,666,596, against $268,925,285 for 1883, the decrease being $24,258,689. The total share capital and indebtedness of all kinds of all the roads making returns equaled at the close of the year $7,676,399,054, a net increase in the year of $198,533,272 over the total of 1884 ($7,477,865,782), the rate of increase for the year being about 2.6 per cent.

The cost per mile of all the roads making return as measured by the amount of their stocks and indebtedness equaled very nearly $61,400, against $61,800 for 1883.

The gross earnings or receipts of all the lines from which returns were received for the year equaled $770,684,908, of which $206,790,701 were received from transportation of passengers; $502,869,910 from transportation of freight; $7,464,099 by lines the returns of which were so incomplete as to preclude their use in the tables giving the general results—the sources of income, amount of tonnage moved, etc., etc.; and $53,749,997 from the transportation of mails and express matter, from investments, and from the sales of lands applicable to the payment of interest or dividends.

The number of persons transported in 1884 by all the lines was 334,814,529, against 312,686,641 for 1883, the increase for the year being 22,127,888, the rate of increase equaling 7.8 per cent.

The number of passengers carried one mile in 1884 equaled 8,778,581,061, against 8,641,-309,674 for 1883, the increase equaling 237,-271,387 persons carried one mile, the rate of increase equaling very nearly 3 per cent.

The distance traveled by each passenger in 1884 equaled 26.24 miles; in 1883, 27.32 miles.

The amount received per passenger per mile

equaled 2.856 cents in 1884, against 2.422 cents in 1883. Had the passenger rates for 1883 been maintained for 1884, the earnings from this source would have equaled $212,617,233, a sum $5,826,532 greater than that received.

The number of tons of freight transported on our railroads in 1884 equaled 390,074,749, against 400,453,439 tons in 1883, the falling off equaling 10,378,690 tons, the rate of decrease being about 2½ per cent. The value of the tonnage moved in 1884, estimating its value at $25 the ton, equaled $9,751,868,725.—*Poor's Manual.*

ORDNANCE AND NAVAL.

VENTILATING SHIPS.—We have just received from Mr. J. M. J. Barton particulars of his system of ventilating ships by means of a close furnace placed in the upper part of the vessel. A series of pipes extends from the several compartments in the vessels to the furnace, which is closed at the bottom, and the doors are made to fit very closely, so that no air can pass to the fire except through the pipes provided for that purpose. The fire in the furnace causes a draught, and as no air can enter except through the pipes, a powerful suction will be produced, and the foul air in the several parts of the ship will be drawn into the furnace. Fresh air will naturally pass into the compartments through passages provided for the purpose. The inner ends of the pipes are closed by gratings, to prevent the entrance of live coals. This device can be applied in any marine vessel, but is especially adapted for steamers, as the furnace of the boilers could be utilized. In sailing vessels a special furnace would have to be provided.

THE steel armor-plated barbette ship Rodney, ten guns, 9,600 tons, 7,000 horse-power, returned to Chatham Dockyard after a successful series of trials of her engines. The official trial was of the most satisfactory character. With a natural draught the following results were obtained:—Mean indicated horse-power, starboard, 4,222; port, 4,040; collective, 8,262; steam in the boilers, 89 lbs.; vacuum in condensers, starboard 28.5 in.; port, 28 in.; revolutions per minute, starboard, 94; port, 93; mean pressure in cylinders, starboard, high, 45.61; low, 11.74; port, high, 43.44; low, 11.50. With forced draught and enclosed stokeholes, the following results were obtained:—Mean indicated horse-power, starboard, 5,598.55; port, 5,558 21; collectively, 11,156.76; steam in the boilers, 90 lbs.; vacuum in condensers, starboard, 27.5; port, 28; revolutions, starboard, 104; port, 103; mean pressure in cylinders, starboard, high, 59.75; low, 12.83; port, high, 60.10; low, 12.78. The rate of speed attained was beyond that anticipated, over 17 knots per hour being made, notwithstanding the fact that the vessel's bottom was foul through having been in the basin at Chatham so long. The machinery worked with smoothness and regularity, the boilers generating an ample supply of steam, and no hitch occurred.

AT the meeting of the Royal Geographical Society held recently, Sir Peter Lumsden read a paper on the countries and tribes he has recently visited west of Afghanistan. He gave an interesting description of the geography of the Murghab valley and the customs of its people, and quoted a singular account of the Numaksar, or salt lakes of Yar-oilan, visited and described by Captain Yate. He said:—"The valley of the lake from which the Tekke Turkomans from Merve get their salt is some six miles square, and is surrounded on all sides by a steep, almost precipitous, descent impassable for baggage animals, so far as I am aware, except by the Merve road, in the northeast corner. The level of the lake I made to be about 1,430 ft. above sea level, which gives it a descent of some 400 ft. from the level of the connecting ridge, and of some 950 ft. below the general plateau above. The lake itself lies in the center of the basin above described, and the supply of salt in it is apparently unlimited. The bed of the lake is one solid mass of hard salt, perfectly level, and covered by only 1 in. or 2 in. of water. To ride over it was like riding over ice or cement; the bottom was covered with a slight sediment, but when that was scraped away the pure white salt shone out below. How deep this deposit may be it is impossible to say, for no one has yet got to the bottom of it. To the east of the dividing ridge is the second lake, from which the Saryks of Penj-deh take their salt. The valley in which this lake is situated is much the larger of the two. The valley proper is itself some fifteen miles in length by about ten miles in breadth. The salt in this lake is not smooth as in the other, and did not look so pure. It is dug out in flakes or strata, generally of some 4 in. in thickness, is loaded into bags and carried off on camels for sale without further preparation."

THE LOADING OF HEAVY ORDNANCE.—The muzzle of a 9-inch gun was blown off at Shoeburyness during some experiments with a Hadfield steel shell a few weeks since, and the fragments have been received at Woolwich for examination in the Royal Gun Factories. So little damage has the gun suffered that it could still be fired effectively, and the extra weight of the projectile, coupled with the strength of the powder charge, amply account, in the opinion of the authorities, for the accident having occurred. The event has, however, says the *Morning Post*, brought into prominence the fears which have been felt for some time past, that the advocates of extreme charges are sailing dangerously close to the wind. The artillerists by whom these charges are fixed are satisfied by repeated experiments of the enduring qualities of the new guns which are now being manufactured, but it is worthy of remark that the cartridge of the 10.4 gun is to be 190 lbs. of powder, whereas the much larger gun of 15.5 inches (the 38-ton gun) has a charge of 160 only. It should be borne in mind, however, that the violent and refractory qualities of gunpowder have been of late subdued beyond all expectation. When the "rifle large grain" was the only powder used for great guns, pressures of 60 tons were by no means uncommon, but

as soon as the pressures began to be measured, they began to diminish until the introduction of the pebble powders, scientifically compounded, sized and shaped, brought about the rule of 25 tons as the maximum pressure. Since that, however, the cocoa and prismatic powders have been invented, and the present "brown" powder, which is adopted for all the large guns in the service, is found to give the full velocities with pressures seldom exceeding 16 tons. In theory, therefore, the gunners might safely fire charges of the new gunpowders three, if not four times the weight of those which were fired when the subject of explosives was less understood.

BOOK NOTICES.

Publications Received.

Papers of the Institution of Civil Engineers:

No. 2046.—The River Buffalo; its Flow, etc. By William Bloomfield Tripp, M. Inst. C. E.

No. 2049.—The Cape Government Railways. By William George Brounger, M. Inst. C. E.

No. 2059.—The Electrical Regulation of Steam Engines. By Peter William Williams.

No. 2071.—Public Works of the Orange Free State. By Gustave Hallé, Assoc. M. Inst. C. E.

No. 2077.—Purification of Water by Means of Iron. By William Anderson, M. Inst. C E., and George Henry Ogston, Assoc. M. Inst. C. E.

No. 2078.—Pollution of the Thames near London. By Robert William Peregrine Birch, M. Inst. C. E.

Introduction to Poor's Manual of the Railroads of the United States for 1885. New York: H. V. & H. W. Poor.

Cassell's Family Magazine for September, The Magazine of Art for September, The Quiver for September. New York: Cassell & Co.

Beton Coignet and Goodridge System of Constructing and Repairing Railways, etc. Pamphlet.

The Application of Wire Rope Tramways for Purposes of Economical Transportation. By F. C. Roberts, C. E. Trenton: Trenton Iron Co.

Measurement of the Force of Gravity and Magnetic Constants at Bonin Island. By A. Tanakadate, Tokio University.

Schedule and Itinerary of Exercises of the School for Engineers at Rome, Italy.

Water Meters. By Ross E. Browne. Science series, No. 81. New York: D. Van Nostrand.

This little book chiefly interests those people who have to employ or submit to the registration of their water supply by some one of the few mechanical devices which can presumably measure a flow of water of varying velocity.

Descriptions and accounts of comparative tests of the Worthington, the Kennedy, the Siemens, the Hesse, the Frazer and the Crown meters are given. The descriptions are rendered clear by excellent illustrations.

Transactions of the American Institute of Mining Engineers. Vol. XIII. New York: Published by the Institute.

This volume has been prepared for publication with remarkable celerity, the proceedings of the meeting of February last being included.

It comes as usual in the form of an exceedingly well-printed paper-bound volume; royal octavo size, 806 pp.

The Preservation of Timber by the use of Antiseptics. By Samuel Bagster Boulton. Science series, No. 82. New York: D. Van Nostrand.

Engineers engaged in all branches of professional labor are interested in the subject of the preservation of timber.

The fact that underground conduits for telegraph lines are in some cases made of prepared timber lends a new interest to the subject. This little treatise presents the views of the members of the British Institution of Civil Engineers.

Many experiences are cited and valuable results recorded.

That the subject is held to be of prime importance is well proven by the fact that the last two issues of the Transactions of the American Society of Civil Engineers are devoted exclusively to this same subject.

A System of Iron Railroad Bridges for Japan. By J. A. L. Waddell, C. E. Published by the University of Tokio.

This voluminous work, consisting of two volumes of royal octavo size, is designed to give instruction to engineering students in Japan. It affords, by aid of numerous plates and many tables, the means of designing iron bridges of the prominent American varieties, even to the smallest details.

The calculations are easy, and the descriptions of parts are drawn out in detail, to suit the condition of learners who are laboring with a newly-acquired language, and are seeking to solve new engineering problems.

The treatise is No. 11 of the Memoirs of the Tokio Daigaku, or University of Tokio. It is a credit to the accomplished author as an engineer and as an instructor.

A Catechism of the Steam Engine. By John Bourne, C. E. New edition, much enlarged and mostly rewritten. New York: D. Appleton & Co.

No particular comment on the scope and aim of this book is called for. There is no extensive treatise on the steam-engine in the English language that does not quote or refer to Bourne's treatises, and nearly all students who have encountered the subject of steam, and have needed a clear and comprehensive elementary work, have been referred to Bourne's Catechism.

A revision of the original well-known edition was naturally demanded by the recent progress in engineering. Such revision it has received at the hands of the author. The preface asserts that he has been unsparing in his excisions, but the total result of his revision is a larger book than before.

The part explanatory of the scientific principles of steam and gas engines is much fuller

than in the earlier edition, and the modern doctrine of thermodynamics is fully expounded. New forms of compound engines are described, and a new chapter on air and gas engines has been added. A collection of data for practical work is also a new and valuable addition.

MAIN DRAINAGE WORKS OF THE CITY OF BOSTON. By ELIOT C. CLARKE. Boston: Rockwell & Churchill.

This work will be gladly welcomed by the civil engineers throughout the country. The successful solution of a great engineering problem is described and profusely illustrated

The cure of defects in a system of sewerage, already established and long in use, involves intricate questions of health, economy, and public policy, apart from the engineering work. The particular solution for any city is dependent upon the location and topography In the case of Boston, the question of method was wisely submitted to a commission of eminent engineers. The history of the new system naturally begins with the report of this commission. An account of the survey follows, and then a description of the entire new system.

The work from 1876 to 1880 was under the direction of Joseph P. Davis, a prominent member of the American Society of Civil Engineers. Upon the resignation of Mr. Davis, Mr Henry M. Wightman became his successor. Mr. Clarke, the author of the present report, was the principal assistant-engineer in charge.

Boston is justly proud of her drainage, and American engineers may regard with satisfaction the evidence afforded by this work of the high order of achievement to be credited to the engineering profession in this country.

CHEMICAL PROBLEMS. By DR. KARL STAMMER, translated by W. S. HOOKINSON, A. M. Philadelphia: P. Blackiston, Son & Co.

Instruction in elementary chemistry is now considered incomplete unless it includes a course of problems. Of late it has been the custom with authors to append suitable examples to each section of their text-books. But many good text-books are deficient in this respect, and the need is then only supplied by separate books of problems. The neat little book before us will supply such a deficiency to a fair extent.

There are 101 problems in the book, involving the principal reactions in elementary chemistry, and with proper attention to their economic bearing. There is scarcely enough variety of kind, however. Problems relating to the determination of the symbols of compounds when the percentage composition is given; to determination of atomic or molecular weight from specific heat; to gaseous volumes of products in burning hydro-carbon vapors, and to electrolysis, might profitably be added to the otherwise good collection.

ELEMENTARY MECHANISM. By ALBERT W. STAHL, M. E., and ARTHUR F. WOODS. New York: D. Van Nostrand.

The attention of instructors in mechanism and of students of mechanical engineering is called to a new text-book on Elementary Mechanism, by Arthur F. Woods and Albert W. Stahl. The authors are engineer officers of the U. S. Navy, for several years past on duty as instructors in mechanical engineering.

The book was prepared by the authors on account of the difficulty of obtaining a suitable text-book for the use of their classes of students. There was no lack of works on the subject, but it seemed to the authors that, for their special purposes, some of the existing books were too vague and incomplete, while others partook more of the nature of exhaustive treatises than of text-books. The aim in the preparation of this volume has been to produce a clear, concise, and accurate *text-book.* All *purely* theoretical discussions have been avoided, except when they led to direct practical results. While much has thus been omitted that is of merely abstract interest, yet it is believed that nearly all that is of direct practical importance will be found in this volume.

An introductory chapter of definitions and general explanation is followed by a chapter on elementary propositions concerning the various modes of transmitting motion. The third chapter treats of the transmission of motion by rolling contact, and includes the discussion of rolling cylinders, cones, hyperboloids, ellipses, and multi-lobed wheels. The next four chapters treat of sliding contact. Special attention is given to the teeth of wheels, and full details are given of the practical method of laying out gearing of all kinds, both by the exact methods and by means of the different methods of approximation. Under the head of sliding contact are also shown the methods of laying out cams, screws, worm-wheels, slotted links, escapements, quick-return motions, etc. The next chapter is devoted to a full discussion of the transmission of motion by means of various kinds of linkwork The usual click and ratchet motions come in this chapter. The following chapter treats of the transmission of motion by bands or belts, and includes the discussion of twisted belts, guide pulleys, speed cones, etc. Having thus discussed the four varieties of elementary combinations for the transmission of motion, the next chapter treats of the manner of employing them in trains of mechanism The manner of designing practical trains of mechanism to give any desired velocity ratio is explained by a number of problems worked out in detail. The last chapter is devoted to the discussion of aggregate combinations, such as Weston's pulley, differential screw, parallel motions, drill-feed motions, oval chucks, etc.

A collection of practical problems is added, to give the student additional practice in the application of the principles acquired.

The book contains 300 pages, and is profusely illustrated.

EXTERIOR BALLISTICS. By CAPTAIN JAMES M. INGALLS, Captain First Artillery, Instructor Fort Monroe, Virginia. Printed at the United States Artillery School.

Captain Ingalls has just published the course of exterior ballistics in which he instructs the students of the Fort Monroe School. This course, remarkable for the spirit of method and of healthy discussion shown in its preparation,

is divided into two parts, which we shall pass in review successively.

I. *Resistance of the Air.*—The author, remarking that the theory of gases is not sufficiently developed to permit a theoretical study of the question, announces that he is simply seeking an experimental law sufficiently approximate. He begins by establishing the law of the square of the velocity by the well-known demonstration based on the consideration of the impact of the moving plane against the gaseous molecules when the velocity is normal to its surface; then he treats the case of the oblique movement, and adopts the following formula, quoted by Poncelet in his *Méanique Industrielle*: $P=ps\frac{2}{1+sec^2\varepsilon}$. p being the pressure on the normal element, s the surface, and ε the angle which the plane element makes with the direction of the motion. He applies, then, this formula to surfaces of revolution as well as to projectiles with conical, ellipsoidal, and ogival heads, remarking that these applications have no experimental sanction, since, to have the true resistance, it would be necessary to eliminate the pressure on the rear portion of the projectile, a pressure whose value is unknown. We must, then, be content with empirical formulæ.

After an historical summary of former experiments, the author studies the method generally adopted to estimate the resistance of the air, knowing the velocity of the projectile in two points; he points out the error committed, the error of the principle, by admitting that in this intermediate path the resistance is constant, and corresponds to the mean of the two observed velocities; he calculates the error thus committed in the case in which the resistance would follow the law of the square or of the cube of the velocity. After having given a summary of the formulæ of General Mayevski, relative to spherical projectiles, he passes to Bashforth's method, based, as we know, on the measure of the time of passage of the projectile between ten consecutive and equidistant screens, and much more rigorous than the preceding method.

We know that Mr. Bashforth puts the resistance of the air under the form $2bv^3$, b being a constant, considered as constant in the analysis, but which varies, in reality, with the velocity, so that we must give it in each case a particular value taken from special tables.

Captain Ingalls, while adopting the results given by Mr. Bashforth, does not keep them under the form $2bv^3$, but in discussing the values of b, or, more correctly, of a constant k, which is proportional to it, he is led to write the following laws, viz.:

$$\rho=\frac{d^2}{g}\,A\,f\,(v),$$

d being the diameter of the projectile in inches, g and v being expressed in feet; we shall have

For $v>1330'$, $f(v)=v^2$, log. A $=\bar{4}.1525284$
from 1330′ to 1120′, $f(v)=v^3$, log. A $=\bar{7}.0364351$
from 1120′ to 990′, $f(v)=v^6$, log. A $=\overline{17}.8865079$
from 990′ to 790′, $f(v)=v^3$, log. A $=\bar{8}.8754872$
from 790′ to 100′, $f(v)=v^2$, log. A $=\bar{5}.7703827$

These formulæ are analogous to those given by General Mayevski in 1882. Capt. Ingalls compares them, and remarks that these last give a lesser value for the resistance of the air, which he explains by the more elongated form of the Krupp projectiles. Finally, he closes this remarkable discussion by generalizing his formulæ, and extending them to projectiles of different forms by the aid of the following demonstration:

If we determine for the same velocity the values A and A′ of the coefficient A of the foregoing formulæ for the typical projectile and a different projectile, we can take, in general $\rho'=\frac{A'}{A}\rho$, ρ' being the resistance opposed to the second projectile, and ρ the resistance given by the above-mentioned formulæ, which allows our using for the new shell the ballistic tables calculated for the first.

This principle is admissible with the reservation, however, that we shall determine experimentally, for different velocities, the values of the ratio $\frac{A'}{A}$, in order to establish its constancy, or else the mean value to be used in each case.

II. *Motion of Projectiles.*—In this second part the author passes in review the different methods now employed; Bashforth's method, in which we express the different elements as functions of the angle θ of the tangent with the horizontal, and in which the data of the problem are changed by the adoption of a mean value for the variable coefficient "k" of the formula of resistance kv^3; Siacci's method, in which all the elements are given as functions of the velocity, and in which, as in Didion, the artifice consists in replacing $\frac{1}{\cos\theta}$ by a constant value a.

He applies this last method to the expressions that he has established to represent the resistance, and calculates the corresponding numerical tables.

It is to be noticed that the author has completely omitted the study of the "derivation," and has examined only the projection of the motion on the plane of fire. We do not know whether he reserves this question for another work, or whether, considering the lack of practical importance of the phenomenon, he has believed that he ought to neglect studying it—a study which the insufficiency of our knowledge of the theory of gases renders, moreover, somewhat unsubstantial.—*Translation—Revue d'Artillerie, May*, 1885

This is a high class of text-book. The calculations are in better shape, and the work is mastered and worked out more carefully than usual. The most noticeable original work is in the investigation of resistances to projectiles with heads of different shapes, although, of course, data will be found supplied by Mayevski and Bashforth. The writer appears to have studied most of the best works on the subject. Didion, Krupp, Siacci, Greenhill, Nivin and Mackinlay are quoted. The work is put in good shape by the author, and is a valuable contribution to the literature of gunnery. The

author points out how far English and foreign results are in accordance with each other, and discusses them fairly. It is important in a book of so technical a character to have the opinions of authorities on the subject, such as are quoted in the work itself. Three of these we know to be favorable.

Capt. Ingalls has received a letter from Madrid, requesting the privilege of translating his work for publication in the "Memorial de Artilleria."—*From The Engineer, London, July* 24, 1885.

MISCELLANEOUS.

A floating dome was some time since presented by M. Bischoffsheim to the Observatory at Nice. It is intended to cover a colossal telescope; it is 22 m. in diameter in inside, and has a circumference of 60 m., or 2 m. more than the dome of the Pantheon. Instead of rendering it movable by placing it on rollers, according to the ordinary method, it is closed below by a reservoir for air, which rests on the water in a circular basin. This system of suspension is said to be so perfect that, in spite of its great weight, a single person can turn it completely round the horison.

According to the report of Dr. Frankland, on water supplied to the metropolis during May, the Thames water sent out by the Chelsea, West Middlesex, Southwark, Grand Junction, and Lambeth Companies, exhibited a further improvement as regards organic matter, the average proportion being even less than in any month of last year. All the waters were clear and bright on delivery. Of the water drawn from the Lea, that distributed by the New River Company was, as regards organic matter, second only to the best of the deep-well waters; while the East London Company's supply contained rather more organic matter than the Thames waters. Both samples were clear and bright.

Martin Kiliana, of Munich, has recently described a new method of zinc extraction. The material to be worked—precipitated zinc oxide, calamine, calcined blende, zinc ashes, &c.—is placed in lead-lined wooden vats, and digested with a liquor consisting of ammonium and ammonium carbonate, until the liquor is saturated with zinc. After filtration, the solution then passes to a reservoir, from which it is fed continuously to the precipitation tanks. The cathodes in these are of zinc or brass, the anodes of sheet iron. A portion of the zinc in solution is precipitated in a compact metallic form on the cathodes, with a corresponding liberation of oxygen at the anodes. The liquor passes through the precipitation tanks at a speed regulated according to the amount of zinc contained and the strength of the current at the electrodes, and then flows into the reservoir at a lower level, from which it is pumped up again into the first tanks, to extract a fresh amount of zinc, and pass again to the precipitation tanks. All the vats and reservoirs are well covered over, to prevent loss of ammonia.

The longest bicycle ride ever made has just been completed by Mr H. R. Goodwin, of the North Manchester Club. Leaving Land's End on June 1st, he journeyed to John O'Groats, having reached which point in 7½ days, he at once turned southward, and again arrived at Land's End on the 16th, the double journey of about 1750 miles, or from one extremity of England to the other, having occupied less than sixteen days. From Land's End he rode to London, which was reached on the 19th, the rider having just completed a journey of 2050 miles in exactly nineteen days, or an average of 108 miles per day. Mr. Goodwin rode a 40-inch "Facile" safety bicycle, and arrived in London well.

A Select Committee of the House of Lords, presided over by the Duke of Richmond, has passed the Southwark and Vauxhall Water Bill, which empowers that company to construct a reservoir at Forest Hill, in order to give a high-service pressure to Wimbledon. It also enables the company to affix stop-cocks to every service-pipe in their district for the purpose of preventing and detecting waste, which stop-cock must be paid for by the consumer. The bill also empowers the company to raise £250,000 additional capital, but the Committee have inserted a clause compelling the company to raise the sum by debenture stock to be issued by public tender at par. The clause by which the company sought powers to purchase the dust-sifting yard near their Battersea filter beds was struck out on the opposition of the Brighton Railway Company, the owners of the yard.

The coal fields of Russia are, Mr. W. Mather says, still practically undeveloped. The Donetz coal field is too remote for the manufacturing districts, and the railroad communications are too uncertain to admit of its being largely used. The lignite found within a radius of 200 miles of Moscow does not offer fuel of a sufficiently good quality. It is a remarkable fact that during the past two years English coal has been found to be the most profitable fuel that manufacturers could use immediately around Moscow at a price laid down of about 40s. per ton. Twenty years ago the price of wood fuel was so low as to be equivalent to coal at 10s. per ton, and now coal at 40s. per ton is cheaper fuel. This is apparently a consequence of the reckless destruction of forests in Russia without systematic planting under Government supervision.

In the *Bulletin de la Société d'Encouragement pour l'Industrie Nationale*, M. Carnot, in a report presented by him on behalf of the Committee of Chemical Arts, showed that the cause of the corrosion of sheet copper, employed for the sheathing of ships is the presence of cuprous oxide, which, in contact with salt water, occasioned the formation of soluble salts, even when the air is excluded. In order to reduce more completely the oxygen compounds present in the copper, he introduces a small quantity of metallic manganese, which completely reduces the cuprous oxide remaining in the metal, and becomes converted into a manganese silicate, in contact with the sides

and the sole of the furnace. If a few thousandths of manganese remain alloyed with the copper, they affect neither its malleability nor its resistance to the action of sea water. The manganese is introduced in the form of cupro-manganese, an alloy containing 75 per cent. of copper and 25 of manganese.

M. DE TROMELIN has attacked the hypothesis that clouds are composed of vesicles or hollow spheres of condensed vapor. He supposes that every solid body, whatever may be its diameter, retains around it by adhesion a special atmosphere of the gas in which it is plunged; that the thickness of this atmosphere is nearly independent of the volume of the solid body; that the attraction which retains it is within the domain of the molecular forces, and is manifested only within very short distances. In this way he accounts for the difficulty of completely removing the air from a tube which is to be filled with liquid. In the case of a vesicle surrounded by its atmosphere, the thermal absorption of the water is much greater than that of the surrounding diathermanous air. The atmosphere of the vesicle is consequently expanded, and the particle with its atmosphere floats by displacing an equal volume of the circumambient air. The dust particles which are observed in the sun's rays are supposed to be sustained in the same way.

THE report of Mr. William Crookes, F.R.S., Dr. William Odling, and Dr. C. Meymott Tidy on the water supplied to London during April, states that "throughout the preceding months of February and March, the water supply manifested, although but in a moderate degree, what may be called its wintry characteristics; which our results show to have been put off altogether in the supply of the past month. Thus, while the maximum proportion of organic carbon in the Thames-derived water furnished in February and March was .256 part, and the mean proportion .181 part in 100,000 parts of the water, the maximum proportion in the water furnished during the past month was .152 part, and the mean proportion .141 part in 100,000 parts of the water—the seasonal improvement, it is noticeable, being shown as much in the greater uniformity of the samples, as in the mean reduction of the rarely otherwise than small proportion of organic matter present. Of the 167 samples examined, the whole were found to be perfectly clear, bright, and well filtered."

IN his instructions relating to sewer works, Sir R. Rawlinson observes that Portland cement and lias limes make good hydraulic mortar. The proportions of cement, or of lime to sand, should not exceed 2½ of clean sharp sand to one by measure of ground Portland cement or lias lime. If clean furnace ashes or slag is available, there may be two of sand and one-half of ashes or slag, the whole to be mixed in a revolving pan, each panful to have twenty minutes' grinding. When mortar is used with bricks, the beds and joints should be spread thick and full over the entire area of both bed and joint, leaving, when pressed into place, a bed and joint never less than ⅛ in. in thickness of mortar. In four cubic yards of completed brickwork there should not be less than one cubic yard of mortar incorporated. In making mortar or concrete it will be of the utmost importance to use clean materials and to preserve them clean. The water used for wetting bricks and for mixing concrete and mortar must be free from salt. Concrete and mortar should also be used on clean surfaces.

A BROKEN FILE.—There is no tool so easily broken as the file that the machinist has to work with, and is about the first thing that snaps when a kit of tools gets upset upon the cross-beam of a machine or a tool board from the bed of an engine lathe. It cannot even be passed from one workman to another without being broken, if the file is a new one or still good for anything, if an apprentice has got anything to do with it, and they are never worth mending, however great may be their first cost, unless the plaster of Paris and lime treatment can make a perfect weld without injuring the steel or disturbing the form of the teeth. Steel that is left as hard as a file is very brittle, and soft solder can hold as much on a steady pull if it has a new surface to work from. Take a file, as soon as it is broken, and wet the break with zinc dissolved in muriatic acid, and then tin over with the soldering iron. This must be done immediately as soon as the file is broken, as the break begins to oxydize when exposed to the air, and in an hour or two will gather sufficient to make it impossible for the parts to adhere. Heat the file as warm as it will bear without disturbing its temper as soon as well tinned, and press the two pieces firmly together, squeezing out nearly all the solder, and hold in place till the file cools. This can be done with very little to trim off, and every portion of the break fitting accurately in place. Bring both pieces in line with each other, and, for a file, it is as strong in one place as in another, and is all that could be asked for under the very best of welding treatment.—*Boston Journal of Commerce.*

THE incandescent lamp life-test which has been going on at the Franklin Institute, Philadelphia, has reached its 1064th hour. The *Scientific American* says the Edison, the Weston, the Stanley, and the Woodhouse and Rawson companies competed. The Sawyer-Man and Brush-Swan companies were invited, but declined to participate in the trial. Extraordinary precautions were taken to prevent access to the lamps except by members of the committee. The lamps were lighted on April 11th, and have burned ever since. At 11.35 this morning the Edison Company, who had entered 21 lamps, had lost 1; the United States Company, who entered 24 had lost 17; the Stanley Company had lost 19 out of 22, and Woodhouse and Rawson, an English firm, had lost 11, or their whole number entered. The Edison Company used the natural fiber bamboo carbon, while the Weston people used the artificial tamidene carbon.

VAN NOSTRAND'S

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ON THE STRUCTURE OF STEEL INGOTS.

By CH. WALRAND.

Translated from the "Jernkontorets Annaler," by Magnus Troilius.

Contributed to Van Nostrand's Engineering Magazine.

Much has been written concerning the chemical composition of steel ingots, but only small attention has as yet been paid to the internal appearance of the same. The study of the structure of steel is, however, equally as interesting as the study of chemical composition and may throw light on many facts which cannot be explained by chemical analysis alone.

Steel ingots may be faultless as to chemical composition and still give both good and bad results, owing to different modes of casting the metal into the molds.

Some of the faults that are found in ingots are due to purely physical causes, whilst others are derived from the chemical composition.

The defects found in steel ingots may be divided into:

I. Those due to physical causes;

II. Those due to chemical causes; and

III. Those that arise when the steel is heated.

I. Physical defects in Ingots.

If we split an ingot in the longitudinal direction, a cavity will be found, varying in size according to the general solidity of the metal.

This cavity is called "pipe;" it occurs not only in steel castings, but in castings of other metals, and the higher the melting point of the metal, the larger the "pipe." A few words will explain this.

When a metal of any kind is cast into a cast-iron mold, the outer part, or that part which is in contact with the mold, cools and solidifies whilst the inner part of the metal is yet fluid and hot. The outer part, therefore, by becoming rigid sooner than the inner part, caused the ingot to occupy a larger volume than would have been the case if all parts had cooled equally quickly.

When the cooling takes place concentrically with the mold, a moment will arrive when the solidified outer layers, in the upper parts of the ingot at least, do not longer remain in contact with or receive addition of molten metal, the latter sinking away as the temperature falls, owing to its contraction as well as to its weight.

Therefore, if the ingot is filled in the position indicated Fig. 1, a cavity, A, will be found in the top; and this cavity is larger the higher the melting point of the metal is.

This cavity is not absolutely regular in form, but generally consists of a large cavity A, and several small cavities *aa*.

The large cavity, A, stretches almost

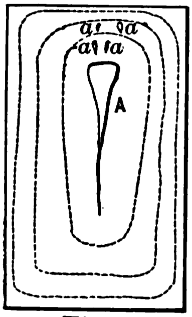


Fig. 1

all the way from the top to the bottom of the ingot, but is of smaller significance in the bottom part, or the part which was cast first.

These central cavities form the "pipe," they are characteristic not for steel only, but for all metal castings. They increase in size with increased melting point and co-efficient of expansion of the metal.

From what has been said it is evident that it must be very difficult to avoid these central defects in steel. We will now show by what means the evil can be lessened.

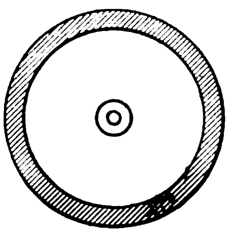


Fig. 2

Let us imagine a perfectly spherical mold, containing fluid steel. It is then evident that, on cooling, a symmetrically-shaped cavity will be formed at the center of the sphere, no regard being taken of the force of gravity acting on the steel, Fig. 2. In reality, however, this cannot take place, because the metal must be poured in through a hole, Fig. 3, and the metal that first comes into contact with the mold is first cooled. The continued inpouring of metal fills up the shrinkages of the first-cooled metal and prevents cavities therein. The force of gravity causes the fluid metal to flow in and fill up any cavities that may have formed. Therefore, when the mold is filled, there will be a layer of first-cooled metal as indicated by the dotted lines, Fig. 3, and the "pipe" will be located somewhat above the center, at *oo*.

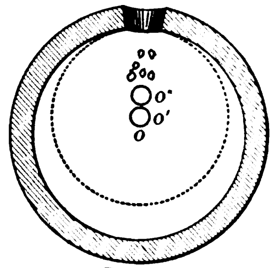


Fig. 3

The position of the "pipe" varies, even for the same metal, with the temperature at casting and the heat-conducting capacity of the mold.

A cast-iron mold should thus be made thicker at the casting hole than at the lower end, Fig. 4. The first layer of cooled metal will then be nearly concentrical with the mold, for the reason that the upper and thicker parts abstract more heat than the lower and thinner parts. The "pipe" will in this case be more centrally located than in a cast-iron mold of uniform thickness.

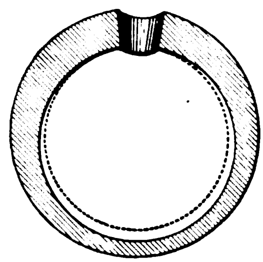


Fig. 4

In general, the "pipe" will be the nearer to the center the less the mold conducts heat, the first-formed layer of chilled metal being thinner. Thus, in a sand mold, the pipe will be more central than in a cast-iron mold.

MOLD MADE OF MATERIALS OF DIFFERENT HEAT-CONDUCTING CAPACITY.

Fig. 6 shows a spherical mold, in which the lower half H consists of cast iron and the upper part (H′) of sand. The dotted lines I, I′, I″, indicate the shape of the chilled layers in this case. The irregular shape is due to the difference in heat-abstracting power between the sand and the cast iron. The "pipe" is here situ-

ated much nearer to the casting hole than if the whole mold had consisted of cast iron.

We will now proceed to describe the various cases that may occur in practice.

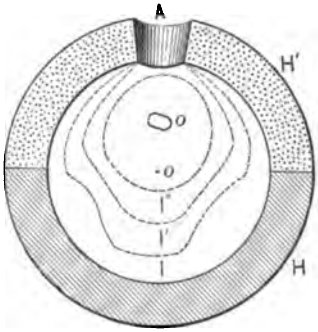


Fig.6

"PIPE" IN ORDINARY INGOTS.

For ingots that are to be hammered or rolled, the fluid metal is generally cast into slightly conical cast-iron molds, open at both ends, and resting with the larger end upon a cast-iron plate.

When the metal has been cast into the mold, water or sand is thrown on top of the ingot according to the more or less quiet behavior of the metal. Hereby a solid crust is immediately formed, which resists the rising of the steel during cooling. An ingot, on cooling, may become either solid or full of holes (honeycombed). In the former case a small amount of water suffices to produce the solid top-crust. The upper surface cools in the air, whilst the other surface parts of the ingot cool in contact with the sides of the mold. The air has much less power to conduct heat than cast iron; Fig. 7

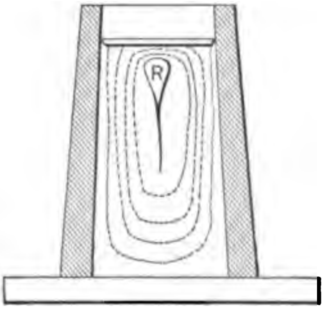


Fig.7

shows how the cooling in the ingot probably proceeds in this case.

If, instead of letting the steel cool slowly in the mold, the ingot is taken out as soon as the top crust is sufficiently thick, and thrown down, so as to rest upon one of its sides, the "pipe" will obtain the position shown, Fig. 8.

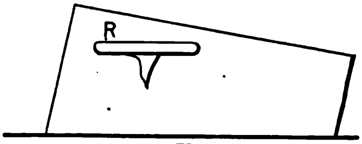


Fig.8

By turning the ingot upside down, as shown, Fig. 9, the "pipe" is moved to the thicker end of the ingot.

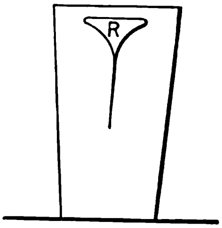


Fig.9

Sometimes a brick, B, is inserted in the cast-iron plate, Fig. 10, to protect the latter from the destructive action of the stream of metal when casting. In this case it may occur that we find a "pipe" R′ in the lower part of the ingot as well as in the upper part, viz.: if the brick be sufficiently large.

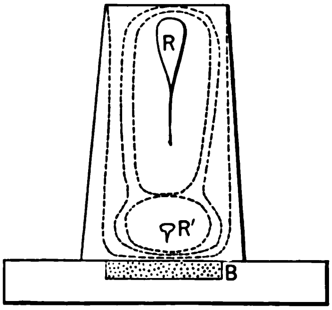


Fig.10

In all these cases we have supposed that the steel is "cast from the top." Now, let us consider the case when the steel is "cast from the bottom."

Fig. 11 shows how the steel enters the mold from the bottom. When the mold is filled, the hottest steel will, of course, in this case, be in the lower parts, and the colder metal in the upper parts. The

"pipe" R will therefore be found somewhat nearer to the bottom than what would have been the case if the steel had been cast from the top.

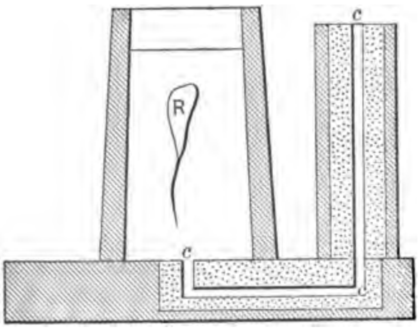


Fig. 11

From what has been said it is obvious that the "pipe" cannot be completely avoided in steel; but its influence on the quality of the metal can be considerably modified, and, in some cases, completely destroyed.

In many cases, where a perfect steel is required, it is necessary to remove that part of the ingot that contains the "pipe." Particularly when hardening steel this becomes necessary, as the steel is sure to crack in that operation if it be internally defective.

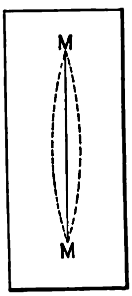


Fig. 12

Suppose we have a forged cylinder of steel, Fig. 12, in which the "pipe" has not been removed, but simply pressed together during the hammering. Let us submit this cylinder to an effective hardening and try to follow the cooling step by step.

As soon as the metal is plunged into the liquid, the outer parts of the cylinder are violently contracted; but, as the inner parts are yet very hot, the outer parts cannot reach the smaller volume which corresponds to the diminished temperature. The outer parts are consequently stretched, and remain in a state of tension. During the second period of the cooling, viz.: when the outer parts of the cylinder give off to the liquid as much heat as they receive from the inner parts, these latter strive to contract; but, as they are intimately combined with the outer parts, which have already become rigid, they are prevented from resuming the volume which they would have occupied if the cooling had taken place in the ordinary way. The inner parts try, in their turn, to contract the outer parts, and finally the whole remains in a state of tension.

Thus, in every hardened piece of steel of the shape mentioned, the outer parts are subject to a compressing, the inner parts to an expanding, strain.

It is evident, then, that the ends MM of the compressed "pipe" may cause ruptures, and it is easy to see that the strains above referred to must weaken the material at these points and make the piece crack.

What has been said regarding the "pipe" can be applied to other similar defects in steel.

THE "SINKHEAD" AS A MEANS OF REMOVING THE "PIPE."

If we investigate the various means for removing the "pipe," the influence of which is so destructive for certain articles, such as steel guns, for instance, we can easily see, from the mode in which the "pipe" is formed, that if the cavities could be filled with molten steel as they form, there would be no "pipe." Unfortunately, it is impossible to accomplish this in practice; but, with the aid of what has been said regarding the casting of steel into molds of different material (Fig. 6), we can come tolerably near the solution of the problem.

Fig. 13 shows a cast-iron mold, placed upon a cast-iron plate. The cast-iron mold has at the top a continuation, consisting of some non-conducting substance. When this mold is filled with steel, and the upper end closed with dry sand, the whole "pipe" will be concentrated in the upper part above the cast-iron mold. If then the part ABCD be cut off, one can be certain of having an ingot free from "pipe."

What now has been said only refers to

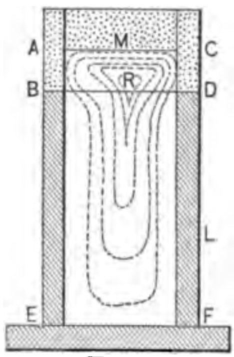


Fig. 13

such steel that does *not rise* in the mold after casting.

It is easy to explain the action of the "sinkhead," or that part of the molten metal that is contained in the non-conducting part of the mold. This sinkhead consists of the hottest steel, or the steel that came last from the ladle or furnace, and is prevented from cooling by the sand and the non-conducting part of the mold. Fluid steel, therefore, flows down and fills the cavities in the ingots as they form. The ingot thus becomes solid, whilst the sinkhead is full of holes.

The sinkhead need not be very big, as compared with the ingot, if it only be wide enough, so as not to become rigid before the formation of cavities in the ingot has thoroughly ceased. Otherwise two "pipes" will be formed, one in the head and one in the ingot, Fig. 14.

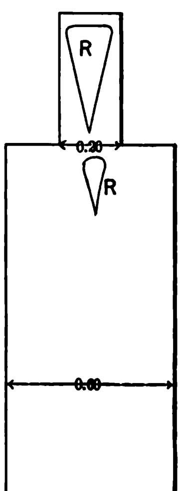


Fig. 14.
Insufficient Sinkhead. 900 Kg. Ingot.

20 % of the height of the ingot should be taken as sinkhead; this does not, however, correspond to 20 % of the weight of the ingot, as the sinkhead is nearly empty.

PRECAUTIONS TO BE TAKEN IN ORDER TO MAKE THE SINKHEAD EFFECTIVE.

It should be borne in mind that what has been said above does only apply to such steel that stands quietly in the molds without evolution of gas. The case of rising steel will be considered below.

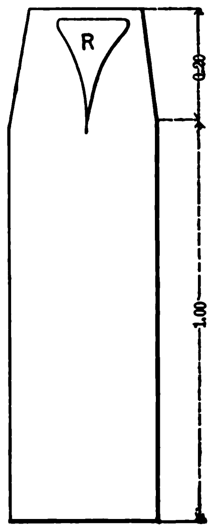


Fig. 15
Sufficient Sinkhead. 1200 Kg. Ingot.

The sand in the sinkhead should be sufficiently refractory not to melt in contact with the hot steel; otherwise the slag thus formed reacts on the steel so as to cool and impede the useful action of the sinkhead. The sinkhead-mold must be perfectly dry before pouring in the metal; the moisture would, if present, cause too rapid cooling.

The sinkhead should have as nearly as possible the same transverse dimensions as the ingot, and its height must not be less than 20% of the total height.

Casting from the bottom must be avoided, because such casting brings the colder metal to the top. When the mold is so high, however, that casting from the top would injure the bottom-plate, the casting may be commenced from the bottom and completed from the top.

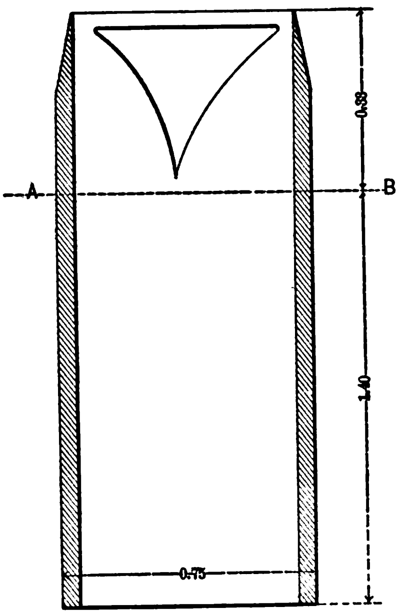


Fig 16

ANOTHER MODE OF USING THE SINKHEAD.

It is not absolutely necessary to make the sinkhead-mold of sand. For small ingots another method may be used; it is not quite so efficient, but in many cases useful.

The method consists in having a mold, as shown in Fig. 17, resting with its *narrower* end on the bottom-plate. The thickness of the sides of the mold is decreasing upwards. The metal is tapped direct into the mold from the top. As is seen from Fig. 17, an upper layer AB acts as sinkhead for a lower layer CD. The metal in AB reached the mould later than the metal in CD, and is consequently hotter. The sides of the mold being thicker towards the lower end more heat is lost by conduction in CD than in AB. Finally, AB being larger than CD, the former cools slower than the latter. For these reasons the metal in the upper part of the mold must be hotter than the metal underneath, thus acting as sinkhead for the latter.

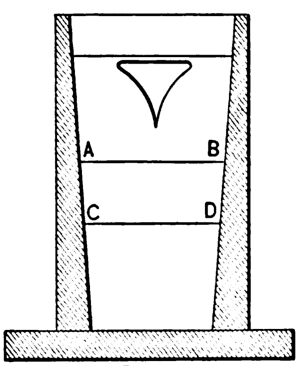


Fig.17

If dry sand be put on top of the steel, the upper part of the ingot will remain superheated sufficiently long to cause the whole "pipe" to concentrate there completely.

MEANS FOR DISTRIBUTING THE "PIPE" THROUGH THE WHOLE MASS OF THE INGOT.

In certain cases it is suitable to have the "pipe" spread through the whole mass of steel instead of concentrating it in the upper part.

The method for doing this, which we will now mention, has only a limited usefulness, but may be applied in certain cases.

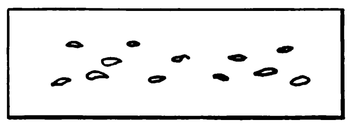


Fig.18

Suppose the ingot to be taken out of the mold and laid upon one of its larger sides. The "pipe" will then occupy the position indicated, Fig. 8, after cooling. If, however, instead of allowing the ingot to cool undisturbedly, we keep rolling it over and over, it is evident that the "pipe" must be split up and distributed throughout the steel, Fig. 18.

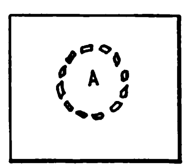


Fig.19

The ingot need not be revolved very rapidly, but the motion must be kept up as long as the metal remains fluid; otherwise the central parts of the ingot will be almost separated from the rest, Figs. 19, 20. This has been confirmed by experience.

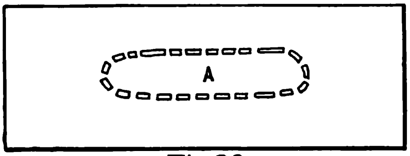


Fig. 20

INFLUENCE OF THE "PIPE" ON CERTAIN FORGED AND ROLLED ARTICLES.

Everybody who has had to do with the rolling of steel rails knows that the end of the rail sometimes shows a crack, R, Fig. 21; this crack is often situated in the web or stem of the rail.

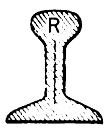


Fig. 21

This crack is due to the "pipe," the sides of which have not welded together during rolling. Such a rail is apt to break at one single impact. The crack in question may often exist in the rail without being visible at the end.

If the "pipe" should happen to occupy the position indicated, Fig. 8, the crack will probably be found in the head or in the foot of the rail.

Doubtless many rails have broken on account of said crack, and not in consequence of some slight deviation from the chemical composition ordinarily used.

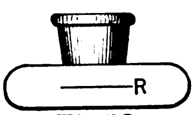


Fig. 22

In tyres the effect of the "pipe" is different. When punching the hole in a round tyre-ingot, it may happen that only part of the crack R, Fig. 22, is removed. In this case a line, R, remains on the inside of the tyre, Fig. 23.

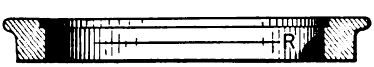


Fig. 23

Such a tyre, after being in service for some time, generally splits in two.

In the manufacture of axles it often happens that too small ingots are used. In this case a crack, due to the "pipe," is

Fig. 24

likely to occur in one end of the axle, Fig. 24, whilst the other end is perfect.

"PIPE" IN STEEL THAT "RISES" DURING COOLING.

Some steels, poor in silicon, when tapped at a certain temperature, continue to rise in the mold for some time immediately after casting.

Whatever be the cause of this phenomenon, it is a fact that it occurs chiefly in soft Bessemer and Siemens-Martin steel.

When such metal is cast into a mold a solid crust is formed where the metal touches the sides of the mold, whilst in the interior gas is being evolved, keeping the fluid metal in a state of agitation. It is probable that, owing to this agitation, the temperature is pretty uniform through the whole mass, and that therefore the "pipe" in this case will not be concentrated exclusively in the top, Fig. 25. The evolution of gases gives rise to

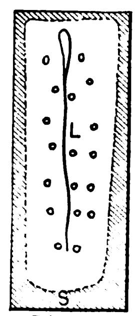


Fig. 25

a number of blow-holes and cavities, thus rendering the ingot unsound. It is evident that the sinkhead is of little use in a case of this kind. The sand, which is used to form the top of the sinkhead, may be drawn into the bubbling mass, where it melts and mixes with the steels, giving rise to violent reactions and explosions. If, therefore, it be desired to use a sinkhead for a rising metal, the sinkhead-mold should be made out of cast-iron, surrounded by sand to prevent the abstraction of heat, Fig. 26. Thus arranged, the sinkhead may be of some use, even for rising metal.

We may now condense what has been said concerning the "pipe," as follows:

1. The extent and importance of the "pipe" is greater the higher the temperature at casting.

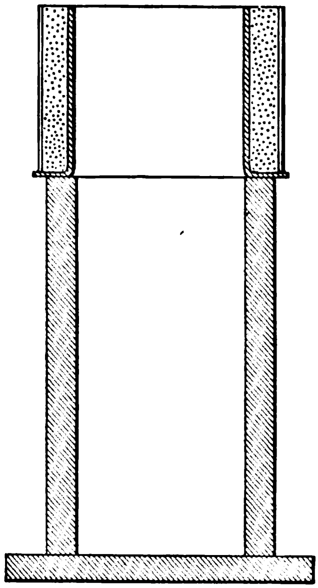


Fig. 26

2. The "pipe" is larger in "quiet" steels than in "rising" steels; in the latter the "pipe" is more evenly distributed through the whole mass, and gives, therefore, less trouble in cases where it is desired to use the *whole* of the ingot.

3. The "pipe" may be concentrated in the upper part of the ingot by promoting the cooling of the lower parts of the ingot and keeping the upper part at a high temperature.

At the exhibition, 1878, Sir Joseph Whitworth exhibited ingots that had been compressed after tapping. These ingots did not show any traces of "pipe," although a "pipe" ought certainly to have been there. Compression being nothing but a kind of forging, it is probable that the lips of the "pipe" had been very closely compressed, so as to leave no lines visible to the naked eye; after polishing and etching with acid it is probable that the steel would have shown marks of the "pipe."

It is, in fact, impossible to entirely remove the pipe; one can only lessen and counteract it by the means above shown.

SURFACE CRACKS ON INGOTS.

When looking closely at an ingot of sufficiently large dimensions, we often discover some cracks, some running transversely or horizontally across, and some lengthwise along the ingot. These cracks are often quite small; they may, nevertheless, cause serious difficulty in hammering and rolling the steel.

The horizontal cracks are generally due to resistance to contraction during cooling, such, for instance, if the ingot stick to the side of the mold in one or more places.

Slightly conical ingots are less apt to crack horizontally than those that are of equal thickness throughout.

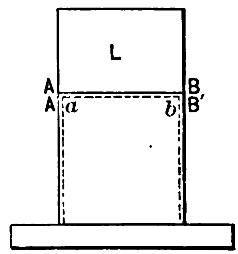


Fig. 27

The explanation of this is easily found. Fig. 27 shows a cylindrical mold; Fig. 28 a conical mold. The sections AB and $A_{11}B_{11}$ contract to ab and $a'b'$ on cooling. Thus, if $AB=A_{11}B_{11}$ we have $AB - ab = A_{11}B_{11} - a'b'$. But whilst $AB=A_1B_1$, we have $A_{111}B_{111} > A_{11}B_{11}$ and $A_{111}B_{111} - a'b' > A_1B_1 - ab$. Consequently, in the conical mold the ingot is further from the mold after cooling than in the cylindrical mold, and thus less apt to stick.

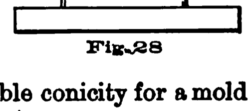


Fig. 28

A suitable conicity for a mold is 25 mm. in one meter.

The principal causes of horizontal cracks are—1st, defective molds; 2d, too hot metal; 3d, suspension of the ingot-top, caused by the metal either flowing over

the edge of the mold or squeezing itself between the stopper and the mold; and, 4th, a defective bottom plate.

The molds must be carefully watched and rejected as soon as they are found defective. Care must be taken not to let the metal strike the sides of the mold when casting, as otherwise cold skins may be formed and the mold eaten out.

Casting from the bottom, as a rule, gives ingots of a better appearance, and more free from horizontal cracks than casting from the top, the metal in the former case rising quietly without being thrown up against the sides of the mold. The coldest metal is at the top, causing the shrinking to begin there; in this way at least one cause for the cracks is removed, viz., the influence exerted by the force of gravity, when the ingot, after having shrunk together at the bottom, remains suspended by the top part, which still sticks to the mold.

Longitudinal cracks may, like horizontal cracks, be due to resistance to shrinking, but, as a rule, they are due to quite a different cause, viz., the pressure exerted on the lower parts of the ingot. A cylindrical ingot is particularly liable to crack from this cause, Fig. 29; such ingots are therefore difficult to make. A square ingot is better, but as the square ingots generally have rounded corners, they are apt to crack longitudinally at those.

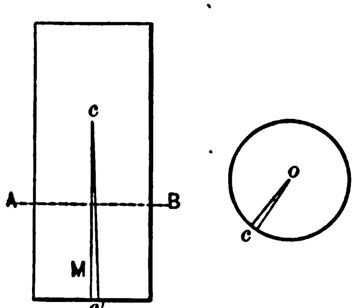


Fig. 29

Casting from the bottom promotes longitudinal cracking, the hottest metal being at the bottom; thus the bottom crust, which has to resist the pressure of the superimposed metal, is weaker than if the metal had been cast from the top. Too rapid casting also promotes longitudinal cracks.

To avoid longitudinal cracks it is therefore necessary—

1st. To give the ingot throughout its whole length such a section as will permit of change of form.

2d. To cast as slowly as possible.

3d. To use the bottom cast as little as possible.

II. Defects due to Chemical Causes.

When examining the fracture of a steel ingot we generally find a large number of small cavities, more or less irregular in shape, some spherical, some oval, and some formed through the combination of several surfaces. Some of these cavities have a pure metallic, silver-white surface, others are colored through oxidation, which shows that they must, at a certain moment, have been in contact with the air; others may be covered with a brown coating,* the cause of which we have not been able to find out.

These cavities, or blowholes, are generally symmetrically grouped in regular geometrical figures; sometimes, however, they are quite irregularly distributed.

When steel is poured into a mold, we see a large quantity of gas being evolved, and more so the softer the metal. Some claim that this gas consists of carbonic oxide; others say it is hydrogen.†

Of late years this matter has caused a good deal of discussion; we, for our part, believe that the gas evolved during the cooling of the steel is simply carbonic oxide, the ingredients of which gas exist in large quantities in the molten steel. For many reasons it is difficult to believe that this gas would be hydrogen. Bessemer metal is generally more free from blowholes than is Siemens-Martin metal, although the former ought to have more hydrogen, owing to the mode of manufacture. And how explain the influence

* When HCl. is poured into a blowhole thus coated, the brown coat is instantly removed with evolution of H_2S.

† After the publication of Dr. Muller's works concerning the gases in ingots, we repeated the principal tests which he made in order to show that steel mechanically occludes hydrogen.

As well known, Muller's experiment consists in boring ingots under water and collecting the gas thus set free.

We found that gas always was obtained when water was used, whilst not a trace could be found when mercury or oil was employed.

The reason for this is not difficult to conjecture. The drill is magnetized by the rapid revolutions, and causes an electric current, which decomposes the water. The gas thus obtained detonates without any extra addition of air and gives water.

of silicon upon the suppression of the blowholes if the latter be due to hydrogen? It is, nevertheless, an established fact that the presence of silicon in steel causes the almost entire disappearance of the blowholes.

It is easy to explain the relation between silicon and dissolved oxides, but it is difficult to see how silicon can lessen the influence of dissolved hydrogen.

We believe that the hydrogen theory has no more foundation than the old theory which claimed that nitrogen was the element that determined the qualities of the steel.

THE DEVELOPMENT AND POSITION OF THE BLOWHOLES IN INGOTS.

Two circumstances exert the greatest influence on the formation of blowholes in ingots, viz., the chemical composition of the metal, and the temperature at which the casting takes place.

Let us suppose that the metal be entirely free from silicon, and that the casting temperature be what we may call normal. A little carbon and oxide of iron always occurs in the metal, whether it be made in a Bessemer or a Siemens-Martin furnace. The respective quantities of these bodies change necessarily with the softness of the metal; the harder the metal is, the more carbon and the less oxide of iron is present in it. We may even say that in hard steel there is no oxide of iron at all, owing to the large quantity of carbon, which reduces the oxide as soon as it forms.

If we investigate how such a steel behaves in the molds immediately after casting, we find that the surface is moved by a slight bubbling, which lasts only a little while, the ingot soon covering itself with a crust or skin, which closes itself entirely. During this bubbling blue flames, resembling the carbonic oxide flame, are emitted; these flames cease when the crust has formed. The fracture of an ingot of this kind shows the blowholes arranged, as indicated by Fig. 30, on a parallel line with the sides of the mold. It is easy to see how these blowholes are formed. When the steel is poured into the mold the metal next to the sides of the mold is cooled, and a layer of nearly rigid metal is formed. No blowholes can be formed in this layer, as the metal inside is kept in agitation by escaping gas, thus filling up any cavities that may form. But when bubbling ceases, *i. e.*, when the top crust has been formed, the reaction between the carbon and the dissolved oxides continues during the cooling period, and carbonic oxide is generated through the whole molten mass. The blowholes, which form in the parts which are yet in a molten state, ascend to the surface, and the gas contained in them either breaks through the crust or passes into the "pipe." On the other hand, the gas that forms in the not-longer fluid part of the steel cannot ascend, and remains in regular lines as shown, Fig. 30.

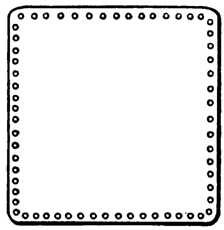


Fig. 30

But why is it, then, we may ask, that, with the exception of this belt of blowholes, the inner part of the ingot is quite sound and solid? This can be explained in two ways:

1st. The metal remains fluid long enough to enable the carbon to reduce all the oxides of iron compounds, and to escape as carbonic oxide.

2d. The evolution of CO causes sufficient internal pressure to suppress continued evolution of gas, which therefore remains dissolved in the metal.*

The explanation No. 2 seems to us the most probable, because it has been shown that even a moderate pressure can considerably diminish the evolution of gas in a certain metal.†

When the amount of carbon is small, and consequently the amount of dissolved oxygen is larger,‡ the period of bubbling

* There ought to be an analogy between the evolution of gas in fluid steel and that in a soda-water bottle. If we watch the bubbling in a glass of soda-water, we find that 90% of the bubbles rise from the circumference

† We have cast several ingots under 5 to 6 atm. pressure; these ingots had always less blowholes than ordinary ingots.

‡ Possibly the actual amount of dissolved oxygen is not larger in this case, but the amount of carbon present being small, the reaction between the two elements takes place more slowly; the period of bubbling is therefore longer. Sometimes, however, a larger quantity of dissolved oxygen has really been found in the soft steels than in the hard ones.

immediately after casting lasts longer. In this case the belt of blowholes may be found farther in than shown in Fig. 30, *vide* Fig. 31. In every low-carbon steel,

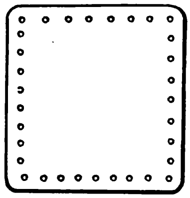


Fig. 31

such as boiler-plate, etc., the bubbles may be found still farther in, Fig. 32.

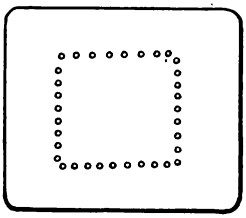


Fig. 32

The explanation is always the same, viz., that blowholes cannot continue to exist as long as the metal is in motion.

These experiences have been chiefly derived from the steels obtained by the basic Bessemer process, in which there is no silicon to counteract the formation of blowholes, but the above is equally applicable to certain Bessemer steels that have been obtained by complete decarburization and recarburization, and more especially to Siemens-Martin steel, which has comparatively little silicon.

At the beginning of this chapter we mentioned that the chemical composition of the steel had a great influence on the formation of blowholes. The presence of silicon prevents their formation very effectually.

Manganese alone cannot prevent blowholes, as shown by experience.

Silicon and manganese combined remove the blowholes almost completely, an easily fusible silicate of MnO or of FeO+MnO being formed, which slag easily separates from the molten metal.

By the addition of a very silicious pig, that metal free from blowholes is obtained, which has caused such frequent discussion during the last years. Bessemer already mentioned this fact at the introduction of his process.

Everybody has seen or heard of the famous solid castings from Bochum and Terrenoire, etc. All these objects have been obtained by making use of the powerful oxygen-absorbing properties of silicon, when mixed with a bath of molten steel, containing oxidized iron.

Bessemer and Siemens-Martin steel behave somewhat differently according to the amounts of silicon and manganese in the pig iron used. But notice must also be taken of the temperature of casting the metal.

Let us suppose that the temperature of the steel, without being *too* high, is sufficiently high to prevent the formation of any scrap in the ladle. The temperature may then be called normal, and every steel-maker soon learns to know the same. If a pig iron, with a moderate percentage of manganese (2 to 3%) be treated under these conditions, and the oxidation not driven too far before carburizing with spiegel or ferromanganese, a product is obtained, which may be cast exactly like pig iron, and therefore gives ingots free from blowholes.

The metal need not be very rich in carbon. As soon as the carbon exceeds .3 every casting will succeed, if the temperature be the right one. With less carbon the difficulties become greater, and special precautions are required to prevent oxidation.

As regards silicon, the unfavorable influences of the same during rolling and forging is well known; it may be lowered to .1 to .15. Between these limits its presence is rather of use than otherwise, particularly if the steel contain sulphur.

If the temperature at the casting of the steel into molds be too low, as well as if the heat developed during the operation be insufficient,* the metal contains a certain quantity of iron oxides, which react on the carbon during cooling, and form CO.

Unless the temperature be *entirely* too low, the metal in contact with the mold solidifies through a thick layer, but remains fluid internally. A belt of large

* Iron seems to oxidize easier at lower than at higher temperatures; for this reason, soc. "cold" metal contains little silicon and manganese, owing to the presence of oxide of iron.

blowholes is formed as shown, Fig. 33, nearly extending to the edge. Such metal generally stands quietly in the molds, covering itself with a symmetrical skin.

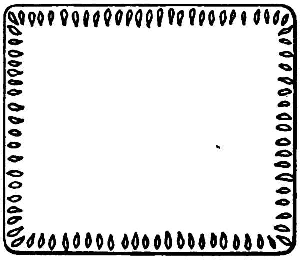


Fig.33

If the oxidation for obtaining a soft metal has been driven very far, the ingot has the fracture shown in Fig. 34. The metal behaves like soft metal free from silicon.

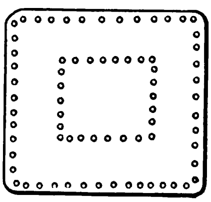


Fig.34

If the metal be cast at a very low temperature, it becomes "pasty" throughout when poured into the mold. The result is a spongy metal—Fig. 35. If the metal at the same time be very soft, it rises violently in the molds and recedes alternately; finally it sinks back, giving ingots of which only the lower parts can be used, Fig. 36. Sometimes the upper part of such an ingot is closed, so that the whole, externally, resembles a solid ingot.

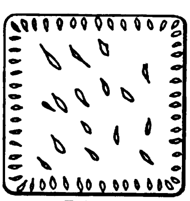


Fig.35

It should be remembered that the normal tapping temperature is higher for soft metal than for hard metal, the melting point being higher in the soft metal.

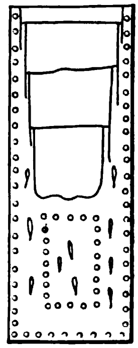


Fig.36

In the Bessemer process it sometimes happens that the temperature rises too much. Metal thus obtained must be altogether rejected, because, besides many physical faults, such metal generally is extra rich in silicon, probably owing to a continued reduction of silicon from the walls of the converter by carbon or iron. It would seem as if such silicious metal ought to be free from blowholes; and yet, if it be cast at once, when ready, it gives ingots at least as honeycombed as ingots made of cold hard steel. But the nature of the blowholes is entirely different. The fracture of such ingots shows the edges perforated by innumerable small, narrow, very elongated, and closely-packed blowholes, Fig. 37.

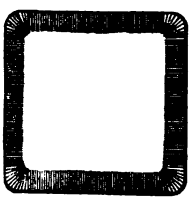


Fig.37

Such ingots very seldom behave well during rolling and forging, the product almost always becoming cracky and full of unsoundness, due to compressed blowholes.

Now it would seem from this as if the theory of silicon as a remedy for blowholes would not hold good in this case; such is, however, not the case. That special kind of blowholes indicated by Fig. 37, has nothing to do with the chemical composition of the metal. If we polish the surface of an ingot that has been

cast too hot, we will find that ends of the blowholes in question run right out to the circumference. Ingots of this kind are also difficult to get out of the molds, which shows that they stick to the sides of the molds during a large part of the time required for cooling.

The cause of the special kind of blowholes, shown Fig. 37, is an external one, so to speak. The molds are always more or less oxidized; the reaction between the oxides of the molds and the molten metal causes evolution of CO, which, being unable to pass up between the mold and the ingot and the fluid metal, forces its way into the latter, forming blowholes as shown Fig. 37.

These blowholes have given so much annoyance that we have been induced to make some experiments in order to throw some more light on their origin. We polished a cast-iron mold until it presented a perfectly metallic surface. We then cast into this polished mold superheated Bessemer metal, at the same time casting such metal into an ordinary mold, for comparison. The ingot obtained in the polished mold was perfectly sound, whilst the ingot obtained in the ordinary mold had the appearance of overheated metal, just described.

Siemens-Martin steel is less liable to become overheated than Bessemer metal.

When the temperature at the beginning of a tap is very high, the first ingots may be defective, as per Fig. 37, whilst the last ingots may be sound, owing to gradual cooling of the metal.

III. Precautions to be Observed to Avoid Defects Resulting from the Heating of the Steel.

When a cold ingot is placed in a very hot furnace one may often hear sharp reports, indicating internal ruptures in the steel.

This is due to too rapid heating. Hard steels and steels rich in silicon are particularly sensitive to such heating.

When an ingot is placed in a too hot furnace, the exterior expands before the interior is sufficiently warm to follow. If the steel has only small power of elongation, the stretching forces may therefore rupture the interior. If such an ingot be fractured, a circular rupture will generally be found, Fig. 38. When such an ingot is rolled out this rupture will be transformed into a longitudinal hole, Fig. 39, which is not always visible from the outside, thus involving considerable danger.

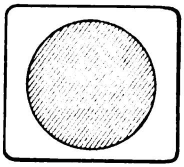


Fig.38

At the time when the manufacture of large objects of forged steel was in its infancy, one used to ridicule the great secrecy with which Krupp surrounded his methods of heating large ingots; but we are now compelled to admit that Krupp's precautions were the result of a long experience.

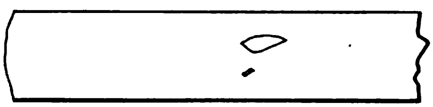


Fig.39

If we wish to protect ourselves against this kind of defect in steel, we must thus observe the following precautions:

1. Never to let an ingot cool completely before putting it into the hot furnace.

2. If it be necessary to use cold ingots, to heat them very gradually. Such large ingots as are intended for guns, shafts, etc., are best placed in a cold furnace, which is then slowly heated up. Small ingots are best heated in a long furnace, in which the ingots can be gradually pushed up towards the hottest part.

A paper was recently read before the Paris Academy of Sciences on the electric conductibility of solid mercury and of pure metals at low temperatures, by MM. Cailletet and Bouty. From numerous experiments made with mercury, silver, tin, aluminium, magnesium, copper, iron, and platina, the authors conclude that the electric resistance of most pure metals decreases regularly when the temperature is lowered from 0 deg. to −123 deg., and that the coefficient of variation is apparently much the same for all. It seems probable that the resistance would become extremely slight at temperatures lower than −200 deg., although this point has not yet been practically tested.

PRINCIPLES OF FORECASTING BY MEANS OF WEATHER CHARTS.

By the Hon. RALPH ABERCROMBY, F.R. Met. Soc.

From the "Nautical Magazine."

The Meteorological Council has issued a work on "Weather Forecasting," by the Hon. Ralph Abercromby. It gives an insight into the secret working of the forecaster, and contains a fund of information for those who aim to be weather wise. The modern method of forecasting, by combining on a chart the weather observations made at the same instant of time at a large number of places, has only been introduced within the last twenty-five years. Prior to this the forecaster was limited to isolated observations of the barometer, and to such popular weather signs as the appearance of the sky or the movements of animals.

The forecast now issued by the Meteorological Office are based upon the telegraphic reports from upwards of fifty stations, which represent not only the entire area of the British Islands, but extend along the entire western coast of the continent of Europe, from Christiansund, in latitude 63° N., to Corunna in latitude 43° N., and include four stations on the coast of the Baltic, and one in the Mediterranean. The height of the barometer and thermometer, with the direction and force of the wind, together with the actual state of the weather conveyed in these telegrams, are charted immediately on their receipt, and from an examination of a series of these charts the necessary deductions and conclusions are drawn.

The work gives instructions in the numerous intricacies involved in the art of manufacturing weather forecasts. It is not, however, possible with our limited space to touch on all the various problems placed before us, but it will be useful to indicate the outline of the information conveyed, keeping as far as possible to the author's words.

CYCLONES.

The technical terms referring to cyclones are first explained:—

The center of a cyclone is the geometrical center of the innermost isobar, or line of lowest barometer.

The diameter of a cyclone is a line drawn through the center as far outwards as the curvature of the isobars is distinctly related to the center, and is often very ill-defined, since the influence of the cyclone extends much further in some directions than in others. A cyclone may be of any diameter, from 100 to 3,000 miles. The commonest are between 1,000 and 2,000 miles; and anything less than 1,200 miles across may be called a small one. The diameter is the measure of the size of a cyclone.

The axis of the cyclone is an imaginary line round which the whole air in a cyclone may be supposed to circulate. There is reason to believe that it is not usually vertical to the earth's surface, and that both the inclination and direction of the axis is variable.

The intensity of a cyclone is measured by the maximum steepness of the barometrical gradient in any portion of it. If this exceed 0.02 inch per15 nautical miles, then the cyclone may be said to be of considerable intensity.

By the expression the level of a "cyclone" is meant the barometrical reading at the lowest point. If the lowest point in the cyclone is above 29.9 inches, we may call it a high-level cyclone; if below that, a low-level one.

The path of a cyclone is the path described by the center. In this country 90 per cent. move towards some point of east, the most frequent direction being about east north-east.

The velocity of a cyclone is the velocity of the center. It may be anything from 80 miles an hour eastward to 10 miles an hour westward. About twenty miles is a common velocity, but sometimes a cyclone is stationary.

The trough is a line drawn through the center of the cyclone, generally at right angles to its path, which marks out the position of all the places where the barometer has attained its lowest point. Everywhere the mercury is falling in front and rising in rear of the line, in

consequence of the forward motion of the cyclone. This line defines the front and rear of a cyclone.

The right and left sides of a cyclone are the right and left sides to an observer looking in the direction in which the cyclone advances; they are thus relative to the direction of the cyclone path.

The life of a cyclone is measured by the number of days during which it can be traced on synoptic charts. The length of the life may be anything from a few hours to about twenty days. Any cyclone whose life is less than 24 hours may be called short-lived.

With respect to the details of wind, weather, temperature, &c., in different portions of a cyclone:—

The temperature is always higher in front than in rear; the warm air in front having a peculiarly close muggy character, quite independent of the actual height of the thermometer. The cold air in the rear, on the contrary, has a peculiarly exhilarating feeling, also quite independent of the thermometer.

The front is always very damp, especially the right front, while the rear is dry to a marked degree.

The wind blows round the center in a direction contrary to the motion of the hands of a watch, which is lying horizontally with its face upwards; but as the direction is slightly inclined to the isobars, on the whole the circulation is an in-going spiral. The amount of incurvature is usually greatest in the right front and least in the rear of the cyclone, so that sometimes the passage of the trough is marked by a sudden shift of wind.

The grouping of the weather into types is somewhat new, and has been undertaken by Mr. Abercromby expressly for this publication.

WEATHER TYPES.

The general idea of weather types will be readily understood by considering the fact familiar to all observers, that the weather in this country frequently occurs in spells of several weeks' duration, during which there is a remarkable persistence of the general type of weather overriding both a considerable fluctuation from day to day, and a considerable local variation from place to place.

For instance, the wind will often back to some point of south, with a high temperature, a dull sky and rain, and then veer to some point of west, with a cooler air and brighter sky; and after a day or so of fine weather it will back again to the south with bad weather, perhaps this time rising to the intensity of a gale, and subsequently veer towards the west with finer weather, and so on for weeks together.

The changes only vary in intensity and detail, not in general character, while the feel of the weather and the look of the sky remain through all of them characteristic of westerly winds.

Similarly, the wind will often come persistently frome some point of east, fluctuating between south-east for fouler weather, and north-east for finer weather, and back again with many variations for several weeks, during which the predominant features of the weather are always characteristic of east winds. The frequent recurrence of particular types of weather at particular seasons of the year is also a matter of common observation; the north-east winds of March, the cold north winds in the middle of June, and the wet west winds of September are well-known instances.

If we examine a large number of synoptic charts, we soon see that relatively to Europe, the general position of the great areas of high pressure frequently remained constant for a lengthened period. Further examination shows that the constancy of these positions coincides with persistency of types of weather similar to those above mentioned, the fluctuation of weather in each type being due to the passage of cyclones, while the local variation depends on the exact position of the cyclone center, and on the innumerable local conditions which modify any general type.

The types may be considered as applying generally to Europe, while the details of weather refer only to Great Britain.

The distribution of atmospheric pressure for the Northern Hemisphere presents certain constant features; namely:

1. An equatorial belt of nearly uniform low pressure.

2. A Tropical belt of high pressure rising at intervals into great irregular elevations or anticyclones.

3. A temperate and Arctic region of generally lower pressure, but in which oc-

casionally areas of high pressure appear for a considerable period.

The equatorial belt constantly covers the Sahara and the Amazon Valley, and always narrows over the Atlantic at about 30° west longitude, where it often does not reach higher than 10° north latitude. The shape and depth of this area are tolerably constant.

The Tropical belt comprises a region of high pressure, rising at variable intervals into great anticyclones; these anticyclones are usually longest in an east and west direction, and often rise into two or more heads. Their position is generally variable with the exception of one, which is always found over the central Atlantic. This anticyclone forms a very important factor of the weather of Western Europe, and will be constantly referred to as "the Atlantic Anticyclone."

The extension south and west of this anticyclone is tolerably constant, while north and east it is variable, sometimes rising as far as 60° north and stretching over Great Britain and Continental Europe.

Cyclones are rarely, if ever, formed to the south of this anticyclone; sometimes they have their origin on its south-west side, when they work round the anticyclone first towards the north-west, and then towards north-east. These are the West India Hurricanes.

The north side of the anticyclone is the birthplace of innumerable cyclones of every size and intensity, which invariably move towards some point of east, and then play a most important part in the meteorology of Great Britain.

Cyclones are also occasionally formed on the south-east side near Madeira; these either work very slowly round the high pressure to the south-west, or else leave the anticyclone and go east.

The Temperate and Arctic region extends from the tropical high pressure to the pole. Though ordinarily low, the pressure in this region is perpetually fluctuating by reason of the incessant passage of cyclones; yet occasionally persistent areas of high pressure appear in certain portions of it.

With reference to Western Europe, there are at least four well-marked types of weather:

1. The Southerly, in which an anticyclone lies to the east or south-east of Great Britain, while cyclones coming in from the Atlantic either beat up against it or pass towards north-east.

2. The Westerly, in which the tropical belt of anticyclones is found to the south of Great Britain, and the cyclones which are formed in the central Atlantic, pass towards east or north-east.

3. The Northerly, in which the Atlantic anticyclone stretches far to the west and north-west of Great Britain, roughly covering the Atlantic Ocean. In this case cyclones spring up on the north or east side, and either work round the anticyclone to the south-east, or leave it and travel rapidly towards the east.

4. The Easterly, in which an apparently non-tropical anticyclone (or one disconnected from the tropical high-pressure belt) appears in the north-east of Europe, rarely extending beyond the coast line, while the Atlantic anticyclone is occasionally totally absent from the Bay of Biscay. The cyclones then either come in from the Atlantic and pass south-east between the Scandinavian and Atlantic anticyclones; or else their progress being impeded, they are arrested or deflected by the anticyclone in the north-east of Europe.

Sometimes they are formed to the south of the north-east anticyclone, and advance slowly towards the east, or in very rare instances towards the west.

SOUTHERLY TYPE.

In this the Atlantic anticyclone extends very little to the northward, while a large area of high pressure covers Europe to the east and south-east of the United Kingdom.

The North Atlantic is occupied by a persistent area of low pressure in which cyclones are constantly being formed; these beat up against the high European pressure, and either die out or are repelled.

Sometimes, especially in summer, small cyclones, arising on the easterly side of the area of depression, pass rapidly near the British coasts in a north or north-east direction. In either case it is somewhat rare for the center of a cyclone to reach over these islands, so that generally Great Britain is under the influence of the rim or edge of either a cyclone or anticyclone.

At other times the Atlantic low press-

ure extends over Great Britain, driving the high pressure eastwards, without forming any definite cyclone. In this case the indications are for tolerably fine weather and little wind, with a very low barometer: a condition which often excites remark.

In winter the cyclones are usually large, but in summer the general depression of the Atlantic is much less pronounced, while the cyclones are smaller, their centers progress further eastward, and the gradients are less steep. The weather in them is not so dirty, though the air is always close, and the sky is harder than in winter.

This type of weather occurs at all seasons of the year, but is most common and persistent in winter; in fact, the warmth or otherwise of the winter principally depends on the number of days of this type.

The temperature of this type is always high, partly because of the prevailing southerly winds, and when the cyclones die out, the slight degree of cold which follows is very noticeable. Sometimes a portion of the Russian anticyclone reaches Great Britain, and in winter gives rise to white frost of short duration.

The wind in this type is remarkable for its steadiness and absence of gustiness, except when the intensity is extreme; and, for various reasons, the gales of wind do comparatively little destruction either on sea or land, considering their force and duration.

To a single observer the sequence of weather in this type is very simple. As atmospheric pressure falls, temperature rises, and the sky grows dirtier, till drizzling rain sets in. The wind from some point of south, having backed slightly, rises in velocity till the barometer has reached its lowest point; as soon as pressure begins to increase, the wind veers a little towards south-west and gradually falls, the air becomes cooler, and the sky begins to clear; but it rarely becomes hard, or contains firm cumulus. By next day, perhaps, the same sequence is repeated, varying only in intensity, but not in general character, and this alternation often lasts for weeks at a time.

WESTERLY TYPE.

In this type the tropical belt of anticyclones is constantly to the south of Great Britain, and the pressure to the east, west, and especially the north, comparatively low. Under these circumstances, cyclones are developed on the north side of the Atlantic anticyclone, which roll quickly eastwards along the high-pressure belt, usually dying out after they have been detached from the Atlantic anticyclone in their eastward course. Their intensity, and consequently the weather they produce, may vary almost indefinitely.

When the cyclones are formed very far south, so that their centers cross Great Britain, and are of moderate size, the intensity is usually great, and severe well-defined storms with sharp shifts of wind are experienced. These occur most frequently in spring and autumn, and are the most destructive storms which occur in Great Britain.

Another intense form is found in a series of small, short-lived, quick-moving cyclones, and numerous secondaries, without very steep gradients, during which the indications are for rainy broken weather, rather than for storms. This phase is common at all seasons of the year.

In another modification, while the pressure is low to the north, and the isobars run nearly due east and west, the whole of the Arctic area of low pressure surges southward with an exceedingly ill-defined cyclone, bringing a rim of steep gradients along the edge of the Atlantic anticyclone, and across Great Britain, in a manner analogous to a phase of southerly type, before explained. The indications then are for rain, and westerly gales with very little shift of wind. This phase belongs almost exclusively to the winter months.

But the commonest modification at every season, and that which forms about 70 per cent. of our weather, is when the intensity is moderate, and the cyclone paths are so far to the north of the British Islands, that the wind merely backs a point or two from south-west as the cyclone approaches, and veers a point or two towards the west as the cyclone passes, the general direction of the wind being between south-west and west, without rising to the strength of a gale, while rain is moderate in quantity.

To a single observer the principal peculiarity of this type lies in the variable wind and weather. In the southerly type an observer only gets, as it were, the

front of a series of cyclones, while in this type he gets the characteristic weather of both front and rear. In the former condition the worst weather is always in front; while in this the worst often occurs in rear, after the pressure has begun to increase.

NORTHERLY TYPE.

In this type the pressure is high to the west and north-west of Great Britain, while it is low over Germany and Scandinavia, thus giving gradients for northerly winds, from whence the type is named.

Sometimes this high pressure seems to be simply a great extension northwards of the Atlantic anticyclones, on the east side of which cyclones form and pass towards some point of east, thus approximating to a phase of westerly type into which it may merge insensibly. This form is most common in July and August.

More frequently a moderate extension northwards of the Atlantic anticyclones is met by a persistent anticyclone lying over Greenland. On one side any cyclones coming from the American continent are arrested by the belt of high pressure thus formed. On the other side, a large area of low pressure lies over northern and central Europe, which is the theater of the formation of an incessant series of cyclones.

The centers of these cyclones always lie to the east of Great Britain, but modify our weather by their approach or recession. Thus the northerly type, during which Great Britain is constantly under the influence of the rear of cyclones, may be considered the exact converse of the southerly type, during which it is as constantly under the influence of the front of cyclones. This form of the type is most common in the winter, and especially the spring months, notably in March; while it is very rare during the autumn.

To a single observer the most marked feature is the constancy of the wind in the north, generally veering towards north-east with increasing cloud as the barometer falls, and backing towards north-west with brighter weather as it rises; but there is no approach to such a regular shift as occurs in the westerly type. The appearance of the sky is usually hard, and any rainfall takes the form of showers or squalls, rather than of a drizzle or a steady downpour.

EASTERLY TYPE.

In the commonest form of this type, the Atlantic anticyclone is very small and lies far south, while another anticyclone lies persistently over Scandinavia. On the south side of the Scandinavian anticyclone, a cyclone is usually formed over Central and Southern Europe, which either remains stationary, or else moves very slowly towards the west. On the west and south-west side of the anticyclone, cyclones formed in mid-Atlantic press up against it, and either die out, or are rebuffed.

In a less common form, the edge of the Atlantic anticyclone stretches as far north as Portugal, and cyclones coming in from the Atlantic pass across Great Britain in a south-east direction, through the cold which lies between the Atlantic and Scandinavian regions of high pressure.

This type is much more common than the northerly, and occurs at all seasons of the year. It is most common in October, November, February and May, but very rare in July, August, or September.

Though much less common than the westerly or southerly types, it is so stormy that nearly one-half of the wrecks on the British shores are due to gales of this type. This is, however, partly due to the large number of unseaworthy colliers which trade along our east coasts.

To a single observer the general sequence of weather is, that as the barometer falls the sky gets blacker, the air warmer, and the wind veers towards the south-east; then as the barometer rises the air gets colder and the wind backs towards east or north-east, while the strength of the wind and amount of rain depend on the intensity.

In most cases the shift of wind is small, but we may note that the wind veers for an oncoming cyclone, instead of backing, as in the westerly type. This is because the cyclone centers are always towards some point south of the observer.

This type sometimes persists for two or three weeks on end, but no definite symptons of a change of type can be

given. It often succeeds the northern type, and sometimes the two types alternate with one another. At other times the Scandinavian anticyclone lies so far to the south that this type merges insensibly into the southerly.

CYCLONE PATHS.

As the motion of cyclone centers is always towards some point of east, except in certain cases of the easterly type, as already explained, in an uncertain case we may always assume some eastward drift.

Another very noticeable point about cyclones is the tendency of their centers to follow a coast line rather than to strike inland. Also when they do cross the land, they seem to take by preference the lines of valleys, or at all events, of lowest ground, which seem to be lines of least resistance to cyclone motion. In this country cyclone centers have a tendency to pass up channel, or round the northwest coasts of Ireland and Scotland; and when they cross the country they frequently select the line of the Caledonian Canal, or the line of the Forth and Clyde Canals.

GENERAL REMARKS.

Under this head are given a few reflections on forecasting generally.

In the first place, it is remarked that the meteorological changes are so rapid that they can only be traced by means of electric telegraph, and that the expense is therefore so great that this problem can only be efficiently handled by a Central Government Office.

Then as to the amount of detail which is possible, it is manifest that in many cases of small disturbances the minor features, such as squalls, thunderstorms, &c., are so local, that even if it were possible to define their range accurately, every few square miles of the United Kingdom would require a separate forecast, so that it is only the general character of the weather which can ever be forecast, and that, roughly, the larger the scale of disturbance, the more likely are the forecasts to be successful. These minor features are also usually short lived, so that the more frequently the observations are taken, the more unlikely is a storm to escape detection. In summer time a small storm may sometimes form or die out between the 8 A.M. and 6 P.M. reports.

Then, as to the length of time ahead for which forecasts can be issued. It is obvious that owing to the rapid nature of all meteorological changes forecasts can never be issued very long in advance.

It will also be very apparent that the British forecaster labors under peculiar difficulties from his geographical position. Situated on the most outlying portion of Europe, and in the very track of storms which almost always advance from the westward, he has no intimation of an approaching cyclone, till it is actually on him. In the United States and in Germany they are more fortunate. For instance Hamburg often receives timely warning from our English office. In the year 1869, 23 storms were felt in Hamburg, and of these 22 had previously passed over some part of the United Kingdom.

Owing to the nature of weather changes, the average or mean value of any meteorological quantity affords no clue towards estimating the probable change in any existing system. In fact meteorology is not an exact, but an observational science like geology or medicine; and just as however accurately the symptoms or treatment of any malady may be described, the skill to recognize and the judgment to treat must rest on the ability of the physician, so in meteorology, however carefully the relation of weather to isobars may be defined, and the nature of their changes described, the judgment, which experience alone can give, to enable a warning to be issued, must ever depend on the professional skill of the forecaster.

THE mean daily motion of the air in 1884, as given in the report of the Astronomer Royal, was 286 miles, being three miles greater than the average of the last seventeen years. The greatest daily motion was 891 miles on January 23d, and the least, 78 miles on February 8th. The only recorded pressure exceeding 20 lbs. on the square foot in 1884 was 22.7 lbs. on January 23d, after which the connecting chain of the pressure plate broke. It is probable that greater pressures occurred afterwards on the same day, and also in the gale of January 26th, at which date the chain had not been renewed.

THE ELEMENTARY PRINCIPLES OF THE GAS ENGINE.

By DENNY LANE.

From the "English Mechanic."

Before the invention of the steam engine, the only powers employed in mechanics were those of wind and water mills and animal power. In the first two no conversion of one force into another took place; they were mere kinematic devices for employing the mechanical force already existing in the gale of wind and the head of water. With regard to the power developed by man and other animals, we had in them examples of most efficient heat engines, converting into power a large percentage of the fuel burnt in the lungs. But animal power is small in amount, and it is expensive for two reasons—first, because the agents require long intervals of rest, during which they still burn fuel; the next, because the fuel they require is very expensive. A pound of bread, or beef, or oats or beans, costs a great deal more than a pound of coal; while it does not, by its combustion, generate nearly so much heat. The steam engine, therefore, took the place of animal power, and for a long time stood alone; and nearly all the motive power derived from heat is still produced by the mechanism which Watt brought to such great efficiency in so short a time. Now, the practical question for all designers and employers of heat engines is, to determine how the greatest quantity of motive force can be developed from the heat evolved from a given kind of fuel; and coal being the cheapest of all, we will see what are the results obtainable from it by the steam engine. In this we have three efficiencies to consider—those of the furnace, the boiler, and the cylinder. First, with respect to the furnace, the object is to combine the carbon and the hydrogen of the coal with a sufficient quantity of the oxygen of the air to effect complete combustion into carbonic acid and water. In order to do this, we have to use a quantity of air much larger than is theoretically necessary, and also to heat an amount of inert nitrogen five times greater than the necessary oxygen; and we are therefore obliged to create a draught which carries away to the chimney a considerable portion of the heat developed. The combustion, moreover, is never perfect, and some heat is lost by conduction and radiation. The principal loss is by hot gases escaping from the flues to the chimney. Even with well-set boilers, the temperature in the chimney varies from 400° to 600°. Taking the mean of 500°, this would represent a large proportion of the total heat, even if the combustion were perfect; for, as a general rule, the supply of air to a furnace is double that which is theoretically necessary. For our present purpose it will be sufficient to see how much the whole loss is, without dividing it under the several heads of "imperfect combustion," "radiation," and "convection," by the heated gases passing to the chimney.

With a very good boiler and furnace, each pound of coal evaporates 10 lbs. of water from 62°, changing it into steam of 65 lbs. pressure at a temperature of 312°, or 250° above that of the water from which it is generated. Besides these 250°, each pound of steam contains 894 units of latent heat, or 1,144 units in all. A very good condensing engine will work with 2.2 lbs. of coal and 22 lbs. of steam per horse per hour. Now, 1 lb. of good coal will, by its combustion, produce 14,000 heat units, and the 2.2 lbs. of coal multiplied by 14,000 represent 30,800 heat units. Of these we find in the boiler $22 \times 1,144$, or 25,168 units, or about 81½ per cent. of the whole heat of combustion; so that the difference (5,632 units, or 18½ per cent.) has been lost by imperfect combustion, radiation, or convection. The water required for condensing this quantity of steam is 550 lbs.; and taking the temperature in the hot well as 102°, 550 lbs. have been raised 40° from 62°. Thus we account for $550 \times 40 = 22,000$, or (say) 71½ per cent. still remaining as heat. If we add this 71½ per cent. to 18½, we have 90 per cent., and there remain only 10 per cent. of the heat that

can possibly have been converted into power. But some of this has been lost by radiation from steam pipes, cylinder, &c. Allowing but 1 per cent. for this, we have only 9 per cent. as the efficiency of a really good condensing engine. This estimate agrees very closely with the actual result; for the 2.2 lbs. of coal would develop 30,800 heat units; and this multiplied by Joule's equivalent amounts to nearly 24 millions of foot-pounds. As 1 horse-power is a little less than 2 million foot-pounds per hour, only one-twelfth, or a little more than 8 per cent., of the total heat is converted; so that, whether we look at the total quantity of heat which we show unconverted, or the total heat converted, we find that each supplements and corroborates the other. If we take the efficiency of the engine alone, without considering the loss caused by the boiler, we find that the 25,168 heat units which entered the boiler should have given 19,429,696 foot-pounds; so that the 2 millions given by the engine represent about 10 per cent. of the heat which has left the boiler. The foregoing figures refer to large stationary or marine engines, with first-rate boilers. When, however, we come to high-pressure engines of the best type, the consumption of coal is twice as much; and for those of any ordinary type it is usual to calculate 1 cubic foot, or 62½ lbs. of water evaporated per horse-power. This would reduce the efficiency to about 6 per cent. for the best and 3 per cent. for ordinary non-condensing engines; and if to this we add the inefficiency of some boilers, it is certain that many small engines do not convert into power more than 2 per cent. of the potential energy contained in the coal.

Before explaining the principle upon which the gas engine and every other hot air engine depends, I shall remind you of a few data with which most of you are already familiar. The volume of every gas increases with the temperature; and this increase was the basis of the air thermometer—the first ever used. It is to be regretted that it was not the foundation of all others, for it is based on a physical principle universally applicable. Although the volume increases with the temperature, it does not increase in proportion to the degrees of any ordinary scale, but much more slowly. Now, if to each of the terms of an arithmetical series we add the same number, the new series so formed increases or decreases more slowly than the original; and it was discovered that by adding 461 to the degrees of Fahrenheit's scale, the new scale so formed represented exactly the increment of volume caused by increase of temperature. This scale, proposed by Sir W. Thomson in 1848, is called the "scale of absolute temperature;" its zero, called the "absolute zero," is 461° below the zero of Fahrenheit, or 493° below the freezing point of water; and the degree of heat measured by it is termed the "absolute temperature" It is often convenient to refer to 39° Fahr. (which happens to be the point at which water attains its maximum density), as this is the same as 500° absolute, for, counting from this datum level, a volume of air expands exactly 1 per cent. for 5°, and would be doubled at 1000° absolute, or 439° Fahr.

Whenever any body is compressed, its specific heat is diminished, and the surplus portion is, as it were, pushed out of the body, appearing as sensible heat; and whenever any body is expanded its specific heat is increased, and the additional quantity of heat requisite is, as it were, sucked in from surrounding bodies, so producing cold. This action may be compared to that of a wet sponge from which, when compressed, a portion of the water is forced out, and when the sponge is allowed to expand the water is drawn back. This effect is manifested by the increase of temperature in air-compressing machines, and the cold produced by allowing or forcing air to expand in air-cooling machines. At 39° Fahr. 1 lb. of air measures 12½ cubic feet. Let us suppose that 1 lb. of air at 39° Fahr. = 500° absolute, is contained in a non-conducting cylinder of 1 ft. area and 12½ ft. deep under a counter-poised piston. The pressure of the atmosphere on the piston = 144 square inches × 14.7 lbs., or 2,116 lbs. If the air be now heated up to 539° Fahr. = 1000° absolute, and at the same time the piston is not allowed to move, the pressure is doubled; and when the piston is released it would rise 12½ ft. provided that the temperature remained constant, and the indicator would describe a hyperbolic curve, called an "isothermal," because the temperature would

have remained equal throughout. But, in fact, the temperature is lowered, because expansion has taken place, and the indicator curve which would then be described is called an "adiabatic curve," which is more inclined to the horizontal lines when the volumes are represented by horizontal and the pressures by vertical co-ordinates. In this case it is supposed that there is no conduction or transmission (diabasis) of heat through the sides of the containing vessel. If, however, an *additional* quantity of heat be communicated to the air, so as to maintain the temperature at 1000° absolute, the piston will rise until it is $12\frac{1}{2}$ ft. above its original position, and the indicator will describe an isothermal curve. Now mark the difference. When the piston was fixed, only a heating effect resulted; but when the piston moved up $12\frac{1}{2}$ ft., not only a heating, but a mechanical—in fact, a thermo-dynamo—effect was produced, for the weight of the atmosphere (2,116 lbs.) was lifted $12\frac{1}{2}$ ft. $=26{,}450$ foot-pounds.

The specific heat of air at constant pressure has been proved by the experiments of Regnault to be 0.2378, or something less than one-fourth of that of water—a result arrived at by Rankine from totally different data. In the case we have taken there have been expended 500×0.2378, or (say) 118.9 heat units to produce 26,450 foot-pounds. Each unit has, therefore, produced $\frac{26{,}450}{118.9} = 222\ 5$ foot-pounds, instead of 772 foot-pounds, which would have been rendered if every unit had been converted into power. We therefore conclude that $\frac{222.5}{772} = 29$ per cent. of the total heat has been converted. The residue, or 71 per cent., remains unchanged as heat, and may be partly saved by a regenerator, or applied to other purposes for which a moderate heat is required.

The quantity of heat necessary to raise the heat of air at a constant volume is only 71 per cent. of that required to raise to the same temperature the same weight of air under constant pressure. This is exactly the result at which Laplace arrived from observations on the velocity of sound, and may be stated thus:—

	Specific Heat.		Foot lbs.	Per Cent.
$K_p = 1$ lb. of air at constant pressure ..	0.2378	× 772 =	183.5 =	100
$K_v = 1$ lb. of air at constant volume ...	0.1688	× 772 =	130.3 =	71
Difference being heat converted into power.	0.0690	× 772 =	53.2 =	29

Or, in a hot-air engine without regeneration, the maximum effect of 1 lb. of air heated 1° Fahr. would be 53.2 f. p. The quantity of heat K_v necessary to heat air under constant volume is to K_p, or that necessary to heat it under constant pressure, as 71: 100, or as 1: 1.408, or very nearly as 1: $\sqrt{2}$—a result which was arrived at by Masson, from theoretical considerations. The 71 per cent. escaping as heat may be utilized in place of other fuel, and with the first hot-air engine I ever saw it was employed for drying blocks of wood. In the same way, the unconverted heat of the exhaust steam from a high-pressure engine, or the heated gases and water passing away from a gas engine may be employed.

When I first wrote on this subject I relied upon some data which led me to suppose that the heating power of ordinary coal gas was higher than it really is. At our last meeting, Mr. Hartley proved, by experiments with his calorimeter, that gas of 16 or 17 candles gave only about 630 units of heat per cubic foot. Now, if all this heat could be converted into power, it would yield 630×772, or 486,360 f. p., and it would require only $\left(\frac{1{,}980{,}000}{486{,}360}\right)=4.07$ cubic feet to produce 1 indicated horse-power. Some recent tests have shown that, with gas of similar heating power, 18 cubic feet have given 1 indicated horse-power, and therefore $\frac{4.07}{18}=22.6$ of the whole heat has been converted—a truly wonderful proportion when compared with steam engines of a similar power, showing only an efficiency of 2 to 4 per cent.

The first gas engine which came into practical use was Lenoir's, invented about 1866, in which the mixture of gas and air drawn in for part of the stroke at atmospheric pressure was inflamed by the spark from an induction coil. This required a couple of cells of a strong Bun-

sen battery, was apt to miss fire, and used about 90 cubic feet per horse-power. This was succeeded by Hugon's engine, in which the ignition was caused by a small gas-flame, and the consumption was reduced to 80 cubic feet. In 1864 Otto's atmospheric engine was invented, in which a heavily-loaded piston was forced upwards by an explosion of gas and air drawn in at atmospheric pressure. In its upward stroke the piston was free to move; but in its downward stroke it was connected with a ratchet, and the partial vacuum formed after the explosion beneath the piston, together with its own weight in falling, operated through a rack, and caused rotation of the fly-wheel. This engine (which, in an improved form, uses only 20 cubic feet of gas) is still largely employed, some 1,600 having been constructed. The great objection to it was the noise it produced, and the wear and tear of the ratchet and rack arrangements. In 1876 the Otto-Crossley silent engine was introduced. As you are aware, it is a single-acting engine, in which the gas and air are drawn in by the first outward and compressed by the first inward stroke. The compressed mixture is then ignited, and, being expanded by heat, drives the piston outwards by the second outward stroke. Near the end of this stroke the exhaust-valve is opened, the products of combustion partly escape, and are partly driven out by the second inward stroke. I say partly, for a considerable clearance space, equal to 0.38 per cent. of the whole cylinder volume, remains unexhausted at the inner end of the cylinder. When working to full power, only one stroke out of every four is effective; but this engine works with only 18 to 22 cubic feet of gas per horse-power. Up to the present time I am informed that about 18,000 of these engines have been manufactured. Several other compression engines have been introduced, of which the best known is Mr. Dugald Clerk's, using about 20 ft. of Glasgow cannel gas. It gives one effective stroke for every revolution; the mixture being compressed in a separate air-pump. But this arrangement leads to additional friction; and the power measured by the brake is a smaller percentage of the indicated horse-power than in the Otto-Crossley engine. A number of gas engines—such as Bischop's (much used for very small powers), Robson's (at present undergoing transformation in the able hands of Messrs. Tangye), Korting's, and others—aré in use; but, so far as I can learn, all require a larger quantity of gas than those previously referred to.

I have all along spoken of efficiency as a percentage of the total quantity of heat evolved by the fuel; and this is, in the eyes of a manufacturer, the essential question. Other things being equal, that engine is the most economical which requires the smallest quantity of coal or of gas. But men of science often employ the term efficiency in another sense, which I will explain. If I wind a clock, I have spent a certain amount of energy lifting the weight. This is called "energy of position;" and it is returned, by the fall of the weight, to its original level. In the same way if I heat air or water I communicate to it energy of heat, which remains potential as long as the temperature does not fall, but which can be spent again by a decrease of temperature. In every heat engine, therefore, there must be a fall from a higher to a lower temperature, otherwise no work could be done. If the water in the condenser of a steam engine were as hot as that in the boiler, there would be equal pressure on both sides of the piston, and consequently the engine would remain at rest. Now. the greater the fall, the greater the power developed; for a smaller proportion of the heat remains as heat. If we call the higher temperature T and the lower T′ on the absolute scale, T−T′ is the difference; and the ratio of this to the higher temperature is called the "efficiency." This is the foundation of the formula we meet so often: $E=\frac{T-T'}{T}$ A perfect heat engine would, therefore, be one in which the temperature of the absolute zero would be attained, for $\frac{T-O}{T}=1$ per cent. This low temperature, however, has never been reached; and in all practical cases we are confined within much narrower limits. Taking the case of the condensing engine, the limits were 312° to 102°, or 773° and 563° absolute, respectively. The equation then becomes $\frac{773-563}{773}=\frac{210}{773}$ or (say) 27 per

cent. With non-condensing engines the temperatures may be taken at 312° and 212°, or 773° and 673° absolute, respectively. The equation then becomes $\frac{773-673}{773}=\frac{100}{773°}$ or nearly 13 per cent. The practical efficiencies are not nearly this; but they are in about the same ratio, $\frac{27}{13}$ and if we multiply the theoretical efficiencies by 0.37, we get the practical efficiencies, say 10 per cent. and 5 per cent., and it is in the former sense that M. Witz calculated the efficiency of the steam engine at 35 per cent.—a statement which, I own, puzzled me a little when I first met it. These efficiencies do not take any account of loss of heat before the boiler. In the case of the gas engine the question is much more complicated on account of the large clearance space, and the early opening of the exhaust. The highest temperature has been calculated by the American observers at 3443° absolute, and the observed temperatures of the exhaust gases were 1220°. The fraction then becomes $\frac{3443-1229}{3443}=64$ per cent. If we multiply this by 0.37, as we did in the case of the steam engine, we get 23.7 per cent., or approximately the same as that arrived at by direct experience. Indeed, if the consumption is, as stated sometimes, less than 18 ft., the two percentages would be exactly the same. I do not put this forward as scientifically true; but the coincidence is at least striking.

I have spoken of the illuminating power of the gas as of importance; for the richer gases have also more calorific power, and an engine would, of course, require a smaller quantity of them. The heat-giving power does not, however, vary as the illuminating power, but at a much slower rate; and, adopting the same contrivance as that on which the absolute scale of temperature is formed, I would suggest a formula of the following type: $H=C(I+K)$, in which H represents the number of heat units given out by the combustion of one cubic foot of gas, I is the illuminating power in candles, and C and K two constants to be determined by experiment. If we take the value for motive power of the different qualities of gas as given in Mr. Charles Hunt's interesting paper in our *Transactions* for 1882, C might, without any great error, be taken as 22 and K as 7.5. With Pintsch's oil gas, however, as compared with coal gas, this formula does not hold, and C should be taken much lower and K much higher than the figures given above; that is to say, the heating power increases in a slower progression. The data available, however, are few; but I trust Mr. Hartley will on this, as he has done on so many other scientific subjects, come to our aid.

I will now refer to the valuable experiments of Messrs. Brooks and Steward, which were most carefully made. Everything was measured—the gas by a 60-light and the air by a 300-light meter; the indicated horse-power, by a steam engine indicator; the useful work, by a Prony Brake; the temperature of the water, by a standard thermometer; and that of the escaping gases by a pyrometer. The gas itself was analyzed, and its heating power calculated from its composition as 617.50. Its specific gravity was 0.464, and the volume of air was about seven times that of the gas used (or one-eighth of the mixture), and was only 11½ per cent. by weight more than was needed for perfect combustion. The results arrived at were as follows:—

Converted into indicated horse-power, including friction, &c.	17.0
Escaped with the exhaust gas	15.5
Do. in radiation.	15.5
Communicated to water in the jacket	52.0

It will thus be seen that more than half of the heat is communicated to the water in the jacket. Now, this is the opposite of the steam engine, where the jacket is used to transmit the heat *to* the cylinder, and not *from* it. This cooling is rendered necessary, because without it the oil would be carbonized, and lubrication of the cylinder rendered impossible. Indeed, a similar difficulty has occurred with all hot-air engines, and is, I think, the reason they have not been more generally adopted. I felt this so strongly that, for some time after the introduction of the gas engine, I was very cautious in recommending those who consulted me to adopt it. I was afraid that the wear and tear would be excessive. I have, however, for some time past been thoroughly satisfied that this fear was needless, as I am satisfied that a well-made

gas engine is as durable as a steam engine, and the parts subject to wear can be replaced at a moderate cost. We have no boiler, no feed-pump, no stuffing boxes to attend to—no water gauges, pressure gauges, safety valve, or throttle valve to look after. The governor is of a very simple construction, and the slide valve may be removed and replaced in a few minutes. An occasional cleaning out of the cylinder at considerable intervals is all the supervision that the engine requires.

The very large percentage of heat absorbed by the water-jacket should point out to the ingenuity of inventors the first problem to be attacked—viz., how to save this heat without wasting the lubricant or making it inoperative; and in the solution of this problem I look for the most important improvement to be expected in the engine. The most obvious contrivance would be some sort of intercepting shield, which would save the walls of the cylinder and the rings of the piston from the heat of the ignited gases. I have just learned that something of the kind is under trial. Another solution may possibly be found in the employment of a fluid piston; but here we are placed in a dilemma between the liquids that are decomposed and the metals that are oxidized at high temperatures. Next, the loss by radiation—15 per cent.—seems large; but this is to be attributed to the fact that the inside surface of the cylinder is at each inward stroke exposed to the atmosphere—an infiuence which contributes to the cooling necessary for lubrication. The remaining 15 per cent., which is carried away by the exhaust, is small, compared with the proportion passing away with the exhaust steam of a high pressure, or the water of a condensing engine. As the water in the jacket can be safely raised to 212° Fahr., the whole of the jacket heat can be utilized where hot water is required for other purposes; and this, with the exhaust gases, has been used for drying and heating purposes.

With such advantages, it may be asked: Why does not the gas engine everywhere supersede the steam engine? My answer is a simple one: The gas we manufacture is a dear fuel compared with coal. Ordinary coal gas measures 30 cubic feet to the pound: 1,000 cubic feet, therefore, weighs 33 lbs.; and taking the price at 2s. 9d. per 1,000 cubic feet, it costs 1d. per lb. The 30 cubic feet at 6,300 give 190,000 all available heat. Although good coal may yield 14,000 units by its combustion, only about 11,000 of these reach the boiler; so that the ratio of the useful heat is $\frac{11}{19}$. The thermal efficiency of the best non-condensing engine to that of the gas engine is in the ration $\frac{4}{22}$. Multiplying together these two ratio, we get $\frac{11}{19} \times \frac{4}{22\frac{1}{2}} = \frac{44}{4.28}$. That is, speaking roughly, 1 lb. of gas gives about ten times as much power as 1 lb. of coal does in a good non-condensing engine. But at 18s. 1d. a ton we get 10 lbs. of coal for 1d.; so that with these figures the cheapness of the coal would just compensate for the efficiency of the gas. As to the waste heat passing away from the engine being utilized, here the gas engine has no advantage; and so far as this is concerned, the gas is about eight times dearer than coal. The prices of gas and coal vary so much in different places, that it is hard to determine in what cases gas or coal will be the dearer fuel, considering this point alone.

But there are other kinds of non-illuminating gases—such as Wilson's, Strong's, and Dowson's—which are now coming into use; and at Messrs. Crossley's works you will have an opportunity of seeing a large engineering factory employing several hundred mechanics, and without a chimney, in which every shaft and tool is driven by gas engines supplied by Dowson's gas, and in which the consumption of coal is only 1.2 lb. per horse-power. The greatest economy ever claimed for the steam engine was a consumption of 1.6 lb., and this with steam of very high pressure, expanded in three cylinders successively. Thus in a quarter of a century the gas engine has beaten in the race the steam engine; although from Watt's first idea of improvement nearly a century and a quarter have elapsed.

As regards the steam engine, it is the opinion of competent authorities that the limits of temperature between which it works are so restricted, and so much of the heat is expended in producing a change of state from liquor to vapor, that little further improvement can be made. With

respect to gas engines, the limits of temperature are much further apart. A change of state is not required, and so, very great improvement may still be looked for; and it is not impossible that some of the younger members of our body may live to see that period foretold by one of the greatest of our civil engineers—that the happy time when boiler explosions will be matters of history—that period, not a millennium removed by a thousand years, but an era deferred perhaps by only half a dozen decades, when the use of the gas engine will be universal, and "a steam engine can be found only in a cabinet of antiquities."

ON THE CHANGES PRODUCED BY MAGNETIZATION IN THE LENGTH OF RODS OF IRON, STEEL AND NICKEL.

By SHELFORD BIDWELL, M.A., LL.B.*

From "Nature."

The earliest systematic experiments on the effects produced by magnetization upon the length of iron and steel bars are those of Joule, an account of which is published in the *Phil. Mag.* of 1847. Joule's experiments have many times been repeated, and his general results confirmed. In particular, Prof. A. M. Mayer carried out a series of very careful observations with apparatus of elaborate construction and great delicacy. The conclusions at which he arrived were in accord with those of Joule, so far as regards iron; in the case of steel there was some apparent discrepancy, which, however, might to a great extent, be accounted for by differences in the quality of the metal used and in the manner of conducting the experiments. In 1882 Prof. Barrett published in *Nature* an account of some experiments which he had made, not only on iron but also on bars of nickel and cobalt, with the view of ascertaining the effect of magnetization upon their length.

The knowledge on the subject up to the present time may be summarized as follows:

(1) Magnetization causes in iron bars an elongation, the amount of which varies up to a certain point as the square of the magnetizing force. When the saturation-point is approached the elongation is less than this law would require. The effect is greater in proportion to the softness of the metal.

(2) When a rod or wire of iron is stretched by a weight, the elongating effect of magnetization is diminished; and if the ratio of weight to the section of the wire exceeds a certain limit, magnetization causes retraction instead of elongation.

(3) Soft steel behaves like iron, but the elongation for a given magnetizing force is smaller (Joule). Hard steel is slightly elongated, both when the magnetizing current is made and when it is interrupted, provided that the strength of the successive currents is gradually increased (Joule). The first application of the magnetizing force causes elongation of a steel bar if it is tempered blue, and retraction if it is tempered yellow: subsequent applications of the *same* external magnetizing force cause temporary retraction, whether the temper of the steel is blue or yellow (Mayer).

(4) The length of a nickel bar is diminished by magnetization, the maximum retraction being twice as great as the maximum elongation of iron (Barrett).

In order that the results of Joule and Mayer might be comparable with those obtained by the author, he made an attempt to estimate the magnetizing forces with which they worked. From data contained in their paper, it was calculated that the strongest magnetizing force used by Joule was about 126 units, while the strongest used by Mayer did not on the highest probable estimate exceed 118 units. In the author's experiments the magnetizing force was carried up to about 312 units. The metal rods, too, were much smaller than any which had been before used for the purpose,

* Paper read before the Royal Society.

ranging in diameter from 1.40 to 6.25 mm. Their length was in every case 100 mm., and the apparatus was capable of measuring with tolerable certainty an elongation or retraction equal to a ten-millionth part of this length.

By using thinner iron rods and greater magnetizing forces than those previously employed, the following curious and interesting fact was established. If the magnetization be carried beyond a certain critical point, the consequent elongation, instead of remaining stationary at a maximum, becomes diminished, the diminution increasing with the magnetizing force. If the force is sufficiently increased, a point is arrived at where the original length of the rod is totally unaffected by magnetization; and if the magnetization be carried still further, the original length of the rod will be reduced. It also appeared that the position of the critical point in steel depended in a very remarkable manner upon the hardness or temper of the metal; considerable light is thus thrown on the apparently anomalous results obtained by Joule and by Mayer. Further experiments disclosed strong reason for believing that the value of the critical magnetizing force in a thin iron rod was greatly reduced by stretching; this would explain the fact that Joule obtained opposite effects with stretched and unstretched wires.

By ascertaining the relative values of the temporary moments induced by gradually increasing external magnetizing forces, an attempt was made to connect the point of maximum elongation with a definite phase of the magnetization of the several rods in which the elongation had been observed.

Though more experiments must be made before it is possible to generalize from them with perfect safety, the results so far obtained by the author indicate the laws given below. The elongations and magnetizations referred to are temporary only; before the beginning of an experiment the rod was permanently magnetized by passing through the magnetizing coil a current equal to the strongest subsequently used. In iron the greatest elongation due to permanent magnetization was generally found to be about one-third of the total elongation, while in nickel the permanent retraction amounted only to about one-twenty-fifth part of the whole.

I. IRON.

(1) The length of an iron rod is increased by magnetization up to a certain critical value of the magnetizing force, when a maximum elongation is reached.

(2) If the critical value of the magnetizing force is exceeded, the elongation is diminished until with a sufficiently powerful magnetizing force the original length of the rod is unaffected, and, if the force is still further increased, the rod undergoes retraction. Shortly after the critical point is passed, the elongation diminishes in proportion as the magnetizing force increases. The greatest actual retraction hitherto observed was equal to about half the maximum elongation, but there was no indication of a limit, and a stronger magnetizing force would have produced further retraction.

(3) The value of the external magnetizing force corresponding to maximum elongation is for a given rod approximately equal to twice its value at the "turning point."

Definition.—The turning point in the magnetization of an iron bar is reached when the temporary moment begins to increase less rapidly than the external magnetizing force.

(4) The external force corresponding to the point of maximum elongation increases (when the quality of the iron is the same) with the diameter of the rod. So also does its value at the turning point.

(5) The amount of the maximum elongation appears to vary inversely as the square root of the diameter of the rod, when the quality of the iron is the same.

(6) The turning point, and therefore presumably the point of maximum elongation, occurs with a smaller magnetizing force when the rod is stretched than when it is unstretched.

II. STEEL.

(7) In soft steel magnetization produces elongation, which, as in the case of iron, increases up to a certain value of the magnetizing force, and afterwards diminishes. The maximum elongation is less than in iron, and the rate of diminution after the maximum is passed is also less.

(8) The critical value of the magnetizing force for a steel rod diminishes with increasing hardness up to a certain point, corresponding to a yellow temper; after which it increases, and with very hard steel becomes very high. There is, therefore, a critical degree of hardness for which the critical magnetizing force is a minimum; in steel of a yellow temper the value of the critical magnetizing force is lower than in steel which is either softer and harder.

(9) In soft steel a strong magnetizing force subsequently diminished may cause a greater temporary elongation than the diminished force is capable of producing if applied in the first place.

(10) A temporary elongation when once produced in soft steel may be maintained by a magnetizing force which is itself too small to originate any perceptible elongation.

III. NICKEL.

(11) Nickel continues to retract with magnetizing forces far exceeding those which produce the maximum elongation of iron. The greatest observed retraction of nickel is more than three times the maximum observed elongation of iron, and the limit has not yet been reached.

(12) A nickel wire stretched by a weight undergoes retraction when magnetized.

ELECTRIC TRAMCARS.*

By A. RECKENZAUN.

From "Iron."

It may be premature to read a paper on the subject of electric tramcars, considering that at this moment there is only one such car in existence in this country, so far as the author is aware. This solitary example has, moreover, only been in operation experimentally since October last. It was with considerable hesitation, therefore, that this paper was prepared at the invitation of the council of this institute. The Inventors' Institute has, nevertheless, the privilege before other societies in this respect, that it will judge of the merits of an invention as an invention, apart from the commercial aspect of the problem. Utility, however, is the first desideratum in an invention, and the author now merely submits to this institute the bare question of utility. Before going into the details of our subject, it may be interesting to dwell for a moment upon the figures in the table below, which was prepared in order to show what power a pair of horses are capable of exerting. The power exerted in propelling a 46-passenger car, tractive force 30 lbs. per ton, two horses pulling 4.5 tons, is at—

	Horse-power
7 miles per hour on level road	2.52
6 ,, ,, ,, ,, ...	2.16
6 ,, ,, on a gradient of 1 in 75..	4.32
5 ,, ,, ,, ,, ,, 1 ,, 37..	5.4
4 ,, ,, ,, ,, ,, 1 ,, 37..	4.32
3 ,, ,, ,, ,, ,, 1 ,, 25..	4.32
4 ,, ,, ,, ,, ,, 1 ,, 25..	5.76
5 ,, ,, ,, ,, ,, 1 ,, 25..	7.2
3 ,, ,, ,, ,, ,, 1 ,, 18..	5.4

The additional power necessary to pull a car round curves cannot be ascertained with equal accuracy; it depends upon the radius of the curve, the amount of play in the axle boxes, and the size of the wheel flanges; a flexible wheel base will considerably facilitate the movement on curved roads.

STARTING FORCE.

The force required to start a car and to get up speed is necessarily greater than that required to maintain the speed uniformly. "It is a variable quantity," says Mr. D. K. Clark, in his admirable book on tramways, "for it may be anything that horses choose to exert." But it has been found by experiment that the momentary starting force is about four times the tractive force when once in motion; thus we may form a rough idea as to the exertion of a horse in starting

* Paper read before the Inventors' Institute.

a car on the level or on an incline. Horses cannot tell us of their sufferings; we know, nevertheless, that their life in the tramway service is but short, although they work no longer than three to four hours a day. It is barbarous to use horses, as these figures show, yet there has been until recently no economical substitute, and it is only within the last few years that mechanical traction has made any headway. That mechanical power will supersede animal power, and that at no distant date, is admitted on all hands, but the question of the kind of mechanical power to be employed is still an open one. It is often asked why so much mechanical power is required for the propulsion of tramcars, and why it is that a tram locomotive should be made to give as much as 40 indicated horse-power and more, whilst two horses seem to do the same amount of work.

The above table shows what mechanical work is actually being done. James Watt ascertained experimentally that a strong dray horse is capable of producing a continuous effect of 33,000 foot-pounds per minute; but we see that one tram-horse does the work of three or four dray horses very frequently. When we consider that a tramway steam-locomotive often weighs from 8 to 10 tons without the car and passengers, it becomes evident that the indicated horse-power above given is no extravagant measure. Take a locomotive, car, and passengers as weighing together 13 or 14 tons, then, in order to move that load on a level road at a speed of 7 miles per hour, with a tractive force of 30 lbs. per ton, we require from 7 to 8 actual horse-power, which is equivalent, after allowing for engine friction, to about 11 indicated horse-power; and, when traveling up an incline of 1 in 37, something like 34 indicated horse-power. Reducing our figures to a co-efficient, and maintaining that the tractive force is 30 lbs. per ton on a level but dirty road, we come to the conclusion that, when moving at a rate of 7 miles per hour in a straight line, we shall consume 8 foot-pounds of work for every pound of weight on the rails; on an incline of 1.75 we consume 16 foot-pounds, and on an incline of 1.37, 24 foot-pounds for every pound weight carried at the same speed. Therefore it is of the utmost importance to reduce the deadweight to be propelled to a minimum. Where the locomotive engine has to drag the car behind it, it becomes necessary to provide weight in order to obtain good adhesion on the rails, and the best plan, no doubt, would be to utilize the weight of the car and passengers for this purpose.

The number of steam-locomotives employed on tramways is daily increasing, and steam traction is gaining in public favor. It is not, however, within the range of this paper to examine into the advantages or disadvantages of steam traction; but merely to show whether electric tramcars have any chance of success from a utilitarian point of view. We distinguish between the terms electric cars and electric tramway or railway; the electric car carries its energy within itself, and is quite independent of external influences; whereas, in electric tramways, the energy, electricity, is conveyed from the generating station to the rails or other conductor communicating with the motor which turns the car wheels. Separate conductors are used by Messrs. Siemens, at Portrush or elsewhere; Mr. Magnus Volk, at Brighton, conveys the current through the rails—the nature of the ground (shingle) on Brighton beach does not allow of any accumulation of rain or dirt, the rails are above ground, and there is no traffic across the line; an electric tramway is being constructed at Blackpool, where current is conveyed through conductors in a pipe laid underground between the rails; this pipe has a slot throughout the whole length to permit of the connection between the car motor and the conductor beneath the surface of the road. Our electric car, as the model shows, requires no conducting medium; it does not interfere with the rails or roadway nor with other traffic, and it can be shifted from one line to another of the same gauge, it can be run in conjunction with the ordinary horse-cars.

THE BATTERY.

In order to make an electric car, we require a battery which can be stowed away within the car. Such battery has to be of small weight, must be reliable, supply any quantity of current according to the exigencies of the road, must be cheaper than horse flesh, and emit no smell. Primary batteries are out of the question;

the cost of zinc consumed in them would be much greater than that of animal power, independent of the difficulties attending the process of refilling such batteries when exhausted. The invention of the secondary (storage) battery put the question entirely in a new light; certain successful laboratory experiments at once suggested the adaptability of this invention to the propulsion of vehicles. Four years have elapsed since the introduction of the Faure accumulator into this country. It caused quite a sensation at that time, and money could be found in abundance for the exploitation of anything that bore an electrical name. Things have changed entirely since then. The original Faure battery has never been of any practical use, and very substantial improvements had to be made, at enormous expense, to bring the secondary battery to a commercial value. The Electrical Power Storage Company have required the patent rights of M. Faure, in addition to those of Messrs. Sellon, Swan and Volckmar, and they have, step by step, improved the mechanical details of these storage batteries, until they arrived, after immense labor, at what may be termed a thoroughly practical article. But the prejudices created in the public mind by early failures have not quite worn off yet, and it will require something wonderful and startling to convince the public that the work of the last four years has produced remarkable and practical improvements.

None but those who have been intimately connected with the manufacture of storage batteries can form an adequate idea as to the numerous difficulties which had to be surmounted before a permanent success was attained. The battery was long perfect from a theoretical point of view, but it could not stand the test of time—time was an essential factor; it took many months to test the durability of a set of cells, and it was only by close observation and careful remedying of defects, one by one as they appeared, that the storage battery assumed its present form—simple as they may appear to the uninitiated. The cell before you is one of a set of the type specially designed for tramcar work. In a lead-lined strong teak box are placed twenty-one lead plates, weighing together 26 lbs. inclusive of connecting strips and terminals. Ten of these are called positive, and eleven negative. Each plate is formed of a leaden grid, the perforations of which are filled with a paste of lead oxide; the positive plates contain red lead, which, in charging, is converted into peroxide; the negative grids are filled with a paste of litharge, which, in charging, is reduced to spongy lead capable of absorbing hydrogen. We store, therefore, not electricity, but oxygen and hydrogen, which gases, when the battery is discharging, manifest themselves externally in the form of electrical energy. The box is filled with sulphuric acid and water of a specific gravity of about 1150°; then a lid is put on, sealed all round the edges to prevent any spilling of the acid. This acid is never removed so long as the battery lasts. There is no reduction of lead or any material going on within the cell, and the battery would last for ever, but for the fact that the lead grid of the positive plates becomes so brittle through oxidations that it crumbles to pieces in course of time; then, these positive plates have to be replaced periodically by new ones; but the old lead is valuable. The life of a positive plate depends entirely upon the amount of work it has done. The plates in the box before you have been at work since September last, and are still in excellent condition—we should say they are still as good as new. They have frequently been discharged at the rate of 100 ampères, whilst the average working current is 46 ampères, they are always charged at the rate of 32 ampères, and their storage capacity is 150 ampère-hours. Sixty such cells will weigh 1¼ ton, and propel a car with forty-six passengers for about two hours over a road with ordinary gradients, curves, and sixty stoppages per hour.

The diagram (which was exhibited by the lecturer) represents such a car, which has been running at Millwall and at Battersea. The accumulators are placed on trays under the seats out of sight. These trays can be drawn out through doors at one end of the car, and replaced in a few minutes. A trolly containing a fresh set on trays and rollers is drawn up to the end of the car. The discharged cells are pulled out all together by means of a small winch, and the newly-charged cells pushed in, when the car is at once ready to proceed on its journey. There are

three sets of accumulators to each car, two sets being charged, while one set is propelling the vehicle, thereby saving time and preventing delay.

THE ELECTRO-MOTOR.

This machine has to convert the electric current into mechanical power. For tramcar propulsion it is absolutely necessary that the motor should have a high efficiency, and, at the same time, be of small dimensions and of light weight. To combine these requirements has been the aim of the author, and he has, at length, produced a machine which has had some rough tests in actual service under the most trying circumstances and conditions. There are two motors driving the car, each capable of working up to nearly 9 horse-power and weighing 420 lbs. Each motor is supported independently upon a small bogie; the whole mechanism is self-contained; and each separate bogie forms a small locomotive engine, upon which the car rests. One axle of each bogie is a driving-axle. Thus we obtain four small driving-wheels, which give the requisite grip upon the rails. Either bogie can be detached from the car in less than one hour, so that in case of repair and inspection the motor can be taken out and replaced without letting the car stand idle for any length of time. The speed of the motors is high, about 1,000 revolutions, when the car is running at seven miles an hour; thus it is necessary to introduce some mechanical reducing gear between the motor-shaft and the driving-axle.

THE GEARING.

The gearing so employed consists of a worm on each motor-shaft and worm-wheels on the driving-axle giving a ratio of about 1.12. This worm-gearing is boxed in, as likewise is the motor, and the wheels run in oil, dirt is thereby excluded, and the lubrication kept perfect. Easy access is obtained to motors and lubricators through doors in the floor of the car.

VARIATION OF SPEED AND POWER.

This is obtained by means of a compound switch, which arranges the motor-circuits so that the machines shall work in series, in parallel or singly; thus the resistance of the circuit being varied, the power and the speed vary accordingly. When a greater range of speed is desirable, the motor-circuits are still further divided by arranging the field magnet wires apart from the armatures. This obviates cumbersome gearing, which would add to the weight and the expense also—increase first cost and maintenance as well. The driver has full command over the motive power; one handle sufficing for all the operations of starting, stopping, and varying the speed or power. There is no useless electrical resistance, and, therefore, no waste of energy, whatever speed the car may be traveling at. The car is provided with these details at both ends, so that the driver has only to remove the handles and two connections when arriving at the journey's end, and then proceeds. It would be an easy matter to vary the speed by decreasing or increasing the number of cells, thereby varying the electromotive force; this method, however, is injurious to the accumulators, because some of the cells would be discharged sooner than the others, and when they are all re-charged in series, some would have to be very much overcharged before the rest could receive their proper share. There would be not only a waste of power occasionally by the evolution of gases for no purpose, but the life of the cells and their efficiency is reduced by this irregular treatment.

THE BRAKE POWER.

At each platform there is the usual vertical shaft and brake-handle. A chain is wound upon this shaft when the handle is turned, and eight brake blocks are simultaneously pressed against the corresponding number of wheels. The car can be stopped almost instantaneously; but beside this there is an electrical brake, so that the motors act as dynamos driven by the momentum of the car or by the car running down an incline; the whole power stored up in the momentum of the car is converted into electricity and the current generated is utilized in magnetizing the brake-blocks, thereby increasing their grip upon the wheel tires. Arrangements are being made to render this electric brake automatic, so that the main circuit will be broken and the brake-circuit with the motors closed automatically when the speed of the car attains a certain maximum.

COST OF MOTIVE POWER.

It has been mentioned that the capacity of the tramcar cells is 150 ampere-hours. We do not exhaust them entirely, but leave a margin of at least 20 per cent. in the cells. A charge of 120 ampere-hours is sufficient to propel the car full of passengers for two hours or about 12 miles over an average road with frequent stoppages. When charging sixty cells at the rate of 32 amperes for four hours, and replacing the accumulators in the car every two hours, we require steam power to the amount of about 15 indicated horse-power per car. Assuming that the car has to run 72 miles a day, and that we are supplying several cars at the same time from one engine, the fuel consumed need not exceed 4 lbs. per indicated horse-power per hour. The charging takes place during 12 hours of the day only. Thus 7 cwt. of coal per car per day will give a consumption of about 10 lbs. of coal per mile. Reckoning the price of coal at 18s. per ton, the fuel per car-mile would cost less than one penny! By working longer hours, we could do with smaller engines, but, of course, with the same consumption of coal per car-mile. The most economical steam tramway locomotives burn from 9 to 11 lbs. of coal per mile, or about the same as quoted for the electric car. There are two reasons for this consumption. Firstly, the steam locomotive weighs four times as much as the accumulators and electric motor and driving gear—therefore, it requires greater power for its own propulsion; secondly, a tramway locomotive boiler and engine cannot be expected to compete with a large stationary engine as regards economy. The loss thus arising from the conversion of steam power into electricity, and the reconversion of electricity into mechanical power is more than compensated by corresponding advantages. There are instances where water power is available within a reasonable distance from the tramway depot, in which cases the additional economy will be manifest.

PRIME COST, MAINTENANCE AND DEPRECIATION.

The steam engines, boilers, dynamos, and shafting, and all needful apparatus for a charging station to supply a dozen electric cars, including spare power, will cost £4,000, and the complete equipment of twelve two-horse cars, inclusive of ample spare gearing, may be estimated at £6,000. The superintendence of machinery at the charging station will cost £1,100 per annum; fuel at 18s. per ton, water, oil, and waste, £1,400; depreciation, at 10 per cent., on engines, boilers, and dynamos, £400; and an estimated depreciation of 35 per cent. on the whole propelling apparatus. This gives a total expenditure of £5,000 per annum, which is equivalent to 3.5d. per car per mile run. You will observe that these figures are thoroughly reasonable and allow of a good margin. We should have almost to annihilate the whole concern at the end of a year in order to bring the working costs to such an amount as is now allowed by some tramway companies for horsing!

SUMMARY.

I have pointed out the chief but yet only a few of the advantages of this system over horse traction, and I will in a few words enumerate them:

(1) Economy in cost of running, which is estimated at about 3½d. per mile, including depreciation.

(2) The appearance is that of the cars now in general use, and these latter can be readily converted.

(3) The wearing parts of the mechanism which drives the wheels are few in number.

(4) The total weight of the motive power mechanism is less than two tons distributed over two small bogies, whereby the load per unit of rail section is rendered even less than in the case of horse cars.

(5) The propelling apparatus is invisible to the passengers, noiseless, clean, and perfectly safe.

(6) One man (not necessarily skilled) is sufficient to drive an electric car.

(7) The car may be illuminated at night by the electric current sufficiently to enable the passengers to read with comfort, while effecting a saving over oil lamps in cost and avoiding their dirtiness. The power required is so small that the cost may be neglected; the 20 candle-power lamps used consuming only three ampere-hours out of the total of about 150 ampere-hours given by the cells.

(8) The maintenance of the permanent way (including paving) is less than with horse cars, where the hoofs are continuously striking in the same tracks. Existing rails will serve for the electric car of this system, and no additional rail or tube or breaking up of the road is necessary, preliminary to its adoption.

(9) The space required for a charging station is much less than that necessary for stables for a corresponding number of horses.

(10) The same plant which charges the storage batteries may be utilized for lighting the depot and other neighboring buildings by running extra hours at trifling further cost, thus yielding additional revenue.

DISCUSSION.

Professor George Forbes, F.R.S.E., gave a short and concise account of the electric railways laid down in various parts of the world within the last few years, and he considered the methods of conveying the electricity through rails or other conductors clumsy and unsatisfactory. In Mr. Reckenzaun's tramcars they had no stationary engine connected with it by a conductor, and this he considered, other things being equal, a very great advantage. After the experiments made by M. Philippart with tramcars propelled by a Faure battery, he was glad to hear Mr. Reckenzaun say that storage batteries had at last been made commercially useful and practical. He now believed the difficulties which had encountered secondary-battery makers in the past had been surmounted, and that the method of constructing them had been so enormously improved, that those who used them were now masters of the situation instead of *vice-versa*. Pointing to the battery exhibited on the table, the speaker said every one was struck with its light weight, which was brought about by its being made of thin plates, thus giving a large surface and a greater current than had ever be found before in a secondary battery. This he considered one of the chief things they had to be thankful for, because this bringing within a small compass sufficient power to last for a few hours had been their stumbling block in the past. He had been informed that it was impossible to overcharge their batteries and that this could be done for months and months, but it is mentioned in this paper that this overcharging is detrimental to their working. The method of varying the speed and power devised by Mr. Reckenzaun is extremely ingenious and simple, avoiding the disadvantages of mechanical arrangements which would necessarily arise with numerous wheels, chains, or the like. As to the efficiency of electrically-propelled cars as compared with other means of locomotion, he thought that, although steam had made great progress of late years in the propulsion of tramcars, it still had very serious drawbacks, among which were that a small portable locomotive was far less economical than a stationary engine, especially as for the former they had to use a superior quality of fuel. Comparing the Scott-Moncrieff compressed-air car with the one then under discussion, Professor Forbes remarked that it seemed to him that the arguments which applied to one applied to the other, and also pointed out that in Mr. Reckenzaun's car they saw for the first time the application of a worm-wheel to reduce the speed of the motor. In conclusion, he said it was satisfactory to hear that Mr. Reckenzaun was using electricity itself as a brake upon his car, because he thought that where electricity was applied as a motive power it ought also to be used as a brake power.

Mr. Traill (Giant's Causeway Electric Tramway) said that electric-tramway engineers had a great battle to fight. They did not want to replace steam, but to aid railways and, at the same time, the outside public. He thought that through electricity underground railways would become things of the past. There was also the system of overhead tramways, such as was used in America, and which the electric motive power was essentially fitted for, as it did away with the noise and dirt. There were four systems of utilizing the electric energy, and there was no doubt that the system carrying its own motive power, self-contained in its own car, had very great advantages indeed over any other system. The different systems were those which had overhead conductors, those which had side conductors, those which had underground conductors, and the system of accumulators. The overhead conductor was suited to very few places; then there was the side conductor, which they had

adopted on the Giant's Causeway Tramways. This system was very economical, and had many advantages, but after all was only applicable in very few places. There was the underground conductor which could be adapted to street tramways. The fourth system, viz., that of accumulators, had many advantages; indeed, it could be used on any line now worked by horses or steam. After repeated failures at the Giant's Causeway, they had at last hit upon a thoroughly reliable plan for getting the electricity from the conductor. This was by means of a steel spring in the form of a carriage spring, two concave steel springs fastened at the top and rubbing along the bottom.

Professor Forbes: "How long do they last?"

Mr. Traill replied that they had some running for nearly a year, and, when new, they only cost 5s. 6d. He was very much pleased with the system of Mr. Reckenzaun, and, having no doubt that it would come into very large use, he wished it every success.

Mr. Bernard Drake was of opinion that electric cars would have to be made smaller than those which had up to now been constructed in accordance with Mr. Reckenzaun's designs. He pointed out that not only would the smaller cars be more tractable in case of accident, but also that the question of stopping the car would not be so serious, as it stood to reason that if there were forty-six passengers on a car instead of twenty-three, the driver would be compelled to stop double the number of times in a given distance, and that the question of starting such a big car as now used was a severe strain. He considered that the arrangement adopted by Mr. Reckenzaun, of coupling the motors in series or parallel, or running a single motor, was both ingenious and practical; but as regards the overcharging of the extra cells, he thought that the question was more one of carrying the deadweights of these extra cells, which Mr. Reckenzaun had so ably explained was of the greatest importance in ascending gradients than the mere destruction of the cells themselves. It was, of course, wasteful to expend your power in the evolution of gas after the cells were full, and would tend somewhat to soften the peroxide which might, therefore, be more liable to drop out when jolted by the car. He maintained, however, that the overcharging was not nearly as detrimental as was generally supposed. Mr. Drake drew attention to the large output per pound of lead obtained from the tramcar pattern of cell, and the general practical nature of the supporting of the plates and connections, and stated that, as managing engineer of the Storage Company, he felt certain that the company was greatly indebted to Mr. Reckenzaun for the developing of the cells in connection with this class of work, inasmuch as numerous questions of detail had been found out by him, and that by these experiments a most valuable advance had been made in this class of accumulator. Referring to air cars, he thought there were very decided advantages in favor of the electric car, not only in weight, but also from the fact that the pressure of air began to fall from the moment the car came into use, for which mechanical arrangements had to be provided, and also the freezing caused by the expansion of air had to be faced, whereas in the electric car the E.M.F. is practically constant throughout the run, also the electric car would hold its charge for weeks together, whereas the air car charge would entirely leak away in a night.

Mr. Traill rose again and stated that his cars in Ireland have successfully traveled over 30,000 miles with 100,000 passengers, and that the electricity generated by water-power a mile distant costs one quarter of steam used on the same tramway. The commutator of the dynamo made twenty-two million revolutions before it required repairing.

Mr. Shoolbred, M.I.C.E., said that he had seen Mr. Reckenzaun's car running on a difficult piece of line at Battersea, and he was struck with the simplicity of the system and the completeness of design in every detail. He thought that Mr. Reckenzaun's plan of varying the power and speed was just as applicable to a tramway having the current supplied through conductors as it is with accumulators.

Admiral Selwyn, in the course of some general remarks, said he hoped the Inventions Exhibition would help the inventor to take his proper place in the country instead of being absorbed by the

manufacturers, which was, after all, only one result of a defective patent law.

Mr. Waugh having spoken in encouraging terms of the prospects of electrical engineering, the chairman called upon Mr. Reckenzaun to reply.

Mr. Reckenzaun, in reply, testified to the great pleasure and instruction he had derived from the observations of Professor Forbes, Mr. Traill, and the other speakers, and that he felt highly encouraged by the remarks of such authorities. He pointed out that the comparisons made by Professor Forbes between compressed-air cars and electric cars do not hold good in all points, although there is a great similarity in principle. Compressed-air cars are much heavier, the weight of the air reservoirs under the car and its engine cannot be less than four or five tons if made to run eight or nine miles with a pressure of 500 lbs. to the square inch; the accumulators, motors, and gearing in the electric car weigh under two tons in order to propel the vehicle with forty-six passengers over a distance of twelve to fourteen miles, and the time occupied in changing the cells at the end of the journey need not occupy more than three minutes. The moving parts in the mechanism of the air car, like those of the steam car, are at least five times in number as compared with the parts on the electric car, and this must be taken as a very great advantage. Moreover, the efficiency of the compressed air engine is lower than that of the accumulator and electric motor. Mr. Drake has very justly said that whilst the pressure in the air car is gradually falling, the current of the electric battery is constant throughout, but as regards Mr. Drake's opinion that small cars may be more efficient than large ones, Mr. Reckenzaun remarked that a car capable of carrying only twenty-three passengers will weigh much more than half the weight of a 46-passenger car, and that if the traffic is sufficiently large, the bigger cars may be more economical. As regards the overcharging of the batteries, if some cells were used more than others of the same series, which would occur were the speed and power regulated by the number of cells, more stress should have been put in the text of the paper upon the fact that the energy wasted is more serious than the fact of sooner oxidizing the lead plates. Still, the object of the present system of regulation removes both these drawbacks.

A COMPARISON OF BRITISH AND METRIC MEASURES FOR ENGINEERING PURPOSES.

By ARTHUR HAMILTON-SMYTHE, B.A., M. Inst. C.E.

Proceedings of the Institution of Civil Engineers.

III.

Professor H. Hennessy observed that the utility of a complete international system of weights and measures, based on the existing international system of counting and calculation, seemed to be clearly established, and the author of the paper had shown that no profession would profit more by such a system than the profession of civil engineering. If the whole question were open to revision, probably a decimal metric system, founded upon a standard of length common to all nations, such as a fraction of the earth's polar axis, would be most acceptable. This had been proposed by Professor Hennessy long since, and a complete series of measures and weights derived from it had been exhibited at South Kensington in 1876. It fulfilled all requirements of a complete system, and on account of the close approach of one of its fractions to the existing British inch,* it was strongly supported by some eminent persons, but when he reflected on the fact that the majority of the civilized part of mankind had already adopted the French metrical system, and that every day this system was becoming more fa-

* The polar inch differed from the British inch by the thousandth part less.—H. H.

miliar to British students of engineering through their observation of foreign works, or perusal of foreign scientific and technical publications, he could not hesitate to express his concurrence in the views of those who advocated its adoption for all nations. Some inconveniences would have to be endured for a short time before workmen and calculators were familiar with the new system, but these inconveniences had been cheerfully encountered by great nations alalready, and their experience might be fairly appealed to in answer to this difficulty. Men of science and engineers, whose minds were accustomed to accurate estimates of number and magnitude, would, in general, have no difficulty in employing more than one system. Thus, while he had found it advantageous to use the decimal metrical system in applied mechanics, thermodynamics, and mathematical physics, he had been often obliged also to employ the British system, on account of its use in ordinary applications. But this very circumstance had only made the superiority of the metrical system more distinctly felt, and made him more than ever desire that it alone should be employed throughout the world.

Mr. C. L. Hett remarked that in 1870 the Butterley Company obtained the contract for the Dordrecht Railway Bridge, and he had prepared the drawings for the steam cranes, scaffolding, &c., required in its erection. As the piling and superstructure of the staging had to be erected by a Continental contractor, the drawings were made to metric measures. Drawing-scales were ordered with the most generally used English graduations on one edge, and corresponding metric divisions on the other. By the use of these scales all difficulty was overcome, and rapid progress made. The drawings of the bridge itself were prepared in Holland, under the supervision of the Dutch Government engineers, and were dimensioned in metric figures throughout. But in the works there was an outcry. The men at first said they could not and would not work to such outlandish dimensions. The purchase of a few metric rules, however, settled the difficulty, and, after a fortnight's practice, one of the old hands who had been most opposed to them, admitted that the metric measures were much the easiest to use, especially where an even pitch could not be employed, but a given number of equidistant rivets had to occupy a certain space, the pitch being an odd dimension. It should be mentioned that no member of the Butterley staff nor any of the workmen had had any previous experience of metric measures. The absurdity of the present system of measurement was well exemplified in the case of railway bridges and of drainage pumping works. In each case a plan of the site was supplied drawn to a scale of chains and links, the heights being given in feet and decimals of a foot, while the drawings for the ironwork were made in feet and inches. The standing arguments used against the metric measures were the Whitworth screw-threads, the 1-inch and 6-inch ordnance maps, and the loss that would accrue from the depreciation of patterns and templates. The former difficulty was not felt abroad when the metric system was established, any more than inconvenience was experienced at home from the odd diameters of the standard gas-threads. The second objection could be removed by engraving metric scales on the plates from which the maps were printed. At present, owing to the variable contraction of the paper, accurate measurements could only be taken with the scale printed on the maps. Even now the ordnance maps on the larger scales were made without regard to the time-honored yard and inch, being plotted to scales of $\frac{1}{2500}$, $\frac{1}{500}$, &c., of the actual size. On such maps, metrical scales could be used with great convenience, but a rule divided into inches was valueless. The last objection appeared at first to be a very important one, yet it was more so in appearance than in reality. A thoroughly good set of patterns or templates would retain their value, while obsolete ones would probably be destroyed rather sooner than if there had been no change in the measures.

With regard to the British system, or rather want of system, of weights, little need be said. In trying to improve them, matters had only been made worse. The cental of 100 lbs. originated with the Liverpool corn merchants, who after using it illegally for years, obtained Government sanction for its adoption in 1879.

It was expected to effect a reform in the sale of corn, but nothing of the kind had occurred. At present a bill was before the House of Commons to render the use of the cental compulsory in the corn trade of England. The scheme of decimalization of the coinage mentioned by the author, and which had been so frequently advocated, might be easily introduced. The poor would approve of it, as they would get 12½d. for 1s.; but the difficulty would be in the temporary loss to the revenue of 4 per cent. in postage and receipt stamps. Although this subject had been thoroughly discussed years ago, it was under very different conditions to those which now prevailed. At that time there was hardly a competitor with England in the foreign trade. The British were virtually in a position to dictate to countries employing the metric system, and to say, "There is our machinery made to our measurements, and without the slightest consideration of your convenience." But now it was different; there were makers from countries employing the metric system ready to supply any order for machinery, and year by year the excellence of their goods would be improved until their workmanship was equal to our own, and our last advantage over them would be gone. It was sometimes urged that if the United States did not adopt the metric system, why should England? The Americans were agitating for its adoption, and the first country to change would have a great advantage in trading with those other nations who had already adopted it. At the same time America did not purchase machinery from the British, and therefore their case need not be considered. Now that education was compulsory, the extra work involved on the rising generation and their instructors by the clumsy system of English weights and measures should be carefully considered. As so much was being spent in education, and cases of "over-pressure" were so frequently reported, should not an effort be made to abolish a system which was admitted on all hands to involve an enormous amount of unnecessary labor? The feeling that the metric system must sooner or later be adopted was not confined to engineers and scientific men; it was shown by the action of the Leicestershire Chamber of Agriculture in 1878, which opposed the legalization of the cental on the ground that it might impede the adoption of metric weights and measures. Few persons attempted to defend the present system, yet there was a general hopelessness of any change. Improvement had been advocated, but any alteration would involve greater confusion than a total change, while the international character of the metric system would not be attained. The importance of the subject was so great that Government might be expected to aid the change by adopting the metric system throughout their departments. If adopted exclusively in the customs, metric weights would immediately be made the basis of railway charges, much to the relief of goods clerks and accountants. Wholesale houses would follow, and gradually the system would be accepted by the small retailers.

Mr. Ferdinand Hurter, Ph. D., held that the only important reason for a change from the English to the metric system was that many nations had adopted the latter. Perfect internationality was, however, impossible, since Germany had not adopted the coinage of other continental countries. The metric system did not save so much time as was usually claimed for it. Where a decimal system was obligatory, the English people used it. True, it required more time to attain a knowledge of the English system and its special arithmetic at school; but that time was not lost, and the pupil who mastered the greater difficulties became the better man in the end. The examples of saving of time which the author had given were not very well chosen. If the French carpenter had a folding meter rule, he would not measure more quickly than the English carpenter; if the rule were not jointed, give the carpenter a five-foot staff or a measuring tape. The multiplication which the author supposed the carpenter to make was never made to his knowledge. The carpenter said 2, 4, 6, &c., as he turned the rule over. He was certain that a man well acquainted with his business could accomplish as much work, on an average, by the English system as he would by the metric system, and in considering so important a question as a change from one system to another, the

difficulties which the system presented to the uninitiated must not be left out of sight. The calculation of the weight of water in a tank could have been much shortened by abandoning the decimals of feet, and by remembering that one ton of water was 36 cubic feet (error 0.2 per cent., or 5 lbs. to the ton). One point had been overlooked by the author, namely, that the one-foot rule was as often used as an instrument for subdividing as it was to obtain data for a calculation. In this respect the one-foot rule, with its binary and ternary subdivisions, was as much superior to the metric rule as a circle divided into 360° was superior to one divided into 100^c. Suppose it was required to subdivide 1 meter, this could be done by means of one thousand divisions in fifteen different ways. Take the same length, 39⅜ inches, and use an English two-foot rule, divided into sixteenths of an inch, and there were twenty-three ways of subdividing it into equal parts, though the eye had only 630 divisions to examine, instead of one thousand. But, compare the yard in this way: it could be divided by means of an English rule, obtainable for 3s., in forty-two different ways. It was in the workshop that the English system was superior to the French; and the English inch was the standard for screw cutting, not only in England, but throughout the world. It must not be forgotten that the British public was not prepared, nor preparing, for such a change. Decimal calculation was unknown to the mass of the people. Boys were allowed to leave school, having passed the fifth standard, void of all knowledge of decimal arithmetic. Surely it would be unfair to take the masses by surprise! But, if the metric system must be, let it be the metric system pure—the metric system compulsory; not a metric system at the option of any engineer. Let there be no mixture, particularly in plans to be submitted to, and discussed by, parliamentary committees.

Mr. L. D'A. Jackson observed, with regard to the choice between British and French metric measures, that British engineers dealt more with persons of their own nation, with colonial English, with citizens of the United States of America, and with semi-civilized and barbarous nations, than they did with nations that had adopted the French metric measures. Hence British measures were preferable for the English engineer. It was also important to notice that Russia used British measures in part. This first guide to choice should certainly settle the matter. Next, the British measures were good, useful, suitable, and convenient measures, while the French metric measures were mostly mere nominal units, among which there were only three or four useful measures. Lastly, there was no excuse for adopting the bad measures of the French in England, where a single, though incongruous system, already existed. When the French, the Italians and the Germans adopted them, they had booksful of various measures of their own, and their internal jealousies prevented them from selecting among the best of those well known. Moreover, the installation of the meter was effected by fine and by imprisonment, the leading men having been previously won over by flattery. Engineers found that the British measures were inconvenient for purposes of calculation. If, then, they wished to decimalize, let them decimalize on their own existing units, thus keeping themselves in accord generally with the measures of their own nation. The selection of British units for decimalization should be left to committees of experts. In the interim, engineers should be allowed, by Act of Parliament, or by Order in Council, to decimalize without restraint on any or all of the existing British measures, with the sole restriction of adherence to some one unit and its decimal multiples, and sub-multiples, in each class of measure. The difficulties would be few, as any one complete decimal system was easily comparable with any other complete decimal system within the separate classes of length, surface, cubic and weight units.

Professor Fleeming Jenkin said he should like to record his opinion that if any change were made by engineers in their standards of measurement, they should adopt the C. G. S. system, as it is called, with absolute derived units for force, &c. This change must, in his opinion, be made sooner or later, whether engineers liked it or not; and any half measure, such as taking a kilogram as a unit of force, or a kilogrammeter as a unit of work, would only entail addition-

al inconvenience; but dynes and ergs had a clear practical advantage to recommend them. The C. G. S. system (centimeter, gramme, second) had been already sanctioned as regarded electrical measurements by the International Conference of 1883 and 1884, held in Paris under the presidency of M. Cochery. This was the thin edge of the wedge, and since the wedge was a good strong one, and the motive for driving it home was of overwhelming importance to physicists, driven home it would be. Those who wished to know what the C. G. S. system was might consult Everett on "Units and Physical Constants." If this system had been in practical use, the labor of preparing, for instance, his lecture on Gas Engines would have been halved, at least.

Mr. S. W. Johnson remarked that for the last twelve years the decimal system of measurement on the Midland Railway, referred t› by Mr. Fernie in 1863,* had been employed only for the tire boring, for contraction, and for the purpose of measuring test bars, while Whitworth's templates and gauges were in use for all other workshop details.

Mr. T. J. Nicholls observed that the incongruities and anomalies which were of daily experience under the accepted methods of weighing, measuring, and valuing, might be illustrated by reference to "Portland Cement." The evil was first apparent in the specification, drawn more or less in the following general terms: "The cement shall . . . and weigh . . . pounds per bushel (striked); . . . per cent. must be retained on a sieve of . . . meshes per square inch; and bricks of the neat cement must be equal to a strain of . . . pounds per square inch. The tests will be applied to . . . bricks in each lot of . . . tons," &c. But this did not exhaust the weights and measures connected with cement. It was sold "by" the ton (never by the bushel), and "in" either bags or casks of indefinite, varying bulk and weight. It was commonly used by the cubic yard (concrete), or square yard (plastering, flooring, &c.). The annexed table presented a transaction in Portland cement, in terms which were hardly credible as being those of actual experience: cement was

Specified for density............	per bushel.
The full of the bushel to weigh so many......................	pounds.
Bought "by" the..............	ton.
Delivered "in"................	bags or casks.
Tested for fineness.......... ...	per cent.
" over so many meshes per inch depending on....	S. W. G.
" for strength in lbs. per...	square inch.
And used by the...............	cubic yard or square yard.

Surely the fittest parallel to such application of figures as Portland cement evidently required, was to be found in the old farmer's clock which, to those who knew its ways, told half-past four in the day by striking twelve and pointing to five minutes to nine. A similar juggling had to be performed in dealing with iron. It was generally held to

Weigh.. a decimal of a pound per cubic inch	or pounds. per square foot 1 inch thick.
Was measured in..........	feet and inches.
Priced and brought per....	ton.
Tested for tensile strength in pounds per.....	square inch.
" for elongation decimally.	per cent.
" for deflection by a binary sub-division of the......	lineal inch, or by per cent. of span.

To give an additional example: Take the apparently simple case of estimating the weight of rails per mile of railway—

Rails, say...............	80 pounds per yard.
The multiplier for pounds per mile was...........	1,760!
And the divisor for tons per mile was...........	2,240!

Compare the arithmetical process involved by these figures with the methods of arriving at the number of units of gross weight under the decimal (metric) system, which was, so to speak, automatic.

Rails, say............	40 kilograms per meter.
Both { multiplier for weight (kilog.) pr. mile (kilometer) and divisor for tons (milliers), though little used }	=1,000

Thus the arithmetical process was effected immediately, and the required result obtained by simply changing the names of the multiplicand from kilograms to milliers.

To revert to the units in the case of Portland cement. It would be observed that only "pounds" and "tons," and

* Minutes of Proceedings Inst. C. E., vol. xxii., p. 606.

"square inch" and "square" or "cubic yard" had any fixed relationship, according to any tables in existence; and for purposes of calculation, these relationships were represented by the numerical expressions $\frac{1}{2,240}$, and $\frac{1}{36 \times 36} = \frac{1}{1,296}$, and $\frac{1}{36 \times 36 \times 36} = \frac{1}{46,656}$; while under a decimal (such as the metric) system, the representative fraction would be $\frac{1}{1000}$ or 0.000, and sub-multiples or multiples of it. The bushel was said to be of a capacity which, stated in terms of British units, simply could not be measured. On the authority of "Molesworth" it might be taken as equal to $2,218\frac{16}{100}$ cubic inches $=1\frac{28}{100}$ cubic foot. The inch and the foot (which were too small and too large as units of minimum dimension), were defined, or at least, their sizes were accepted, conventionally. Not so the pound weight. The British "system" boasted two distinct pound weights; and while those who knew what was meant, professed to find things easy, it could not be quite so clear to foreigners who might wish or require to refer to English calculations.

A popular magazine had lately informed its readers that a "grain" of gold might be beaten out to cover 56 square inches, with leaves only $\frac{1}{228000}$ of an inch in thickness. What did the statement convey to the general reader as to the relationship of the "grain" to the "inch," square or lineal? None; There was no such relationship. Casual readers might forget that there was any use for the word "grain" so applied, other than as a rough indication of minute bulk; but ancient history traced connection between it and the grain of wheat as a measure of weight; yet it might be that the "grain" of gold referred to would but ill compare with the grain of wheat, either as to size or weight. Hence the particulars in the statement, though not detracting from the wonderful ductility of gold, were wholly worthless as a comparison of actual facts. Anyone having more or less intimate knowledge of workshop practice, or intercourse with the foremen of departments, would realize the contortions of memory and the evolutionary processes necessary to enable the mind to comprehend the rough cube of a piece of stick which was announced to be 2 feet $1\frac{3}{4}$ inch long, $4\frac{5}{8}$ inches broad, and $\frac{7}{8}$ and $\frac{1}{16}$ inch thick. Such an experience might be compared with the equally systematic measurement of a timber log by a farm hand: "Five times the length of my stick, twice the length of my foot, then this straw, then half a brick, and a bit over." He had heard a university fellow and professor boast to his pupils that he had never mastered and did not "know" the English tables of weights and measures. The professor always had his sympathy.

It was to be hoped that the paper would lead to useful results. The advantages of the alteration it advocated were far from being overrated; they were such as seemed to afford an occasion for the origination of a movement having in view the final substitution of a simple and attractive system of numbers, already well tested, for the present anomalies, and the illogical jumble of terms and quantities. It would not be enough to decimalize the British tables of weights and measures; that was impossible, however practicable to deal in that manner with the coinage. Sooner or later there must be a complete decimal system for all purposes of calculation and measurement. It was to be hoped that it might not remain to be secured for the English, as it had been for the French, among the results of a sanguinary revolution.

Mr. C. E. Parker-Rhodes, late of H. M. Consular Service, was in favor of the metric decimal system in substitution for the present British weights, measures, and coinage. In the adoption thereof for all purposes no difficulty would arise, even in ordinary commercial and trade transactions, particularly in the measurement of land. As to advantages, he need only state that a case of survey had recently been submitted to arbitration at the Surveyors' Institution, in which an error of 1 acre in excess existed where the correct quantity was a little over two acres. Saving of time in calculation was likewise in favor of the system now acknowledged by numerous States; and by the diminution of operations and figures the memory was less taxed. The liability to error was also reduced to a minimum. The conversion of British weights, measures, and currency in foreign countries would always be to the disadvan-

tage of British interests, at the same time resulting in considerable fluctuations. International uniformity was greatly needed, as might be proved by the importations of all commodities under the British flag in foreign countries.

Mr. R. H. Smith thought experience proved that there was no great difficulty in introducing new measures into the practical life of a nation; certainly not as regarded engineering practice. Putting the case of France aside, Germany, Russia, Italy and Austria had adopted the French metric system throughout, practically, the whole of their engineering work, and had become familiar with it within a comparatively short period. Again, Sir Joseph Whitworth's workmen used a system different from any other, namely $\frac{1}{10}$ inch, and this without difficulty or liability to error. In the workshop liability to error was, under existing circumstances, more often due to the workmen having a variety of conflicting scales marked on their rules than to any other cause. The theoretic objection most commonly made to the metric system was that the duodecimal division was more advantageous than the decimal. As a matter of theory, he thought this argument mistaken. There was a real necessity for representing the fraction $\frac{1}{2}$ in the simplest possible fashion, whatever system might be adopted, because of the great physical importance of binary symmetry for the sake of balancing weights, &c. The great majority of dimensions was for such reasons marked off in halves, but beyond the fraction $\frac{1}{2}$ he thought no fraction had any more importance than another. Physically, the dimension of 0.88 inch would suit every purpose just as well as $\frac{7}{8}$ inch. Physically it was just as convenient to buy in 0.765 ton of iron as 15 cwt. If the 0.75 was preferred, it was only because the notation made 0.75 a little more easy to calculate than 0.765. If the notation adopted made it easier to reckon 0.765 than 0.75, then the commonly used fraction would be 0.765 and not 0.75. Therefore, so long as the system of notation represented $\frac{1}{2}$ simply, *i. e.*, so long as its base was an even number, he thought it of little theoretical importance what base was taken. Obviously 2 or 4 was clumsily small, because with these large numbers would be represented by great arrays of figures. Practically it was of the utmost importance to adhere to 10, because, although he believed it would be easy to introduce new physical quantitative units, he knew from repeated attempts that it was extremely difficult to change the fundamental idea of numerical notation, and also because the decimal system was not only deeply ingrained in our minds, but was widespread over the world, there being no nations having any notation at all, who did not count by it. No one who had lived and worked on the Continent, and who recollected at the same time what was the usage of scientific men all over the world, could for a moment contemplate the possibility of converting the continental peoples to the use of British measures. It followed that if an international system of measurement was to be attained, it could only be accomplished by the approximation of British to the best metric system that would be generally acceptable. Men devoted to pure science used now almost exclusively the centimeter and its derivatives. He believed the centimeter was commonly used in France by engineers also. In Germany, however, the millimeter was almost universally employed by engineers as the unit of length. The reason evidently was that the centimeter was too long to make it possible even commonly to avoid fractions in dimensioning drawings, &c. In calculating the diameter of a shaft, for instance, it would be most improper to round off the result to a whole number of centimeters, unless the size were an unusually large one. The thicknesses of plates could never be expressed without fractions. But in practice it was found that a fraction of a millimeter need never be used, except occasionally in fitting dimensions together, or for sheet metal, or for wire. The committee of the British Association recommended the adoption of the centimeter and the gram, because the system so based made the specific gravity coincide with the specific weight (weight per unit volume) of each substance. He submitted that this was no good reason. Specific gravities were of no use to engineers, or any other class of practical men. Tables of specific gravity he had always looked upon as nuisances. Weights should be calculated from experiments on the heaviness of the material dealt with, and from

linear measurements. For this purpose specific weights were wanted, not specific gravities. With tables of specific gravities only to go by, a double multiplication must be made where only one was needed according to any rational method. To realize the excessive uselessness of specific gravities, it should be remembered that the comparison only referred to water at one particular temperature and pressure. It so happened that in the one instance, where it appeared at first sight that specific gravities might be of direct practical use, namely, in the calculation of the buoyancy of ships; the specific gravity of the water to be dealt with, namely sea-water, was so far from unity that the difference could not be neglected. Specific weight was what was always practically dealt with, and the specific weight of water was certainly not the most important specific weight entering into the calculations of engineers. It was hardly used, except in hydraulic engineering. In steam engineering, that of steam was of vastly more importance than that of water. He thought, therefore, that for practical purposes there need be no definite relation between the units of length and of weight or mass, and the most convenient units he had used in his own calculations were the millimeter and kilogram.

Mr. W. E. Thursfield observed that there could be no question as to the desirability of an international standard of weights and measures for engineering purposes; and, as England and America were the only two countries which still retained their old-fashioned and complicated methods of arriving at arithmetical conclusions, nothing more than their common assent was necessary to give an international character to the metric and decimal systems employed by engineers on the Continent. Having had some twenty years' practical experience of the immense saving in time and labor, of the simplicity of manipulation, and of the greater accuracy obtainable by the use of the meter as the standard of all dimensions in every branch of technical work, he had great pleasure in bearing testimony to the undoubted advantages, in the several instances adduced by the author, of the metric and decimal systems, and in supporting his arguments in favor of their adoption in place of any other. With regard to the depreciation of existing English technical literature, and of machinery compiled and constructed in accordance with the present systems of calculation and measurement, the rapid improvements in detail, and the wear and tear of material, necessitated renewal from time to time; and the advantages to be derived by the student, as well as by the manufacturer, from the means afforded them, by an international standard of comparison, of judging the theoretical and practical results of their professional brethren in other countries, must be of more value than the loss of waste paper and of old metal, left on their hands at the end of the period of transition. In drawing a comparison between the descent in integral fractions, of the subdivisions of a meter, and of an inch (as quoted by the author), Mr. Sellers appeared to have lost sight of the fact that the meter was divided into 1,000 parts, not 100; and that it was, consequently, capable of fourteen integral subdivisions—$\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{8}$, $\frac{1}{10}$, $\frac{1}{20}$, $\frac{1}{25}$, $\frac{1}{40}$, $\frac{1}{50}$, $\frac{1}{100}$, $\frac{1}{125}$, $\frac{1}{250}$, $\frac{1}{500}$, $\frac{1}{1000}$, all of which could be contained on one scale; whereas, to obtain the full benefit of the inch scale, three separate forms of subdivision, or three scales, were necessary, namely, the inch divided in terms of $\frac{1}{8}$, $\frac{1}{10}$, and $\frac{1}{16}$. That sizes of machinery advancing by $\frac{1}{16}$ inch were more useful and salable than those advancing by millimeters, was rather a matter of local prejudice than of universal opinion. It must be immaterial to a purchaser whether the cylinders of several engines submitted for his choice had advancing diameters of $9\frac{7}{16}$ inches, $9\frac{1}{2}$ inches, and $9\frac{9}{16}$ inches, or 239 millimeters, 241 millimeters, and 243 millimeters, as the respective differences between the latter dimensions in millimeters and the former in sixteenths of an inch, were only 0.25, 0.08 and 0.07 per cent. It would be equally correct to surmise that a fourteen-hand cob and a sixteen-hand hunter would be less useful and salable if advertised at 147.5 centimeters and 168.5 centimeters, supposing the purchaser to be intimate with the metric system. If anything, he thought the author undervalued the saving in time by the use of the metric and decimal system in calculation; the surveyor, most expert at quantities, with the handiest of ready reckoners, would

scarcely be able to work out the same number of items expressed in tons, cwts., qrs., and lbs., at x shillings per ton, in one hour, as could be calculated by anyone, with only a short practice in the decimal system, in three-quarters of an hour, if the weights were expressed in kilograms, and price in shillings per meter ton, to say nothing of the increasing chance of error in proportion to the greater number of operations, and of the longer time necessary to check the results, without which the calculations were useless. With regard to the extension of the metric and decimal system to the purposes of general commerce, he agreed with the author that the full benefit of such a step could only be reaped in conjunction with a compulsory system of decimal coinage; but in the face of the competition of over one hundred and eighty millions of European producers, of whom one hundred and twenty millions either held or were hungering after Colonial possession as outlets for their produce, or as standpoints from which to assail the mercantile supremacy of England, all using one standard of measure, differing from, and simpler than, the British; he thought it would be wiser, if also the mercantile world, or so much of it as would have to withstand the attack of foreign competition, were to place itself on an equal footing with its competitors by adopting, as early as possible, their standard of mensuration.

The course of dissemination, before its adoption became compulsory, was sketched by the author in his reference to the method by which the change was effected in Austro-Hungary. He thought, however, his suggestions might have gone further, and that part of the burden laid on engineers might have been transferred to the shoulders of the schoolmaster. It was of no use to familiarize the workmen with a new and simpler system of measurement and calculation, if their children, who, in all human probability, were destined to take their place, were brought up in an old and complicated one. In conclusion, he agreed with the author that practically the decimalization of the coinage was as simple, if not easier, than the conversion of weights and measures for commerce. This had been aptly illustrated in Germany, where one universal standard of monetary value had been introduced in place of the heterogeneous coinages of her numerous petty States; and in Austria, where the florin, formerly composed of 60 kreuzers, now contained 100. Yet, in both countries no other change than that of nomenclature had taken place in measuring many articles of daily use. For instance, the old Krügel (0.531 liter) had become the modern $\frac{1}{2}$ liter; the old Seitel (0.353 liter) $\frac{1}{3}$ liter; the Pfiff (0.177 liter) $\frac{1}{6}$ liter. Meat and other commodities, formerly sold by the pound, were now computed by the $\frac{1}{2}$ kilogram, and so on, with, however, be it remarked, almost invariably a slight loss to the public, and a corresponding advantage to the tradesman.

MECHANICAL INTEGRATORS.

By Professor Henry Selby Hele Shaw, Assoc. M. Inst. C. E.

Proceedings of the Institution of Civil Engineers.

I.

All measurements are made in terms of some fixed unit. The method may consist of a simple comparison of the unit with the quantity to be measured, but when this cannot conveniently be done, some indirect means must be employed. Indirect measurements may be made by measuring some physical effect, the magnitude of which is known to be a function of the quantity to be measured, as, for instance, when the length of a rod or wire is estimated by its weight. Where, however, the unit, in terms of which the measurement has to be made, is what is known as a derived unit, the indirect method generally consists in measuring in terms of the simple units from which the former is derived, and performing, with the results, the necessary calculation. An example of this latter method

is given in obtaining the contents of an area, by taking its length and breadth and multiplying them together, instead of adopting the tedious process of ascertaining, by direct comparison, how many times the unit of area would be contained within it.

Now, such calculations, even when of so simple a kind as mere multiplication, often become very inconvenient, and a large number of instruments have been designed for performing them by mechanical means. Such instruments may be divided into two classes, one in which the final result of conditions which vary in an arbitrary manner is found, such as the contents of a surface or the work of a motor, requiring a process of multiplication or addition; the other in which the relation or ratio, at any instant, of two such quantities is given, such as space and time in the case of velocity, requiring at each instant a process of division. The object of the present paper is to deal with the theory, design and practical applications to engineering problems of the former class alone. It may be briefly stated that very little has yet been practically done in the use of the latter. Quite recently, Professor A. W. Harlacher, of Prague, has published an account of the instruments and methods of Harlacher, Henneberg and Smreker, for gauging the velocity of a river current, the principle of which is the same as that independently adopted by the author and others in this country.

The conditions or data above referred to, from which the required result has to be calculated by instrumental means, are obtained in two ways:

(1) By intermittent or separate observations and measurements.

(2) By the continuous motion of a machine in connection with self-recording apparatus.

The former is the case in measuring an area of country, taking dimensions of a river or embankment section, or obtaining the forces exerted at different times by a machine or body in motion. The latter is generally given in the form of a graphic record, an important example of which is the diagram of energy or work taken from a prime mover. In both cases the result, whatever it be, whether boundary, area, volume, work, &c., can be found by calculation, but only with an approximation to the truth, depending upon the extent of the calculation. The reason of this is that the data of calculation, which are taken directly in the first case, or selected from the graphic record in the second, only represent actual conditions more or less closely, according as the number of data so taken is greater or less, and the greater the number the greater is the labor of determining the result. The instruments discussed in this paper perform such work mechanically, with the great advantages of rapidity of operation, accuracy of results, and without requiring mental effort on the part of the manipulator; and all this, moreover, to a great extent independently of the complexity of the calculation required. All the results, the measurement of which will be considered, can be represented graphically. If the observations have been made separately, they can be plotted, whether in the form of a diagram of energy, or on the plan or elevation of an area or section, and the boundary can be filled in with a tolerably close approximation to accuracy. In the other case the graphic record is, or may be, directly given. The subject, as far as the theory of the calculation goes, can therefore be studied with reference to such diagrams without the necessity of considering in the first case how they were obtained, and it will be convenient to do this, and afterwards to examine separately various examples of their application. Such diagrams may be drawn upon any kind of surface, and an instrument for dealing with measurements upon that of a sphere will be hereafter described. A plane surface may, however, be employed upon which to represent all cases of any practical importance, and the question thus arises, What are the measurements of the nature under consideration which are required, and which can be obtained from either a regular or an irregular plane of figure?

Such measurements are of three kinds:

(1) The length of its perimeter or boundary.

(2) The area of its superficial contents.

(3) Its relation to some point, line, or other figure on the surface, *e. g.*, its moment of area or moment of inertia about a given line.

All these three kinds of quantities can be ascertained by successive operations of addition. The first requires the addition of elements of length, the second may be obtained by adding up successive elements in the form of strips of area, and the third by adding products obtained by multiplying such strips by some quantity, the magnitude of which depends upon the position of the other point, line, or figure in question. Taking the general case of an irregular figure, it is evident that absolute accuracy can only be obtained when this operation becomes that of integration or summing up of an infinite series of indefinitely small quantities. Instruments for performing this operation are therefore called "mechanical integrators." In all such instruments the rolling action of two surfaces in frictional contact is employed, for this, as will be hereafter seen, enables the conditions of motion to be continuously varied in a way which could not be effected by mere trains of wheel-work, such as form the mechanism of some kinds of calculating machines. This fact necessitates something more than a mere discussion of the mathematical principles upon which the calculations are performed, for though the action of an integrator may be absolutely correct as far as its theory of the performance of the calculation is concerned, yet there is always some instrumental error depending upon the rolling, and also, as will be seen, of the slipping of the two surfaces in frictional contact. This error may be exceedingly small, but it is a matter of great importance to ascertain its exact amount, and the subject will therefore be investigated at length, under the heading "Limits of Accuracy of Integrators," where an account will be also given of the experimental results of Professor Lorber, of Leoben; Dr. William Tinter, of Vienna, and Dr. A. Amsler, of Schaffhausen. In this investigation it will be shown that when integrators are examined upon the mechanical principles of action, they are all found to belong to one of two classes.

(1) In which the surfaces in question slip over each other.

(2) In which only pure rolling motion of the surfaces is assumed to take place.

The significance of this mode of classification is that it not only leads to a clear understanding of the nature of the results to be expected from any particular instrument, and teaches the best method of manipulating it, with regard to its position relatively to the figure to be measured, but it also brings out prominently the mechanical principle upon which the inventor has relied sometimes, as it would appear, unconsciously, for the accuracy of the results expected to be obtained.

It may be here remarked that the same principle, by which an integrator is employed to determine a result from an autographic record, may be applied directly to obtain a continuous result from the machine or body in motion, such, for instance, as an ordinary dynamometer or dynamo-electric machine, from which the autographic record was obtained. Thus, after discussing the action of integrators for dealing with diagrams, it will only be necessary to consider the mechanical details of the instruments for direct application to a machine, and this will be done under the head of "Continuous Integrators."

Coming now to the consideration of the actual measurement of the three kinds of quantities, it will be found that the first is very simple, and, in fact, the only reason why it is not convenient to measure it, by comparing it with the unit of length in the ordinary way, is its continuous change of direction.

The only mechanical method of rectifying a curve, as it is called, that is, obtaining its length as a right line, is by rolling a wheel along it. This wheel is connected with a suitable train of wheels for recording the total number of revolutions, and as the rolling circle of the first wheel is either a unit in length or contains a known relation to this unit, the length of the curve or boundary is given at once by the reading of the graduated wheels. The use of such instruments is very ancient, and Beckmann, in his "History of Inventions," describes amongst various odometers, one mentioned in the Tenth Book of Vitruvius. Such an instrument is made upon a small scale for use by a draftsman, and in one form it is sometimes termed an "opisometer," in another form the chartometer, or Wealemefna; it is also employed upon a large scale as a road or route measurer. The same principle has been em-

ployed in one of the latest forms of anemometer, in which the plane of the wheel is always kept coincident with the direction of the wind, while its edge rolls in contact with the recording surface, and measures the total travel of the wind. In this class falls the device suggested in a letter to "Nature," by Mr. V. Ventosa, of Madrid, for continuously obtaining the N., E., S. and W. components of the wind, a device which was independently arrived at by the author in connection with certain mathematical principles referred to hereafter.

The object of employing a rolling wheel is merely to enable the direction to be changed so as always to coincide with the curve to be measured, and the principle is, therefore, that of direct unit measurement or comparison. The foregoing instrument, which evidently belongs to the second class in which only rolling motion, without slipping, is supposed to take place, forms, however, one kind of mechanical integrator.

The measurement of the other two kinds of quantities is, as in the case of the first, a problem of addition; but, instead of being the addition of an infinite number of infinitely short lines, an infinite number of continually changing magnitudes has to be added. This is the same both for instruments required in the second kind of measurement, or "Area Planimeters," and those required in the third kind, or "Moment Planimeters." From the fact that the changing magnitudes referred to are simpler to deal with in the case of calculating the contents of areas than in finding their moments or any mathematical results of an equivalent kind, the discussion of the theory of the two above kinds of instruments is not the same, and will, therefore, be dealt with under two separate headings.

Area Planimeters.

The area of any plane figure, such as ABDE (Fig. 1), can be obtained in the following manner. Take any straight lines, OX and OY, at right angles to each other, and parallel to OY; draw a series of straight lines equidistant from each other, dividing the figure into a number of strips, or elements of area. A series of rectangles may then be found, as shown at AB, the area of which is equal to that of the corresponding strip, so that, by adding the rectangles together, the area of the whole figure is obtained, as adopted in the common method of finding the area of an indicator diagram. The greater the number of strips the more closely will the height of the two sides of each tend to become equal to each other, and to the height of the corresponding rectangle.

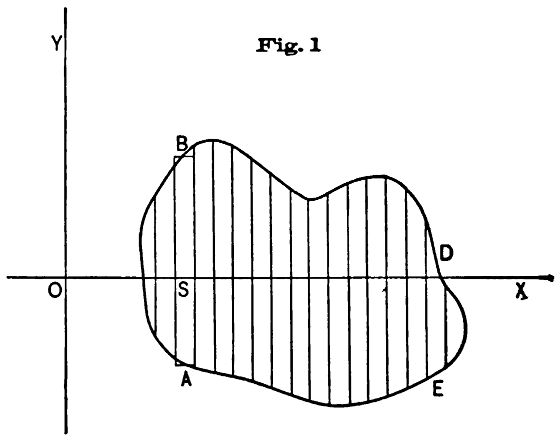
Let Δx = the width of any element of area such as AB, at a distance x from O.

y = height of the corresponding rectangle.

Then $y \Delta x$ = area of the element AB,

and the sum of all such elements of area is the area of the figure ABDE. When

Fig. 1



the number of elements is increased indefinitely, this expression becomes

$$\int_a^b y dx = \text{area of the figure ABDE},$$

a and b being the extreme values of x, *i.e.*, the limits of integration.

It is evident, therefore, that the area will be correctly measured by an instrument in which the recording wheel or measuring roller always turns at a rate proportional to the ordinate y of the curve, while the body from which it derives its motion moves at a uniform rate along the axis OX.

Area planimeters have been classified according to apparently different modes by which the operation of integration is performed; but, since the action of them all can be explained upon the foregoing principle of adding elements of area, and,

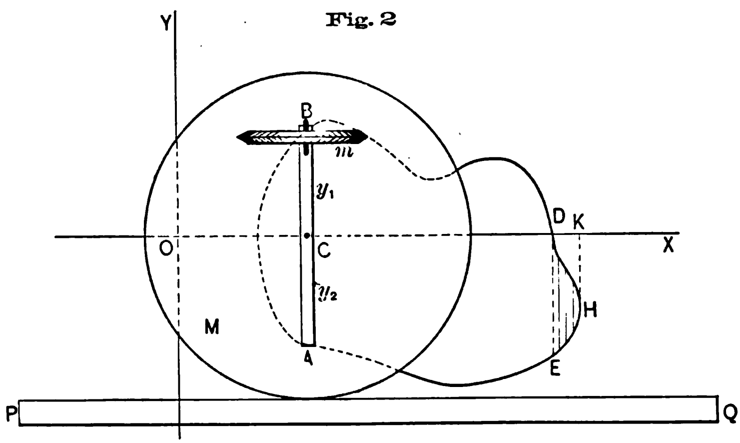
in fact, by means of the same notation, it is not surprising that such classifications are anything but satisfactory. In fact, in one important kind of planimeters, it becomes doubtful to which class they belong, or, whether they should not be placed in two or more classes. It is, without doubt, very convenient to distinguish different planimeters, and, therefore, the names which have been given them will be used; but this will not denote any difference of principle, and the classification which will be adopted is that already explained, and depends on mechanical conditions of action. In what follows, one mode of viewing the mathematical operation is adhered to throughout, and it may be stated that the object of the author has been to make clear the principles of action of integrators, rather than to obtain rigid and exhaustive demonstrations of their theory.

Planimeters in which Slipping of the Measuring Roller Takes Place.

From a brief account of the subject by Professor Lorber, it appears that the first recorded idea of a planimeter is attributed to Hermann, of Munich, who worked it out with Lämmle. This idea of Hermann's, which was published in 1814, seems to have fallen into oblivion, for in 1827 Oppikofer, of Berne, constructed a planimeter upon similar principles, and it was thenceforth called after his name. On the other hand, Favora gives the priority to Professor T. Gonella, of Florence, who, in 1828, without any knowledge of what Hermann had done, invented and described a very similar instrument. The development of the planimeter seems to have grown out of the instrument of Oppikofer, who, in conjunction with a Swiss mechanic, Ernst, finished a planimeter which won a prize, in Paris, in 1836. Important improvements are due to Wettli, of Zurich, who, with Starke, in 1849, took out a patent in Austria for the instruments now called the Wettli-Starke planimeter. Later on, in England, other inventors (Sang, Moseley) worked at the subject, but all these instruments depended for their action on the same principle, which is as follows:

Let M (Fig. 2) be the plan of a disk rolling in contact with a straight guide PQ, which is parallel to OX, and at a distance from it equal to the radius of the disk, so that the plan of the center of the latter always lies in OX. Let m be a roller upon the surface of the disk, graduated and connected with wheel-work and an index, so that the distance turned through over the surface of the disk can be read in revolutions, or parts of a revolution. The plan of the point of contact (B) of the roller with the disk is always made to coincide with that particular point on the curve, which is in the line drawn at right angles to OX, through the center C of the disk. The plane of rotation of (m) which may be called the measuring roller, is always perpendicular to the disk M, and the plan of its axis, as shown in the figure, is always parallel to OY, so that, in following the curve, it slips backwards or forwards across the surface of the disk, in a direction parallel

Fig. 2



to OY. Suppose the disk to roll along PQ for a distance Δx, equal to the width of the element AB.

Then, if y_1=distance of B from OX.
R=radius of disk M.
r=radius of measuring roller m.

n_1=consequent reading of measuring roller for this travel of disk.

Then $\frac{y_1}{R}\Delta x$= linear distance turned through by a point on the disk at a distance y_1 from the center.

$2\pi r n_1$= linear distance turned through by a point on the circumference of m; but since m rolls on M these distances are equal.

Therefore $$2\pi r n_1=\frac{y_1}{R}\Delta x,$$

or, $$n_1=y_1\Delta x\times\frac{1}{2\pi r R};$$

but $\frac{1}{2\pi r R}$ is a constant, which, by taking r and R in suitable ratio may be made unity.

Then $$n_1=y_1\Delta x,$$

that is, the reading of the roller m measures that part of the area of the element above OX.

If the point of contact be made to follow round the curve continuously in one direction, then, when the portion of AB below OX is being measured, the disk is moving in the opposite direction along PQ, but, at the same time, the roller is turning in the opposite way relatively to the disk to that which it was doing before, since the point of contact is now below C. The final result of these two opposite motions is to cause the roller to turn, as at first, and so add the result given for CA to what was given for CB. If the motion of the disk Δx for the width of AB be now regarded as negative,

and $-y_2$=distance AC

also n_2=reading of roller for this element of area,

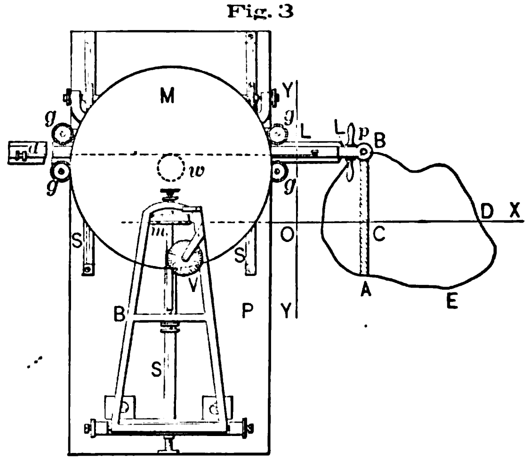
then by similar reasoning to that already used,

$$n_2=(-y_2)(-\Delta x)$$
$$=y_2\Delta x,$$
and $$n=n_1+n_2=(y_1+y_2)\Delta x=y\Delta x=$$
$$=\text{area of element AB.}$$

This reasoning holds for any possible position of the roller, or of the axis OX, which may be altogether outside the figure, as it practically is for the integration of the portion DHE. Then it will be found that DKH is subtracted, and DKHE is added, so as to give the required actual area DHE.

Inasmuch as this reasoning is independent of the actual value of the width of the element, and as the vertical motion of the roller m has no effect in theory upon the distance rolled through by it, therefore, in the limit when Δx becomes

Fig. 3



infinitely small, the actual value of the series of infinitely narrow strips which compose the figure ABDE is given by the final reading of the roller when the traverse of the boundary is completed.

The Wettli-Starke planimeter (Fig. 3) acts directly upon this principle, with the exception that it is the disk that is moved according to the changes in y instead of the measuring roller, and the following is a description of the best form of this instrument:

On a base-plate P (Fig. 3), are three guides SSS, along which a frame carrying the vertical axis of the disk M can be moved to and fro. The disk, which is made of glass and covered with paper, has two motions, one rectilineal along the guides, and one of rotation about its axis. The motions are imparted to it by

means of an arm (L), which passes through the roller-guides (gg) in the frame carrying the disk, and rotates the latter by means of a German silver wire (dd) passing round a cylinder w upon its axis, and attached by the two ends to the extremities of the arm. The measuring roller (m) rests upon the surface of the disk, being carried in another frame (B), which is hinged to the base-plate. The action is as follows: the base-plate being placed in juxtaposition to the figure to be integrated, any line parallel to the guides, *i.e.*, to the direction of rectilinear motion of the disk, may be taken as the axis OY; and line OX, drawn through the edge of the roller, perpendicular to OY, may be taken as the other axis. Then, as the pointer p at the extremity of the arm is made to pass round the boundary of the figure, the disk will be turned through a distance proportional to the travel along OX, while at any instant the roller (m) is at the same distance from the center of the disk as the pointer is from OX.

If y_1=CB=mean height of element Δx= =width of element AB.

Then, by the reasoning already given, the reading of the roller which the pointer passes over the upper boundary of the element AB, is

$$n_1 = y_1 \Delta x,$$

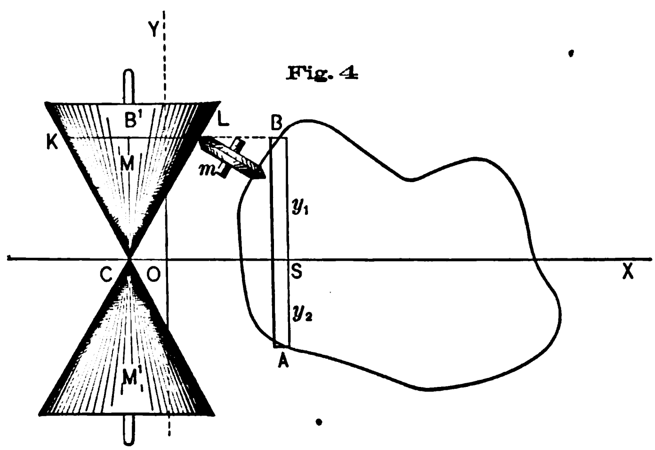
and the final reading of the roller is

N=area of the figure ABDE.

Hansen, in 1850, still further improved this instrument, and, in conjunction with Ausfeld, introduced a different method of reading the result, and of carrying the frame, this instrument being known as the Hansen-Ausfeld planimeter. Various other instruments of the same kind were shown in the Great Exhibition of 1851, but in all, the motion of the arm carrying the pointer was "linear;" that is, the motion, which must be possible in every direction, is obtained by compounding two rectilinear movements, at right angles to each other. Such instruments are therefore called "linear planimeters."

Many different forms of linear planimeters have been suggested, but the only modification of the disk and roller which it will be worth while to notice is the cone and roller.

Fig. 4



Let MM′, Fig. 4, be the cone corresponding to the disk M, and rolling on the edge of its two bases in a direction parallel to OX. Let the roller m always be in contact with a circle on the cone, whose center B′ is at a distance CB′ from the apex C of the cone, such that

CB′=SB=y=mean ordinate of element SB.

where the element AB is being at that instant integrated. Adopting the same notation as before, when the cone has rolled over the surface through a distance Δx, then, whatever be the angle of its apex, the distance rolled through by the roller m is

$$2\pi r n_1 = \frac{y_1}{R} \Delta x,$$

or

$$n_1 = y_1 \Delta x \times \frac{1}{2\pi r R}.$$

As might have been anticipated, the expression is the same as was obtained in the case of the disk, the latter being a special case of the cone when the vertical angle is 180°.

Thus the cone may be employed instead of the disk, and such an instrument was invented by Mr. E. Sang, who, in 1852, published a description of it, according to which the action was extremely accurate, but it does not appear to have come into very extensive use.

No more instruments of the kind will be described, since they have given place to those in which the arm carrying the pointer turns about a center or pole, and which are, therefore, called "polar planimeters."

In the year 1856, Professor Amsler-Laffon invented and brought before the world the now well-known polar planimeter bearing his name, and, since then, no less than twelve thousand four hundred of these instruments have been made and sent out from his works at Schaffhausen. According to authorities, which Professor Lorber quotes, Professor Miller, of Leoben, invented independently a planimeter of this kind in the same year (1856), which, being made by Starke, of Vienna, is known as the Miller-Starke planimeter. Previous to this, in 1854, Decher, of Augsberg, as well as Bouniakovsky, of St. Petersburg (1855), had improved upon previously-existing forms of polar planimeter, though it is well to note that the planimeters already mentioned as sent to the Great Exhibition of 1851 from various parts of Europe, as Italy, Switzerland, France, and Prussia, were all linear, and no mention is made of polar planimeters in the jurors' report.

The Amsler planimeter is shown in Fig. 5. It consists of two bars, (*a*) the radius bar, and (*b*) the pole-arm, jointed at the point C. The tracing point *p*, which now coincides with the point B of the figure ABDE, is carried round the curve, and the roller *m*, which partly rolls and partly slips, gives the area of the figure; and by means of the graduated dial *h*, and the vernier *v*, in connection with the roller *m*, the result is given correctly in four figures. The sleeve H can be placed in different positions along the pole-arm *b*, and fixed by a screw *s*, so as to give readings in different required units. A weight at W is placed upon the bar to keep the needle-point in its place, but in instruments by some other makers T is a pivot in a much larger weight, which rests on the paper. A recent minor improvement has been to fix a locking spring to the frame, so that the roller can be held when the planimeter is raised for the purpose of reading it.

The theory of the polar planimeter may be simply deduced from that of the disk and roller thus:

Let Fig. 6 represent the same disposition of the disk M with regard to the

Fig. 5

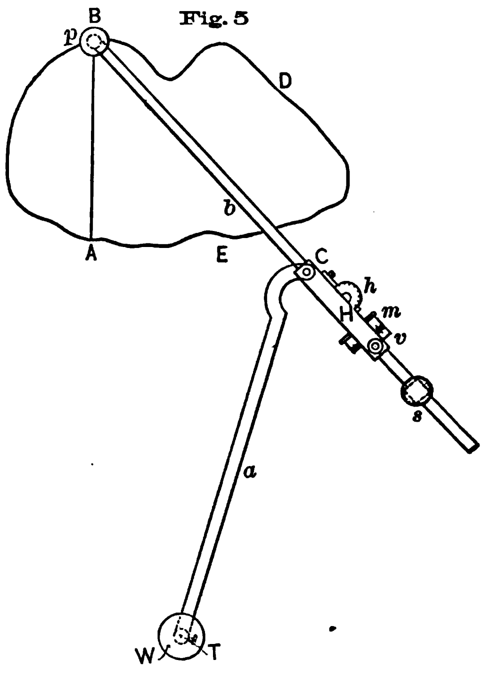


figure ABDE, as in Fig. 2. but now let the roller *m* move round the edge of the disk instead of across it, its distance from OX being always the same as before, viz.:

$$Oq = SB = y_1.$$

The turning of *m* for a given travel, Δx, of the disk is found thus—draw *lq* (Fig. 6A) tangent to the disk at *m*, so that

$$lq = \Delta x,$$

and draw *lk* parallel to the axis of rotation of *m*, then *qk* is the distance turned through, and *lk* is that slipped through by the edge of the roller *m*, when the disk has rolled through a distance Δx;

therefore, using the same notation as before,

$$qk=2\pi rn_1$$

and $$\frac{qk}{lq}=\frac{2\pi rn_1}{\Delta x}=\sin\angle qlk;$$

but in Fig. 6 $$\frac{Oq}{Sq}=\frac{y_1}{R}=\sin OSq.$$

But by similar triangles

$$\angle qlk=\angle OSq=\alpha;$$

parallel to OX. The turning of m thus measures the area of the element as long as y does not change. If, however, the value of y changes so that m changes its distance from OX, the measuring roller is likewise turned a certain additional amount from this cause; but this does not affect the correct reading of the area so long as its first and last positions are equally distant from OX. The reason is, that then the roller has turned as much in one direction in moving away

Fig. 6A

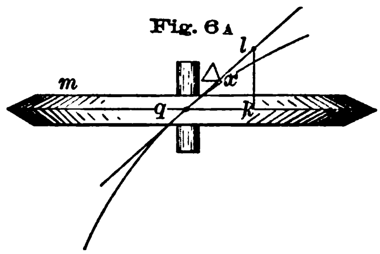
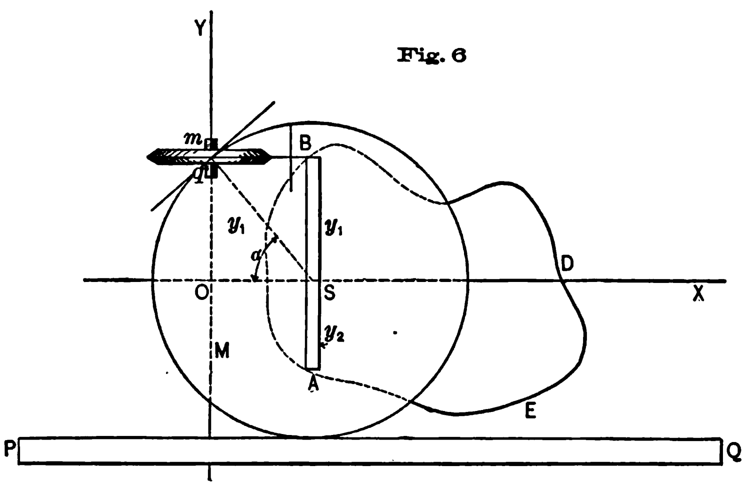


Fig. 6



Therefore $$\frac{2\pi rn_1}{\Delta x}=\frac{y_1}{R},$$

or $$n_1=y_1\Delta x\times\frac{1}{2\pi rR}.$$

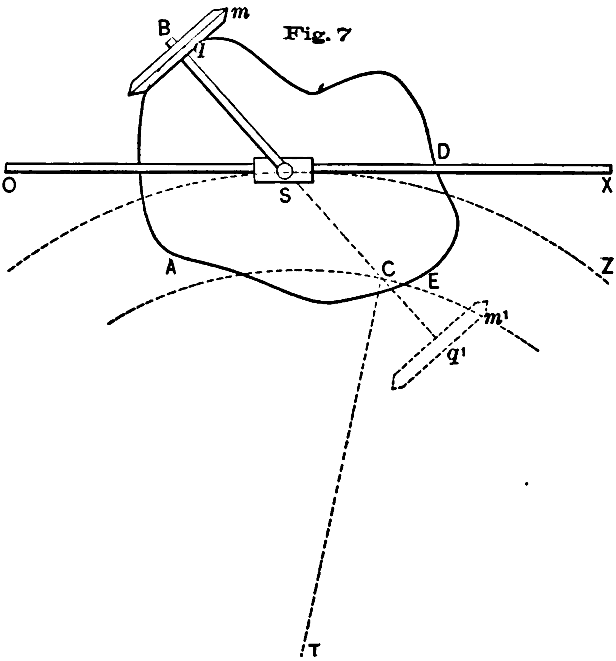
This is the same result as previously obtained, and it has been given in this way because there is an important class of planimeters to be hereafter described, combining the polar planimeter with the disk and roller, in which a principle is employed which is thus made obvious. This principle is that the turning of m is exactly the same as if it were in contact at the point B, no matter in what position it may be along the line through B from OX as it has in moving towards it, and this is the case for the initial and final positions of the pointer when the complete travel of the closed curve has been made. Now, inasmuch as the velocity of the edge of the disk is just the velocity at which the center has been shown to move along OX, the disk may be removed altogether. The roller is then moved in contact with the surface of the figure and with identically the same amount of turning as before, provided that its plane of rotation is turned so as to make the same angle with OX (which is now its direction of translation), as it did before with the direction

of motion of the edge of the disk in contact with it. This is the case when it is turned through that angle, and then its axis of revolution coincides with the radius Sq.

In order to keep the direction of the plane of rotation always at right angles, it is only necessary to have a rod or bar Sq capable of turning on a pin at the point S. The pin at S is attached to a sleeve, which can freely move along a guide-bar, whose direction coincides with the axis OX. By employing the bar qSq^1 itself as the axis of rotation on which m turns, the simple planimeter shown in Fig. 7 is obtained, in which the point of contact of the measuring roller is made to pass around the diagram. The turning of the roller m correctly gives the superficial contents. The roller can be moved to any position on the rod, such as shown in dotted lines, Fig. 7, without in any way affecting its resultant turning, and the former point of contact of the roller is replaced by a pointer, which is made to follow the curve instead of the roller. Professor Burkett Webb has described to the author a planimeter of this form used in the United States, known as "Coffin's" planimeter.

If, finally, the point S, instead of moving along the straight line OX, which may be considered as a portion of a circle with its center infinitely distant, moves along the arc SZ (Fig 7), or any other arc, as, for instance, that with radius TC, the instrument becomes the ordinary Amsler planimeter (shown in dotted lines). This explanation, so far, is based upon that given by Sir Frederick Bramwell, who has further shown that the change from the motion in a straight path to that in the arc of a circle has no effect upon the ultimate reading when the complete travel around the closed curve has been made, and the arm SB has returned to initial position. The following demonstration of the truth of this appears, however, to have an advantage, in that it follows throughout the operation of integration, especially as recent planimeters are more complicated than that of Amsler.

Fig. 7



1st. Let point S (Fig. 8), at the extremity of the radius rod, move along the broken line STUVWXYZ, and from these points draw arcs with a common radius=R, cutting the curve in points t_1t_2, u_1u_2, v_1v_2, &c., s and z being tangent points at the end of the curve from the points S and Z. The proof has already been given that the integration of the

complete portion st_1t_2, taken separately, is given by the reading of the measuring roller; so also are given the areas of the various other portions, $t_1u_1u_2t_2$, &c. If the separate portions were integrated consecutively, any arc, such as t_1t_2, would be traversed in both directions by the measuring roller, because it would move one way around in traversing one figure and the apposite way in going around the adjacent one, and the reading due to the arc would be eliminated.

Thus the whole curve may be integrated correctly at once without going round each separate portion formed as above, even if the point S at the end of AB moves upon a broken line instead of along OX. Next, substitute a continuous curve TUVXYZ (Fig. 8A) for the broken line. This curve may be supposed to consist of an infinite number of straight portions. The infinitely small portions contained between an arc, as u_1u_2, and another very close to it, drawn from the beginning and end of these straight portions, may, just as in the case of the broken line, be supposed to be integrated separately and with a correct result, which is independent even of a possible crossing of the arcs, as v_1v_2 and u_1u_2. In the same way as before, it may be seen that the arcs u_1u_2, etc., need not be traversed and, so long as the point S returns to its initial position, the area of the figure is given by the simple traverse of its boundary, whatever be the curve on which the point S moves, which, in the case of the Amsler planimeter, is a circular arc.

Various writers have explained the action of the simple polar planimeter in ways more or less different. One of these ways, recently given by Mr. F. Brooks, of Lowell, U. S., may be alluded to. He shows that the area may be treated as the difference between the area swept out by the line Tp (Fig. 9) and the sector of the zero circle, or circle upon whose circumference the pointer (p) being moved, the measuring roller is not in consequence turned. This is true, both for the outside or inside, if proper signs be taken. Let the element of area pq be passed over, the curve at p being for a small distance considered concentric with the zero circle, this small area subtending an angle w at the center; then, if values be taken as shown upon Fig. 9, in which

CT = radius-bar = a;

Cp = one portion of pole-arm = b;

Cm = the other portion of pole-arm = c;

Area $pq = \frac{1}{2}w\,(a^2+b^2+2ab\cos\theta) - \frac{1}{2}w\,(a^2+b^2+2cb) = wb\,(a\cos\theta - c)$,

Fig. 8

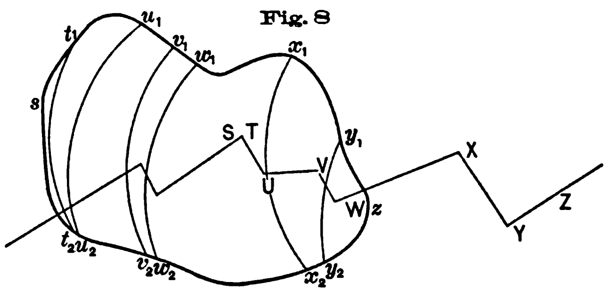
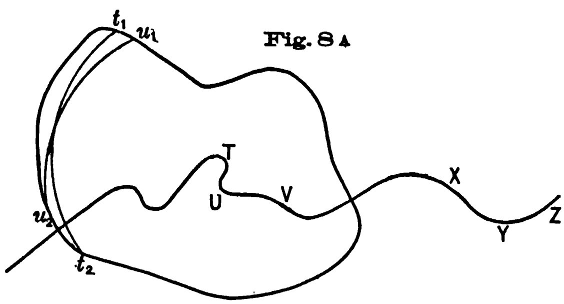


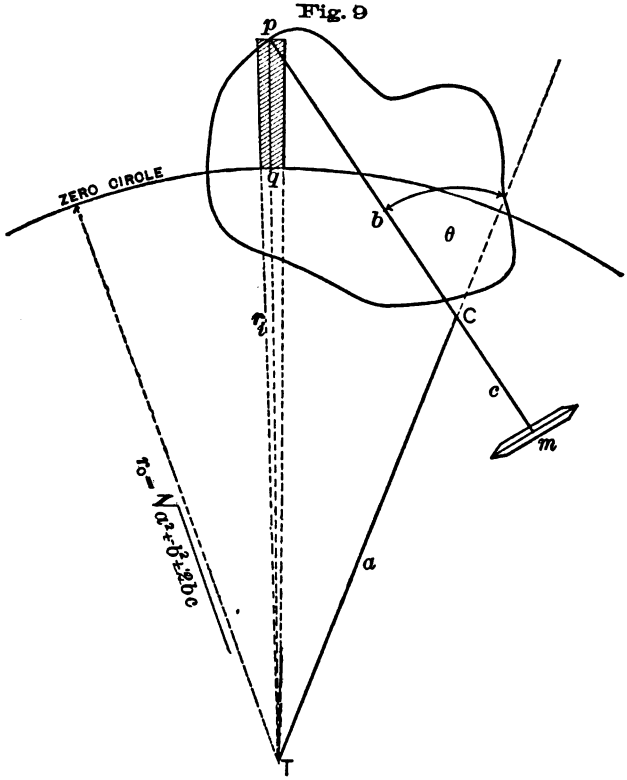
Fig. 8A



and, by a geometrical construction, the travel of the measuring roller is easily shown to be equal to the same expression. Mr. Brooks also explains why the area of the zero circle must be added to the reading if the figure to be integrated contains the center T. The following appears, however, to be a still simpler explanation. Referring to Fig. 7, it is evident that going around the outside of the zero circle corresponds to a movement taken continuously above the zero line OX when only the portion above OX is measured. In this latter case the curve could never be completely traversed as long as the pointer moves in one direction. Suppose, however, that the ends of OX are bent round and brought within the figure, then the motion in one direction will enable the complete circuit to be made; but only the portion outside the line, *i. e.*, corresponding to that originally above OX, will be measured by the roller, and that within must consequently be added to the recorded result. This quantity is evidently the area of the zero circle in the case of the Amsler planimeter, which must, therefore, always be added to the result when the center of the radius-arm is within the diagram to be measured.

As the Amsler planimeter alone, so far as the author is aware, has been modified to measure the area of any non-developable surface, this modification may be here noticed. The only surface of the kind to which it has been adapted is a spherical one. Fig. 10 shows the instrument, and from that figure it will be seen that the chief alteration is the placing of two joints j j', one upon the radius-bar (a), and the other upon the pole-arm b, so as to allow the employment of the integrator for surfaces of varying curvature. The joints are equidistant from the end of each bar, and exactly opposite to each other—the radius-bar and pole-arm being now of equal length, and a pin f is placed on (a), which fits into a corresponding recess in

Fig. 9



(*b*), so that when the two arms are closed, they can be together bent at the joints to the required amount, and, the joints being purposely made stiff, they will remain at the proper angle when the instrument is used. The joint (*j*) acts so that the tracing point (*p*) is always in the place of the axis of rotation of the measuring roller. The theory of the action of this instrument has been fully explained by Professor Amsler, in an article in which the theory of the relations between measurement upon a spherical surface and upon a plane surface is discussed.

S. This setting causes the reading of the instrument, when the diagram is traversed by the pointer in the usual way, to give at once the mean height of the diagram in fortieths of an inch. The simple relation is as follows:

$$\frac{\text{Reading of measuring-roller}}{40} = \text{Mean height of diagram in inches.}$$

$$\text{Mean pressure} = \text{Mean height} \times \text{vertical scale of diagram.}$$

As an instance of the great saving of labor by the use of the Amsler planimeter,

Fig. 10

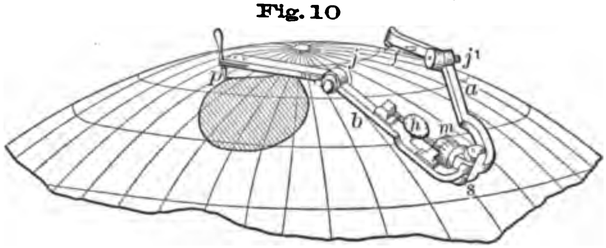
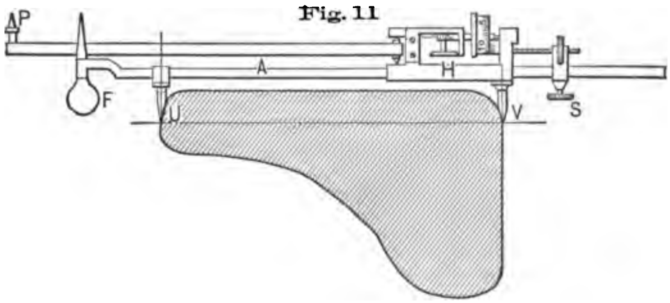


Fig. 11



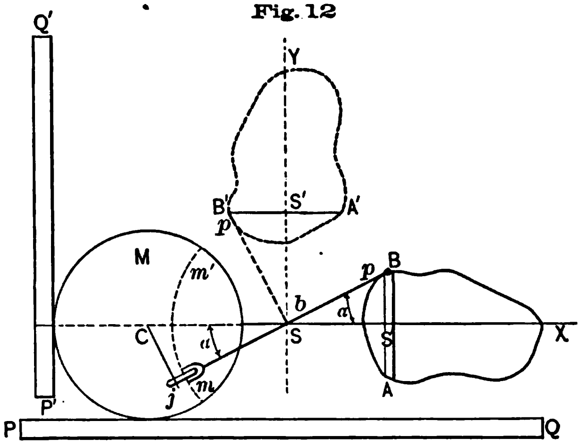
The various applications of the simple planimeter for finding areas are well known, and need not be explained; but there are some slight modifications of the instrument for special purposes, and one of these recently applied by Professor Amsler to his planimeter is worth noticing. This is illustrated at Fig. 11, and is a device for reading at once the mean pressure given from an indicator diagram. Two points, U and V are seen, one (U) being upon the upper side of the bar A, which slides in the tube H, and one (V) upon the tube H itself. These points can be adjusted to the length of the diagram by inverting the instrument in the way shown in the figure, and the sliding-bar is then clamped by the screw the author happens to know a civil engineer's office, where a large amount of earthwork quantities had to be taken out, the calculations proceeded slowly and with many repetitions, until one of the draftsmen procured a planimeter, and then the other, with the result of a great expedition of the work, and almost the complete absence of errors—and even then only in decimal places—where previously the divergence had been as much as by units.

Although the connection between the disk and roller or linear planimeter and the polar planimeter has been shown, it is possible to regard them as acting upon different principles. The former may be considered as measuring the variation in

the ordinate (y) by a change of effective radius of the circle on which the measuring-roller works, the latter measuring the same thing by a corresponding change in the sine of the angle which its plane of rotation makes, with its direction of motion over the surface on which it rolls. They have, in fact, been classified in this way as radius machines, and sine or cosine machines, for the slipping, although occurring in both, appears in the ordinary way of viewing the subject to affect the result in different ways. In the former, slipping is supposed to be entirely due to the variation in the value of (y), and only takes place when the ordinate changes in value; in the latter, the change is supposed to be effected by turning the pole-arm about its center, without any slipping at all. This distinction is, however, quite an imaginary one, for it will be seen that if the curve be obliquely inclined in either case to the axis OX, the action of the measuring roller is precisely the same. Recently, a large number of what are called "Precision Polar Planimeters," have been designed and constructed, which combine in an obvious manner the above two principles of action, the disk giving motion to the roller, while the pole-arm carries it across the disk in an oblique direction. Thus, the advantages of a uniform and invariable surface of contact for the roller, and the convenience of the polar planimeter are combined, with the still more important advantage of a large relative turning of the measuring roller. Before describing a few different forms of the best of these instruments, the general theory upon which they work will be given; it will then not be difficult to understand the action of the several instruments without repeating the explanation in each case. It will be found that both the linear and polar planimeter are only special cases of application of the general principle upon which the correctness of action of precision planimeters depends.

It will be well to approach the matter from the same point of view as in explaining the linear planimeter. Let the disk M, Fig. 12, rotate about an axis C as it rolls along the line PQ, parallel to OX, the pivot on the axle at C being attached to a frame which also carries another pivot S. This latter pivot always lies upon OX, and about it rotates a pole-arm b, carrying a pointer p at one end, and the measuring-roller m at the other end. The plane of rotation of the measuring-roller coincides with the direction of the pole-arm, and is carried over the disk in contact with it, along the arc mm'. Then from what was proved, p. 402, the motion of the roller m is exactly the same as if it were moved, so that its axis always coincides with Cj, the perpendicular upon the pole-arm from the center of the disk—provided only that its axis is always parallel to this line. Thus, adopting the previous notation, and taking

Fig. 12



Sp=length of upper portion of pole-arm=R.

Then when the disk rolled through a distance Δx,

n_1=reading of roller m

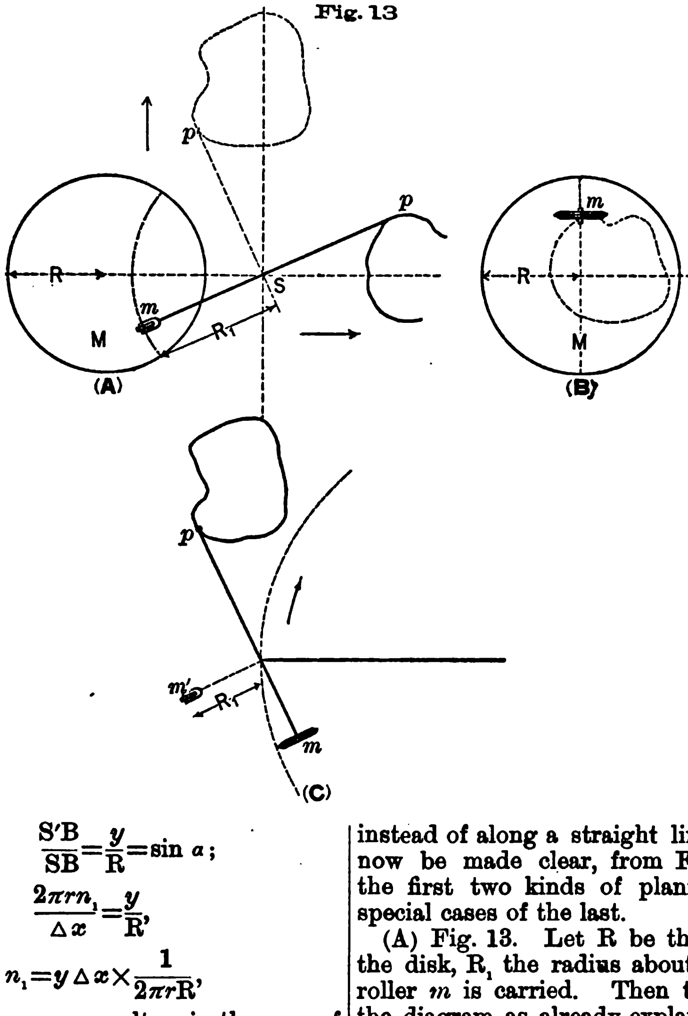
y_1=ordinate SB.

$$\frac{\text{turning of roller}}{\text{distance turned by edge}}=\frac{2\pi r n_1}{\Delta x}=$$

$$=\frac{\text{C}j}{\text{CS}}=\sin\alpha;$$

arm S must be taken parallel to the guide P′Q′, that is, perpendicular to the former direction. It has already been shown in the case of the Amsler planimeter, that it does not matter in what path the center S of the pole-arm is carried, so long as the foregoing conditions are observed, and thus there are several forms of precision polar planimeters in which the point S is carried in the arc of a circle

Fig. 13



but $$\frac{\text{S}'\text{B}}{\text{SB}}=\frac{y}{\text{R}}=\sin\alpha;$$

therefore $$\frac{2\pi r n_1}{\Delta x}=\frac{y}{\text{R}},$$

or $$n_1=y\,\Delta x\times\frac{1}{2\pi r\text{R}},$$

which is the same result as in the case of both the linear and polar planimeters. In practice, the portion of the pole-arm which carries the pointer is usually perpendicular to the other portion, as shown by the dotted lines, Fig. 12. In this case, the direction of motion of the disk and frame carrying the center of the pole-instead of along a straight line. It may now be made clear, from Fig. 13, that the first two kinds of planimeters are special cases of the last.

(A) Fig. 13. Let R be the radius of the disk, R_1 the radius about which the roller m is carried. Then the area of the diagram as already explained can be measured by either pole-arm Sp or Sp'.

(B) Fig. 13. Let the radius R_1 of the pole-arm become infinitely great, while R remains finite; thence m moves across the disk M in a straight line usually, but not necessarily, through the center, and the linear planimeter is the result.

(C) Fig. 13. Let the radius of the disk become infinitely great, and any motion of such an imaginary disk under these conditions would make the result equivalent to moving the roller over the surface of the paper. Therefore the disk may be removed, and the elementary form of polar planimeter is obtained, the roller being placed in either position as shown at m or m', without affecting the result.

The following is a simple explanation of the action of the precision polar planimeter:

Fig. 14

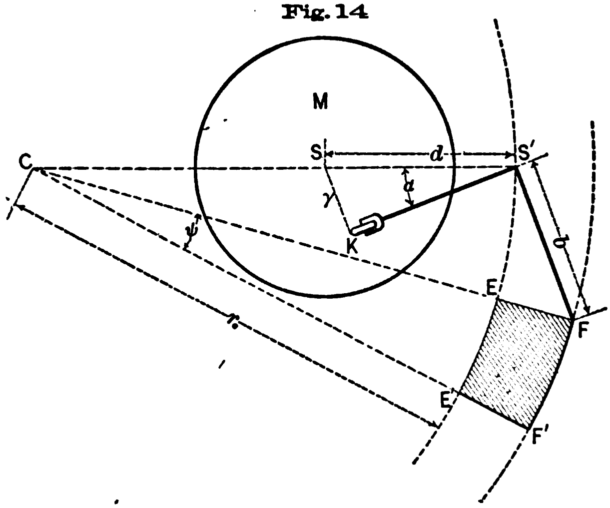


Fig. 15

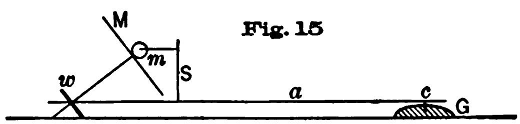
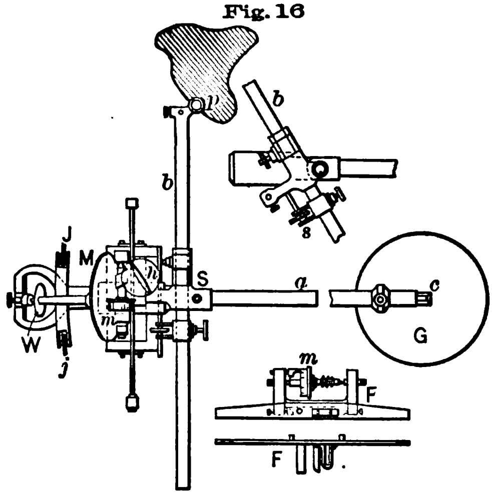


Fig. 16



Let M (Fig. 14) be the disk, which can be turned by any suitable means through a distance corresponding to the linear travel of its center about C.

Let r_0=radius of zero circle (E′ES′).
r=radius of any circle FF′.
α=$\angle$ turned through by pole-arm, when the pointer moves from the zero circle to the circle FF′.
ϕ=$\angle$ turned through by radius arm, CS, when an element EE′ F′F is being described.

where c and d are constant quantities; also if ρ equal the radius of the roller.

$$\text{then } \frac{y}{R}=\frac{\text{linear distance by edge of roller}}{\text{distance traveled by edge of disk}}$$
$$=\frac{2\pi\rho n_1}{r_0\phi c};$$
$$\therefore y=\frac{2\pi R\rho n_1}{r_0\phi c};$$

Fig. 17

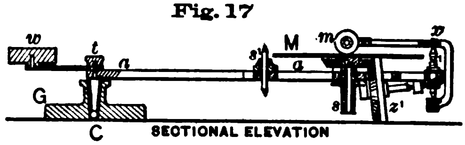


Fig. 18

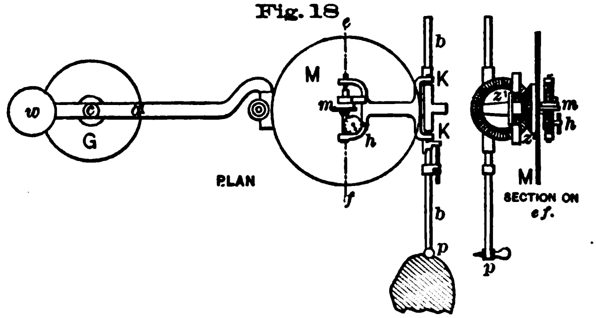
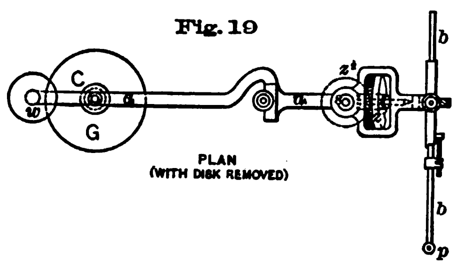


Fig. 19



a=radius arm=CS′.
b=pole-arm=FS′.
d=SS′.

Then from the figure—

$$\text{and}\qquad r_0^2=a^2+b^2$$
$$r^2=a^2+b^2-2ab\cos(90+\alpha)$$
$$=a^2+b^2+2ab\sin\alpha.$$
$$\text{Therefore } r^2=r_0^2+2ab\sin\alpha,$$
$$\text{or}\qquad \sin\alpha=\frac{r^2-r_0^2}{2ab}$$

Now the turning of the plate is proportional to ϕ, and may, for the arc FF′, be taken as equal to $r_0\phi c$.

$$y=\text{SK}=\text{SS}'\sin\angle\text{SS}'\text{K}=d\sin\alpha,$$

$$\text{Therefore}\frac{2\pi R\rho n_1}{r_0\phi c}=d\sin\alpha=\frac{d\times(r^2-r_0^2)}{2ab}$$

$$\text{or}\qquad n_1=\frac{r^2-r_0^2}{2}\phi\left(\frac{r_0cd}{2\pi R\rho ab}\right);$$

but $\frac{r_0cd}{2\pi R\rho ab}$ is a constant quantity, and may be made equal to unity.

Therefore $n_1=\frac{r^2-r_0^2}{2}\phi$=area of element EE′FF′.

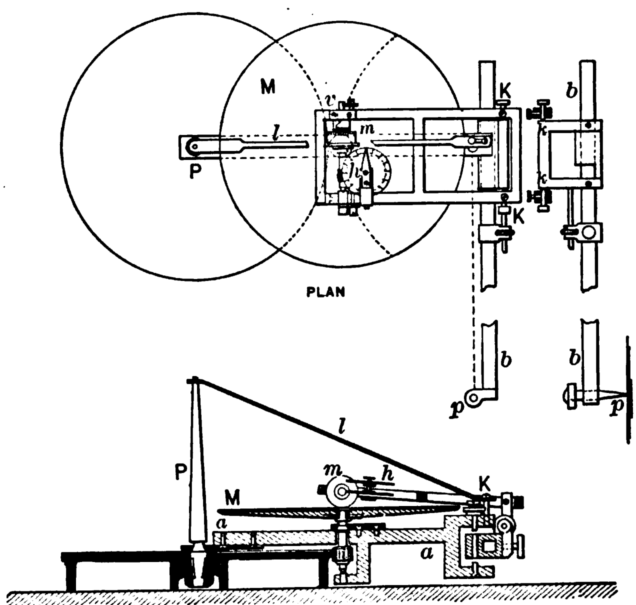
Thus n_1 is a measure of an element of area, and as the motion of m due to the turning of the pole-arm in moving to a

larger or smaller circle does not affect the reading when the pointer at F has passed round a closed curve, the final reading of the roller gives the area of any figure.

The actual construction of the precision polar planimeter appears to have been first carried out by Mr. Hohmann, Bauamtmann of Bamberg, in 1882, in conjunction with the well-known mechanician Mr. Coradi, of Zurich. A plan of the first instrument is given in Fig. 16; but it will be more easily understood by reference to the diagram, Fig. 15, which shows a frame (*a*) pivoted at one end (*c*) to a weight (G), about which it turns. This frame carries a small disk (*w*), which rolls in contact with the surface of the diagram, and gives motion to the disk M. The roller *m* is moved across the disk in the horizontal direction by a pole-arm centered on S as axis. Referring to Fig. 16, which is lettered in a similar way to Fig. 15, it will be seen how the pole-arm, in turning about the center S, effects this motion. A plan and elevation of the frame F, which carries the roller *m*, is shown on a larger scale, and this frame is moved backwards or forwards through a slot in the supporting frame. The roller *m* has the arrangement of the screw and worm for obtaining the readings of the dial *h*, as in the Amsler planimeter, and also the vernier in conjunction with the measuring roller. Two rollers, *j j'*, serve to balance the instrument. The details of the arrangement by which the length of the pole-arm *b* is adjusted are also shown on a larger scale.

In this instrument, the fact that the disk is inclined at an angle makes no difference in the theory of its action, and as the roller W obviously drives the disk so that the angular motion corresponds with the angular motion of the radius bar *a*, the explanation already given makes its mode of operation clear. The case is rather simplified by the fact that the roller *m* is moved radially across the disk.

Figs. 20 and 21



An instrument of similar kind has been designed and recently described by Professor Amsler-Laffon. This is shown in Figs. 17, 18 and 19, where it will be seen that this disk M, which is now horizontal, is turned by means of bevel-wheels z_1 z_2, the back of one of which forms a portion of a frustum of a cone rolling about the center C of the radius-arm *a a*.

The center is itself a sphere, which allows any side motion of the instrument due to the inequality of the surface to take place without affecting the accuracy of the result. The necessary pressure of the centers being purposely adjusted to effect this. The frame can be taken off one center, s' (Fig. 17), by unscrewing a set screw at x, and at once placed upon the other. The weight w can be adjusted

Fig. 22

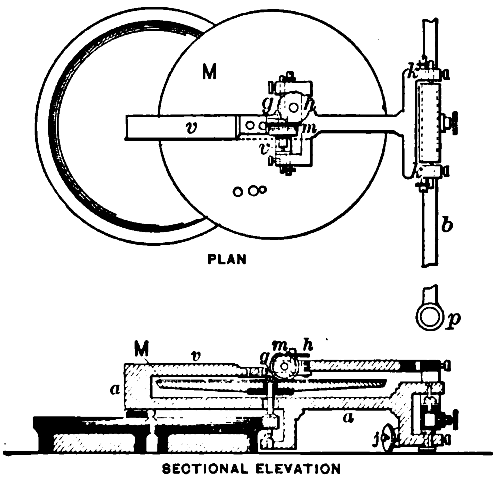
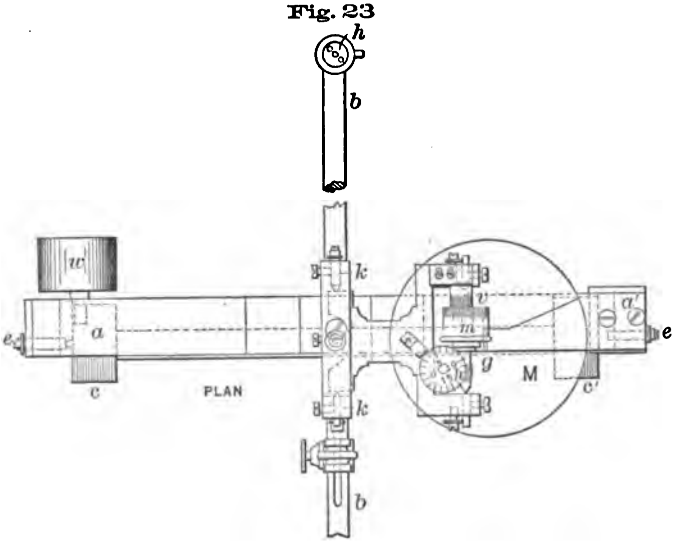


Fig. 23

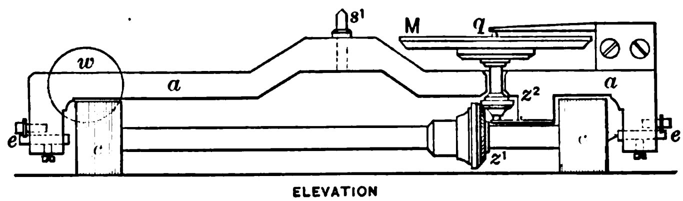


the roller upon the disk is obtained by allowing the weight of the portion of the frame *b* which carries the roller *m* to rest upon the disk by being pivoted by the centers KK (Fig. 18). A peculiar feature of the instrument is that the pole-arm frame can be centered either within or without the frame. If placed in the former position, the reading is twice as great as in the latter, the positions of in any position by means of the nut and screw *t* (Fig. 17), and so the pressure of the pointer *p* upon the surface of the diagram may be regulated.

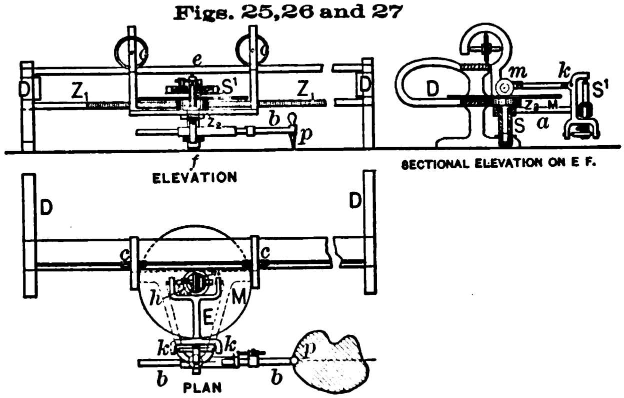
In both the above instruments the disks derive their motion from a roller in contact with the surface of the diagram, but in the next two instruments to be described, Messrs. Hohmann and Coradi have caused the disk to be turned in

a manner which prevents any such error as from the possible slipping of the above roller. The first instrument of the kind is shown (Figs. 20, 21) in plan and elevation. The disk M is carried by a frame (*aa*) as before, but the frame now swings

Fig. 24



Figs. 25, 26 and 27



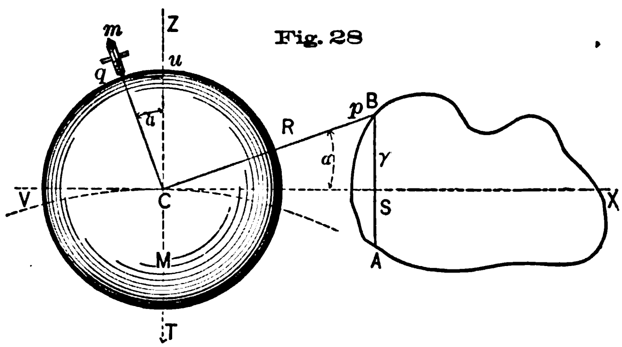


Fig. 28

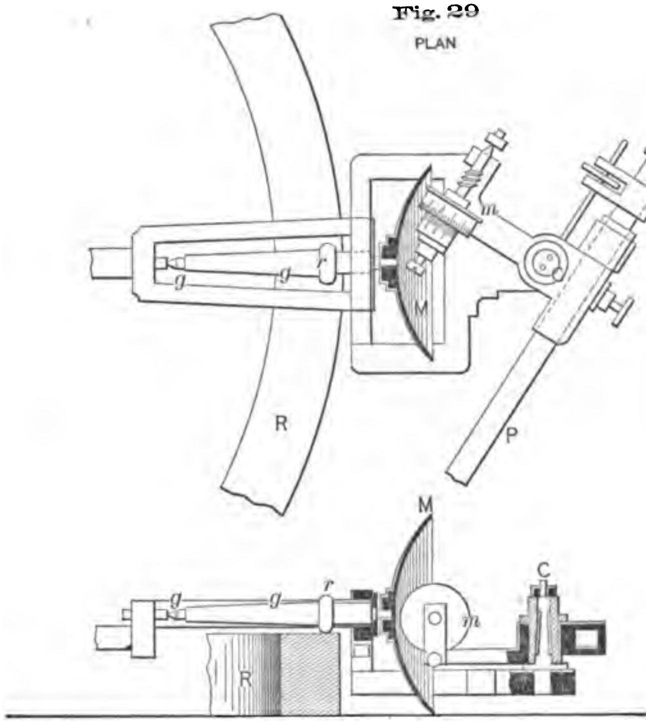
about a circular stand, the edge of which is toothed, so that the pinion (*i*), which is upon the axis of the disk, is turned, and therefore the disk itself, with the same angular velocity. The weight of the frame and disk is, to a great extent, taken off by means of the light rod (*l*), which swings about a central pillar P. A side view of the pole-arm is shown, and the mode of adjusting it and supporting the portion which carries the roller (*m*), so that by means of centers KK the weight of that portion of the frame is allowed to rest upon the disk.

It is evident that this instrument works upon identically the same principles as the foregoing ones. This "Freely swinging" precision planimeter was followed by another, called the "Plate" planimeter (Fig. 22), which is of still simpler construction. In this form advantage is taken of the fact that the measuring roller need not have its path through the center of the disk, and a support (*v*) is obtained above the disk, so that its pivot (*q*) can work between cen

ters, the weight of the frame being supported by rollers (j). The portion of the pole-arm which carries the roller (m) is (as in the last case) pivoted between the centers KK. The dial for higher readings is as in the case of the previous instruments denoted by h.

The last and most recent modification is the "Rolling planimeter," of Coradi. This approaches nearest to the diagram works upon the centers ee, which are set-screws in the frame (aa). The disk M is also carried between centers (qq), as in the instrument last described, and, also, as in that case, the path of the roller does not pass through the center of the disk. This instrument, which has many advantages, and, notwithstanding that it rolls on the diagram surface, gives results of great accuracy, has been examined with

Fig. 29



(Fig. 12), which completely explains its action. Here the center of the radius-arm is removed to an infinite distance, and the center of the disk and that of the pole-arm are carried along straight lines parallel to the axis OY in that figure. The way in which this is effected is seen from Figs. 23 and 24, which show Coradi's rolling planimeter in plan and elevation. Two rollers (cc') are in contact with the surface of the diagram, and on their axis is a bevel wheel (z_1) (Fig. 24), which gears with another bevel-wheel (z_2), which is upon the axis of the disk. Thus the wheels z_1 z_2 are turned as the frame is rolled along, and, consequently, the disk itself. The axis of the rollers cc great care by Professor Lorber, who has given a lengthy description of it and a full account of its theory.

The last planimeter of this kind to be examined is one by Professor Amsler. This instrument, shown in Figs. 25, 26, 27, differs from the last in that the tooth-wheel z_2 works in gear with a rack z_1 z_1, which is cut upon a fixed frame DD. Thus, although it is supported by the rollers cc, there is no possibility of slipping as far as the turning of the disk is concerned. The rollers run in a groove cut in the frame DD, and the action of the instrument is easy and smooth. The theory of its action is identical with that of the foregoing one, as explained by

means of the diagram (Fig. 12). The various parts are lettered in the figures to correspond with the explanations of that instrument previously given.

In the instruments hitherto described the surfaces of revolution are limited to the disk and cone, but various other surfaces may be made to replace these. The only one that has been so employed is that of the sphere; and in the present class of instruments, in which slipping takes place, the following property of the sphere is made use of: Let a sphere M (Fig. 28), which replaces the disk (Fig. 2), roll along the axis OX. Then suppose the roller m can, by suitable means, be moved round the surface so that its plane of rotation shall always contain the center of the sphere and be perpendicular to the arm CB, which corresponds to the pole-arm of the former instruments; it is evident that if the perpendicular be drawn from q, the point of contact of m with the sphere, to CZ the axis of rotation of the sphere, meeting it in the point u, the line qu is the radius of the rolling circle of contact of the measuring roller.

Therefore $\dfrac{\text{motion of measuring roller}}{\text{motion of sphere along OX}} =$

$$=\frac{qu}{\text{CV}}=\frac{qu}{q\text{C}}=\sin a.$$

But from the figure $\dfrac{\text{SB}}{\text{CB}}=\dfrac{y}{\text{R}}=\sin a.$

Therefore, adopting the same notation as hitherto used,

$$\frac{\text{motion of } m}{\text{motion of M}}=\frac{2\pi r n_1}{\Delta x}=\frac{y}{\text{R}}$$

$$n_1=y\,\Delta x\times\frac{1}{2\pi r\text{R}},$$

which proves that the area of the curve may be measured by any device, on the principle of Fig. 28. It may be shown in the same way as on p. 18, that the result is similar if the sphere rolls upon the arc of a circle, about any center as T, instead of along the straight line OX. Planimeters of this kind have been constructed by Mr. Hohmann and Professor Amsler. In both cases only portions of the whole surface of a sphere have been employed, and the motion is given by means of an axis, instead of by rolling the spherical surface upon the diagram. In Mr. Hohmann's planimeter, shown in plan and elevation, Fig. 29, the concave surface M is used. Rotation is given to this by means of an axis gg, an enlarged portion of which (r) rolls upon a circular metal path R. The pole-arm P turns about a center c, and so causes the rolling circle of the measuring roller m to vary according to the foregoing principles. This instrument has not come into use. Professor Amsler has employed the convex surface in an instrument somewhat similar to the one described, except that better provision is made for obtaining the required pressure between the surface of the roller and sphere, and for giving rotation from the roller-path.

HOW MUCH VENTILATION ?

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Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

The topic Ventilation has been treated by so many writers, and from so many points of view, that it may seem unnecessary to dwell upon it any further. The apology for doing so lies in the desire to clear away from the minds of some people a feeling that the asserted requirements of ventilation are made by enthusiasts and hobby riders. This impression, more or less vague though it be, possesses many practical men, influential in their professions.

And why, it may be asked, are they thus influential? Is it not because of devoting their attention to some one branch of work, becoming specialists in that branch? They naturally resent the rejection of their *dictum* by any one who has not by special training fitted himself to judge the matter; even as the architect would look with disgust upon the surgeon who asserted that he could build houses because he had studied the framework of the human body. They demand

faith on the part of others in their assertions, and rightly so, but they must make a like return. This law of reciprocity is an essential requirement of specialism, which word is at the very foundation of success in modern arts and modern scientific work.

Therefore, when men of talent, who have given their time to the study of hygiene and kindred subjects, assert certain facts and give certain data regarding ventilation, let us believe that there is some basis for what they say, and that the data which they give us are correct. There is a definite, *practical* reason, and, therefore, a scientific one, for the assertion that under ordinary circumstances a human being requires a given amount of fresh air in a given time, for health. The fact that he can be proved to have lived to old age on a much smaller allowance is no argument against it, any more than is the fact of persons living to old age in poverty an argument against the expense of a table well supplied with healthy and nourishing food.

An attempt may be made to rehearse the main features of the reasoning that leads to the requirements of modern science regarding amounts in ventilation. It will, perhaps, serve to help the faith of some doubting or skeptical person, puzzled by seeing thousands living around him in apparent health and in blissful ignorance that there is such a thing as bad air.

In the first place, the object of ventilation of buildings is to provide air for the inmates that shall be healthy for breathing, and not disagreeable to the senses. This necessitates a change of air, because operations are constantly going on that tend to make it objectionable. What are these operations? First in importance is man himself, not because he is always the chief source of vitiation, but the calculation of the amount required by him must generally be considered, for he is always present where ventilation is needed. After man come what may be styled the accidental source of vitiation, such as gas lights and lamps, gases from stoves and poor furnaces, the odors of the various domestic operations, and the gases and mechanical impurities arising from different manufacturing industries. Some would also include the gases and disease germs which in too many houses escape from drainage pipes into the rooms. It does not seem consistent, however, to allow in a statement of sources of impurity to be recognized and allowed for, one that should never be tolerated for an instant.

All the special sources of vitiation are variable in amount and in occurrence. They may cease entirely. Not so with man. He is a pump for inhaling pure air and exhaling bad air, and the machine keeps on its pulsation day and night, winter and summer, until death removes the power and the breath ceases. It is easy to see the propriety, therefore, of basing the air supply on his requirements, that is, on the number of people to be supplied, and then making such allowance as may be necessary for special sources of vitiation. This is the only sound, scientific ground to start from, for it will support all cases, from those in which a large gathering of people is the only source to be allowed for, to one where the number of beings present bears only a small proportion to the special causes of impurity.

The methods of ascertaining the requirements of man regarding air supply may be outlined in this wise. Carbonic acid (CO_2) is exhaled with the expired air, also organic matter and vapors that are the natural outcasts of the body, as well as those proceeding from an unhealthy state of the system. Water is also evaporated from the lungs and exhaled as vapor. In addition to all these, is the operation of transpiration from the pores of the skin, whereby moisture and various organic secretions are given off to the surrounding air. An adult gives off $1\frac{1}{2}$ to $2\frac{1}{2}$ lbs. of water per day in this manner. A person suffering from certain diseases will also infect the air with disease germs, that make it extremely dangerous for breathing. This is not the place for a discussion of the germ theory of disease; suffice it to say that these living germs are well attested to, are exceedingly minute, extremely active, and that so far as is known, a single one taken into a system where it finds congenial soil, may be the cause of virulent disease.

Of course, disease germs, and disagreeable odors arising from certain diseases accompany the presence of sick people whose proper place is in the sick-room or

the hospital, but a perfectly healthy man is constantly vitiating the air. It has in the past been thought that this was due entirely to the CO_2 gas exhaled, and all estimates and designs were made with the idea of removing it alone. Now the best opinion is in favor of the view that the vitiating of air by human beings, so far as it is made unfit for use, is due chiefly to the organic matter given off.

The facts are these: Pure air contains about 4 parts in 10,000 of CO_2, while exhaled breath is burdened with 400 parts in 10,000 of that gas.* The air exhaled is mixed with the surrounding air, so that in a well-ventilated room there will be 6, or possibly 10 parts in 10,000 of CO_2, but in the ordinary class of unventilated buildings the amount of CO_2 in occupied rooms is often 15 or 20 parts in 10,000, and sometimes far more. This CO_2 is a negative gas, odorless, tasteless, and entirely non-poisonous. The aqueous vapor given off is equally harmless in itself. It does not differ in any sense from the water that is always in the atmosphere and is so necessary an ingredient for our comfort. Under usual conditions about five per cent., by volume, of exhaled breath is moisture.

The organic matter thrown off from the healthy body is, like many organic compounds, extremely subtle in its nature. There is no chemical means of analysis known that will define it in its varying composition and determine its amount. The most delicate laboratory test that chemistry can devise will show no quantitative difference in this respect between pure air and the breath that has just left the body. There is a popular test of its presence, however, that is not to be mistaken. The peculiar odor which infests an unventilated room occupied by many people, can be attributed to nothing else than this. The reasonable assumption is, that these organic substances, in the presence of warmth and the moisture of the breath, easily undergo decomposition, and make themselves known by the disagreeable odors of their decay.

* Some experiments made upon London air indicate that the commonly accepted proportion of 4 parts of CO_2 in 10,000 of pure air is too high. 159 tests were made in an open space, at St. Bartholomew's Hospital, in the years 1882 to 1884, that give as an average, leaving out the tests on foggy and misty days, 4.3 in 10,000, the lowest being 3.1 on a fine August day. It was concluded by the experimenters that the proportion for pure country air is probably about 3 in 0,000.

Such are, so far as is definitely known, the constituents given off from the healthy human body to the surrounding air. What are the injurious ones? We know that in an unventilated room, occupied by many people, the air becomes in time unendurable, and even if the case is not so extreme, persons living long in poorly ventilated buildings, where the air is habitually "close" and "stale," are debilitated, and lack the energy of those accustomed to fresh air. This statement needs no proof beyond an appeal to common experience. The moisture given off cannot be injurious. The issue then is narrowed down to the CO_2, and the organic matter. CO_2 in excess may cause injury by occupying the space otherwise taken by the oxygen, which is the vital element of the air on which our blood is fed. If the air is thinned out by CO_2, the blood may be robbed of its required supply of oxygen, and debility result. It seems absurd to suppose that the small increase of even 25 parts in 10,000 of CO_2 would have any such effect, but the human body is a very delicate organization, and it may be that a long continued deprivation of this kind would become observable. It is difficult, however, in the light of numerous experiments, to attribute to the small increase of CO_2 the peculiar and immediate depressing effect of a close, crowded room, which room, moreover, is not necessarily a warm one. The majority of modern hygienists are disposed to consider the organic matter the cause of the bad effects of air vitiated by breathing, and to say that the CO_2 has very little to do with it. This view certainly seems a reasonable one. At all events, it is not the CO_2 which causes the very unpleasant odor of a crowded, unventilated room, and this with most people is a practical and very strong inducement to ventilation. The writer is not competent to say what is the ultimate cause of vitiation of the air by breathing. The evidence appears in favor of laying the charge upon the organic matter, but the proof does not seem positive, that the CO_2 has no effect whatever. It may be that they combine to exert an effect upon the health and comfort of human beings, when allowed to accumulate to any extent in the air inhaled. But this makes no special difference so far as practical ventilation is concerned, for, whichever is the cause,

the removal of one involves the removal of the other, and the practical question is, how much air is necessary, per individual, in order to keep these down to a safe, *i. e.*, a healthy limit?

The answer to this question is the object intended in what follows. But before proceeding further, a sharp distinction must be drawn between what a mere theory would require, and the demand of a theoretical discussion based on the actual conditions of practical ventilation. Some scientists have treated this subject apparently under the supposition that a person inhaled through a tube from one reservoir of fresh air, and exhaled through another tube into another reservoir, the supply and deposit being kept entirely distinct. By this method they get a very small amount as a result. The only case that I am aware of in which the actual bore any resemblance to these assumed conditions, was the somewhat unique one of the shipwrecked mariners of the Thomas Hyke, as related by Mr. Frank Stockton. The fact is that actual ventilation is a process of dilution, the expired breath mingling with the air of the room, and the supply needed must be sufficient to keep the mixture in the room down to a proper standard of purity.

To compare with a standard it is necessary that the amount of CO_2 and organic matter present should be estimated. But the latter cannot be calculated. It, however, bears a direct proportion in amount to the CO_2, for obvious reasons, and the latter, which can be calculated by analysis of the air, is made the basis for estimating the presence of the former. It is a matter of experience by those accustomed to study the subject, that 6 to possibly 8 parts of CO_2 in 10,000 of air in a space occupied by healthy people accompanies an atmosphere that has no disagreeable odor, and can be considered healthful. If the proportion rises to 10 or 12 in 10,000 a slight unpleasantness usually manifests itself, and the ventilation cannot be considered thorough. Above that proportion of CO_2, the air soon becomes from breathing absolutely unfit for use. Having this standard fixed it is a comparatively easy matter to determine on the amount of air supply, for, knowing the amount of CO_2 in pure air, the amount in exhaled breath, and the amount of air passing through the lungs in a given time, a simple use of mathematics shows the amount of fresh air required per hour to dilute the breath and bring the mixture to the required standard. It is simply then a matter of the standard taken that varies the air allowance. Dr. Parks, who is one of the highest authorities, places it at 6 in 10,000, which requires a supply of 4,000 cubic feet per hour per person. The late Mr. Briggs adopted 8 in 10,000 as a standard, which requires a supply of 1,800 cubic feet per hour. M. Planat appears to consider 10 parts in 10,000 a sufficiently high standard.

It may seem that these wide differences destroy all value of calculations, but it should be remembered that the test by CO_2, while in a sense indirect, is yet a very delicate one, that a very slight difference in amount of CO_2 will accompany a decided change in the comfort and health of the air. If we are content with a somewhat tainted air, a standard of 10 in 10,000 will answer, and we will, so far as the air is concerned, probably live to a good old age. If we want air in our rooms, entirely free from traces of mustiness, and as fresh to our senses as that which blows over the mountains, we must take Dr. Park's estimate. The expense may in some cases prohibit such a standard being attained, but, as he writes with reference to another matter, "After all, the question is not what is likely to be done, but what ought to be done, and it is an encouraging fact, that in most things in this world, when a right course is recognized, it is somehow or other eventually carried out." Dr. Parks gives as a rule easy to be remembered, one cubic foot per person per second, equal to 3,600 cubic feet per hour.

Most writers make a distinction as regards age and sex of the people, and also regarding their occupations. In view of the considerable difficulty of obtaining exact amounts in ventilation, it seems an unnecessary refinement to allow a deduction from the above standards for adult males, even in the case of children, for as Dr. Billings observes, the latter are more delicate and susceptible to impure air.

For difference of occupation the case is different, as some kinds of work increase vastly the energy of breathing.

If some one exclaims in impatience, these standard amounts are very fine, but

entirely impracticable on account of expense, let him remember that to get ventilation he must pay for it. If he wants the best ventilation the highest standard is not too high. The entire matter is one of compromise between good air and expense. As a reasonable compromise in many cases the 1800 cubic feet allowonce may be used, but any lower allowonce than that for largely occupied rooms should not be allowed.

So much for the vitiation caused by the persons of human beings. The special sources of vitiation of air may now be briefly considered. It will be recalled that the term *special* is used to denote the causes of vitiation met with in buildings, other than the human beings occupying them. It would be more general to say, other than the living creatures occupying them, for in the case of stables and the like it is a highly important duty of the sanitary engineer to ventilate them and base his calculations on the number of animals present. The chief distinction between the animate and the special sources of vitiation is, that the former are continually in operation, and with some degree of regularity, while the latter are varying, both in degree and continuity. There is another important difference in respect to treatment. The animate sources of vitiation must be allowed to breath into the surrounding space, and their vitiation be mingled with the air of the rooms, whereas, in nearly all cases, the special impurities can be directly conveyed away.

The usual special causes have been enumerated. Some of them, such as gases from poor furnaces, etc., are hardly legitimate, but nevertheless do occur. No calculation can be made for them. The gases and mechanical particles arising from manufacturing operations are so various in nature and amount that no general allowance can be made for them. As a rule they should be immediately and thoroughly removed from the work-room.

Likewise, for the odors arising from numerous domestic operations, no allowance of air can be given for their dilution. It would require all out-doors to make the odors of wash-day unobservable.

The only effectual plan is to carry off such sources of unpleasantness as soon as possible.

The subject of illumination is one that comes under this heading. All do not realize what consumers of air our lamps and gas lights are. From one point of view they are necessary nuisances. In the operation of burning large amounts of CO_2 are given off, and also other products more or less injurious and disagreeable. The heat evolved is a separate matter, entirely apart from the effect on the purity of the air. Experiments show that a candle gives about 0.3 cubic foot of CO_2 per hour, an ordinary lamp about 1 cu. ft. per hour, and an average of tests made by Mr. H. C. Bowen, of New York coal gases, both rich and poor in hydrocarbons, give 0.75 cubic foot of CO_2 per cubic foot of gas burnt. A $4\frac{1}{2}$-foot burner yields then, 3.38 cubic feet of CO_2 per hour. An adult in ordinary conditions of quiet occupation emits about 0.6 cubic foot of CO_2 per hour. So, as producers of CO_2, a man equals two candles, a lamp equals two men, and an ordinary gas-burner can hold its own against nearly six men.

It cannot be claimed positively that the production of CO_2 by lights represents the same corresponding vitiation of the air for breathing purposes, as in the case of man, for organic matter is not proportionally evolved. But, especially in the case of gas, there are other products given off, quite as objectionable in their nature. Anyone who has had occasion to mount a step-ladder in a close room, brilliantly lighted, can bear testimony to the overpowering and peculiar condition and odor of the air near the ceiling, a condition that heat alone could not occasion. More or less free gas escapes unconsumed, and some of the carbon is burned only to carbonic oxide, a very poisonous gas. Also, the gas is very liable to emit di-oxide of sulphur if the coal contained any of that impurity, which enters the lungs and there forms sulphurous acid, injurious in its effects. There are other products of combustion which a gas analyst can better describe, but enough has been said to show that the amount of vitiation increases directly as the amount of CO_2, and hence, if the products of combustion are allowed to mingle in the room, the same test of the purity of the air can be applied as for man.

The question then becomes as before, what standard for the allowable amount of CO_2 should be taken? Wolpert, says that the air supply should be 1,800 cubic feet of air per cubic foot of gas burnt. A calculation shows that this corresponds to a standard of 8 parts of CO_2 in 10,000. Briggs, who we may be sure would give full prominence to the economical side of the subject, gives this rule for halls of audience: "Ten cubic feet of air per minute per cubic foot of gas burnt per hour." Hence, for a $4\frac{1}{2}$-foot burner, 2,700 cubic feet of air per hour. This makes the standard of CO_2 16 parts in 10,000 of air. Dr. Parks approves the larger estimate, and it seems the best standard, although there may be modifying causes in some cases that will make Mr. Briggs' rule advisable.

The preceding remarks are based on the assumption that the products of illumination are to be allowed to mingle with the air in the apartments lighted.

In the case of candles and movable lamps such a course is advisable, but for gas lights it is a question if the evolved gases should not be directly carried away. Or rather, there is no question at all of the propriety of doing so in all cases where arrangements can be made for what is termed exclusive lighting.

This concludes the attempt to indicate the steps by which the foremost sanitarians of the present day have reached their conclusions regarding amounts of air supply.

As was stated before, the apology for devoting space to this subject lies in the desire to leave no room for the opinion that the demands of science in this respect are the demands of hobby riders, or of those who theorize, with supreme indifference as to whether the conditions they assume are ever met in practice or not.

The figures given are as definite as is the price of gold, quoted at the mint, and as practical as the current quotations of iron. The standard of purity can be varied, to be sure, but if lowered to reduce expense the purity of the air is likewise lowered.

The effectiveness of ventilation and the cost of ventilation are, within limits, in direct ratio to each other. When we are told that to keep the standard of purity at 6 parts of CO_2 in 10,000, the supply must be 4,000 cubic feet per hour, this means that if a man is put in an absolutely tight room of 4,000 cubic feet capacity filled with fresh air, at the end of one hour the air will have reached the limit of impurity, and after that it will become unhealthy, according to that standard. Similarly for a gas light.

There is no room for uncertainty in these figures; they are as precise and practical as any of the data on which business calculations are based. It is merely a question of how pure you intend to have the air.

These data are precise, and as such are the only fit basis for calculations, but at the same time, in their actual application they must be frequently modified, as I will endeavor to explain. As in all applications of scientific truth to practical necessities, it is necessary to remember that a perfect theory cannot take account in a single formula of all possible conditions that may be met with in practice. It can only serve as a foundation for estimates made to suit the special cases that continually arise. Skill and common sense are necessary to decide how far the conditions of the case should influence the amount demanded by the special conditions used in deducing the theoretical supply. This is the only sensible; it is the only scientific way, and it accounts for the apparently wide separation between the theoretical amounts specified by sanitarians, and the actual amounts required by the best sanitary engineers for different kinds of buildings.

With this sound basis to build upon, the next step in order is to touch upon the main points influencing the selection of an amount of air supply, and then to give the amounts used by engineers for various buildings.

Perhaps the first practical condition to be mentioned that differs from those assumed in estimating the standard air supply, is what may be termed insensible ventilation. It is mentioned first because always existing. The conditions of the theory suppose a man inclosed in a room absolutely impenetrable by air, except through certain definite channels under control. The fact is, this never exists. Movement of air constantly takes place

through window cracks, etc., and even through brick walls. Mr. G. P. Putnam mentions in "The Open Fireplace," experiments he made with a room in which every observable crack and opening was carefully puttied, the brick chimney-back and jambs were oiled, and ultimately four coats of oil paint put on the walls and ceiling, and three on the pine floor, but after all this the inflow from the furnace register was only diminished 20 per cent. At the end of the hour a quantity of air more than equal to the entire capacity of the room had passed through the register. There were, of course, microscopic outlets for the air which it might be possible to close, but this illustrates the fact that the inlet and outlet registers of a room are by no means the only modes of passage of the air.

The importance of insensible ventilation does not lie in its being a condition to be calculated upon in arranging a systematic scheme of ventilation, for in such cases the regular, legitimate air supply is the only one to be relied upon; but it lies in the fact that without it, nine-tenths of civilized humanity would long since have had, like the colored gentleman, "to take to the woods."

In the light of what has been mentioned before it needs no demonstration that in unventilated buildings, if it were not for this insensible ventilation, people could not exist. It is the salvation of thousands of families in elegant homes, and even it does not in many cases prevent premature loss of health, and even death from slow suffocation.

Insensible ventilation is, however, a factor that the architect can very reasonably take account of in designing many houses where the number of inmates is small and the expense must be a minimum.

Cubic space allowance is another condition of the air amount question that is well worth considering. Its position as an element of the problem is very easily defined, although more or less misconception of it exists. If a certain number of people occupy a given room, each person has a certain number of cubic feet of space in that room, that for purposes of estimate may be called his. The simple question is, how does it affect the air supply required by him?

There is, unquestionably, an impression among many that cubic space takes the place of ventilation.

To a limited extent this is true; that is, apparently true; for if you place a few people in a sufficiently large room, with no means of ventilation, the air may still keep passably pure owing to insensible ventilation, and roughly speaking, the degree of purity will depend on the cubic space allowance. Those few inmates may occupy that room continuously and breathe good air. But if a large number of people occupy the room, the insensible ventilation is entirely inadequate, and the air becomes very bad. The space allowance cannot then, in any sense, take the place of air supply. It merely serves as a mixing chamber for diluting the persons' breath with fresh air. Each person should have so many cubic feet of fresh air per hour, independent, within limits, of how large a space that air flows through. But, indirectly, the space allowance may influence the air allowance in this way. If, previously to its occupation, the space is filled with fresh air, it acts as a reservoir, upon which the lungs can draw until it has reached the limit of impurity set by the standard taken. As soon as this limit is reached, however, then the regular air supply must go on entirely independent of the cubic space.

This is theory, but it is theory to which practice makes the closest of approximations in cases of crowded rooms, and can only be disregarded where the active sources of vitiation of the air bear only a small proportion to the insensible ventilation.

It is apparent, then, that when rooms are occupied for short sessions, and between sessions the air is thoroughly renewed, it is perfectly allowable to reduce the air supply in just so far as the space serves as a reservoir. When rooms are continuously occupied by many people, the amount of cubic space cannot lessen the required air supply.

In the case of rooms large relatively to the number of inmates, or other source of vitiation, cubic space allowance may very reasonably materially affect the allowance of air supply, and in some cases may be rightly assumed to take the place entirely of systematic ventilation, especially where the expense must be kept down to the lowest limit. It is on such

grounds as these that the specification of cubic space allowance for tenement houses and some other buildings can be justified. The specification of it for schools, with the idea of its taking the place of systematic ventilation, ought not to be tolerated. If any room needs ventilation it is the schoolroom.

While considering the practical conditions that affect the selection of a standard of air supply, the relations of warming and ventilation may well be discussed. Theoretically they are distinct subjects. Ventilation is, for hygienic reasons, a necessity. Warming is, within limits, merely a matter of convenience and pleasure. A great part of the time we warm our houses to be comfortable, not because it is necessary for health. If we wore warmer clothing we might live in much colder houses in good health. The physical condition of many who live in hot, stuffy rooms would probably be improved by such a course of treatment. Of course this supposes dampness to be eliminated.

Theoretically, then, ventilation is necessary at all times, and in amounts independent of the temperature, while warming is a matter dependent upon the climate and the "cold waves" that the Weather Bureau sends us. The Arab in his tent, and the Esquimaux in his reeking snow hut, both require fresh air.

Practically, however, warming and ventilation are very closely connected especially in this climate of cold winters. The reason lies in the fact, that in order to make a building comfortable in this climate in cold days, and with a reasonable expense, it is necessary to heat the air itself. Hence, the greater the amount of ventilation, the greater the amount of heat required, and the expense. It may be laid down as a practical truth beyond peradventure, that people will insist on being warm in preference to being well ventilated If one or the other must be sacrificed, it will be the ventilation, every time.

If the expense of sufficiently warming the volume of air required for good ventilation seems excessive, the householder will in all probability cut down the air supply. He is hardly to be blamed for this. The most rigid sanitarian will shut the window if he is chilly, although he knows it to be the only inlet for fresh air to the room.

This matter of the relation of warming and ventilation is seen to be one of dollars and cents, rather than of physical science, and hence its influence in determining the amount of air supply is of a different nature from that of cubic space allowance. As a sanitarian, no architect or engineer ought to consider it. As a practical business man, he has to consider it, and should always remember that, as regards the popular use of the title "warming and ventilation," there is a significance in the order of the words.

For many public buildings, however, such as hospitals and schools, no consideration of the expense of warming should move the designer from rquiring a sufficient air supply.

The natural ventilation, cubic space allowance, and the relation between ventilation and warming are the chief matters influencing the reduction of air supply allowance that are not directly connected with the inmates and their occupations. In special cases of manufacturing buildings, etc., there are special causes of vitiation; but, as has been said, these should be carried off directly where practicable, and in that event do not influence the amount of air supply required for the inmates.

The occupations and physical conditions of the inmates of buildings, of course, have a bearing upon the amount of air supply, but are directly connected with the inmates themselves. The best way of considering them is to pass directly to the amounts of air supply selected by engineers for various kinds of buildings. The chief factors in causing the varying figures for different buildings have been—First, the length of time that the rooms were used continuously; second, the physical condition and occupations of the inmates; and, third, but perhaps more potent than all the others, the amount that it is reasonable to expend for ventilation on different building.

The highest figures used are for hospitals. Nothing less than the highest standard already given ought to be taken for such buildings. Dr. Parks has had great experience with hospitals, and he states that the minimum for them ought to exceed the allowance for healthy be-

ings by at least one-fourth. In wards for surgical operations a much larger allowance should be made. Four thousand to five thousand cubic feet per hour per person is required by Péclet. Planat states an amount in cubic meters which equals 5,300 cubic feet. Hospitals for diseases, in which there are very offensive exhalations from the body, and especially for contagious diseases, require practically all the air that can be obtained. The supply should not be less than 7,000 cubic feet per hour. In the case of hospitals the necessity for fresh air is so apparent that there is comparatively little difficulty in persuading people to meet the expense of this thorough ventilation.

For theaters, those engineers who are strong upholders of the higher standards of purity of air require 2,000 to 2,500 cubic feet per hour. In this reduction from 3,600 cubic feet, they doubtless consider that the use of the theater is not continuous, and that the upward movement being a prominent feature of theater ventilation, the breath is more rapidly removed from the level of the audience.

Most engineers adopt figures that are more of a compromise with expense. General Morin requires 1,400 to 1,800 cubic feet per hour. This estimate, although much smaller than the first, will give very fair ventilation. Compared with those theaters which have no thorough system of ventilation, it seems all that could be desired. The Madison Square Theater, of New York, has a theoretical supply of 1,500 cubic feet per person, and is very successful in its ventilation. The Vienna Opera House, which is considered one of the very few well-ventilated theaters, has a theoretical supply of about 1,000 cubic feet per person.

The conditions of church ventilation are in many respects similar to those for theaters, and about the same air allowance should be made. In the majority of churches the services are not attended by crowded congregations, and hence tolerable ventilation may be obtained with much less air supply, but special occasions frequently occur when the ventilating apparatus is taxed to its utmost. Considering this, it is unadvisable to make any less allowance for churches than for theaters.

Rooms used for lectures, etc., of an hour or so duration, require much the same air supply as for theaters, if the ventilation is arranged in the same way as is usual for the latter class of buildings.

All lecture rooms with low ceilings, where the exhalations from the body are more mingled with the air about the inmates than is the case in theaters with good upward ventilation, require a larger allowance. For thoroughly satisfactory results, the full air supply required by the standard taken will be necessary. If exclusive lighting is not practiced, there should be a liberal allowance for the use of the gas-burners, according to the figures already given. Mr. Briggs, in his paper on "Halls of Audience," takes a position on the verge of robbing the ventilation to propitiate the pocket. He writes—"Dr. Reid's value of 10 cubic feet per minute is all that should be used in planning for halls not occupied over two or three hours at a time. All that can be judiciously urged for ventilation in view of the cost of fuel." He states further to the effect that this is the minimum allowance, and that the apparatus should be designed so that during warm weather the supply can be increased to 20 cubic feet per minute. "An amount which, with open doors and windows, if ventilating currents are well distributed among the audience, will be ample for the comfort of a crowd in hot weather." These are the views of a practical and experienced engineer, and as such are of value, but nevertheless the testimony of others, and the results obtained with designs in operation, lead to the belief that they go too far in compromising the ventilation.

It is certain that, in the case of small and crowded lecture rooms, 600 cubic feet per hour per individual will not prevent impurity of the air.

Court-rooms, legislative halls, and all that class of rooms that are liable to continued occupation for many hours by a crowded congregation, require an ample supply of air. Nothing less than the full amount corresponding to the standard of purity selected should satisfy the designer. Dr. Reid, in his writings on the ventilation of the Houses of Parliament, shows the importance of keeping the brains of the law makers clear and

active, and if that is necessary in England, it is quite as much so for our own State and National Legislators, and for the typical American jury. Gen. Morin, who bases his figures on a theoretical supply of 1,800 cubic feet per person per hour, irrespective of conditions, and then modifies this amount for different cases, requires for "lecture rooms and halls for brief receptions" 1,100 cubic feet, and "for assembly rooms and halls for long receptions," 2,100 cubic feet per hour, a difference of nearly 100 per cent. in favor of the latter. In public buildings, like court-houses, State capitols, etc., there is no excuse for not supplying a generous air allowance. The money question should not prevent the best ventilation.

The opinions of engineers as to the air supply for schools are various—from those who would limit small children to 200 cubic feet per hour, or even less, to those that approve of supplying to them the same amount as for adults in crowded rooms long occupied. It is hardly necessary to allude to the importance of well ventilating schools. The miserable condition of many of our schoolrooms in this respect may have no small effect upon the health of a generation, upon which, in fifteen or twenty years, the prosperity of the country will depend.

From considerations already alluded to, it seems unadvisable to reduce the supply because of the youth of the children. Of course, in the case of schoolrooms that are to have few scholars in proportion to their size, a small air allowance may be admissible, but this is more because of natural ventilation taking place than from the age of the inmates. Nearly all schoolrooms will be crowded at times, and that for hours together, and for them the full air allowance should be made. For those rooms that are occupied only an hour at a time, and at the end of the session are completely flushed by fresh air by opening the windows, it is allowable, perhaps, to make some reduction in air supply, because of the space acting as a reservoir. But this latitude should not be imposed upon. A well-filled room with low ceiling will soon become close unless ventilated. If the air allowance is too much reduced, the room will become close and unhealthy before the close of the hour. If the air is to be maintained rigidly up to a standard of purity, little, if any, reduction of supply can be made for well-filled rooms, although they are flushed every hour.

A few years ago a commission was appointed to examine the public schools of the District of Columbia, and in their reports, dated March 15, 1882, appear the following specifications:

"In each class-room not less than 15 square feet of floor shall be allotted to each pupil. In each class-room the window space should be not less than one-fourth of the floor space, and the distance of the desk most remote from the window should not be more than one and a-half times the height of the top of the window from the floor. The height of the class-room should never exceed 14 feet. The provisions for ventilation should be such as to provide for each person in a class-room not less than 30 cubic feet of fresh air per minute, which amount must be introduced and thoroughly distributed without creating unpleasant draughts, or causing any two parts of the room to differ in temperature more than 2° F., or the maximum temperature to exceed 70° F."

The list of buildings of various kinds for which ventilation is necessary is by no means exhausted, but the above are the chief among what may be termed public buildings. Hotels, office buildings, banks, prisons, etc., all offer conditions that must be considered, according to the same principles as the foregoing examples.

There are a great variety of buildings devoted to special trades and manufactures, for which special allowances must be made. The impurities, both mechanical and chemical, given off in various processes of manufacture, should, if possible, be immediately carried away at their source; but this cannot always be done, and then a large supply of fresh air is required to sufficiently dilute them and keep the air of the room in a healthy condition for the workmen. The accurate estimate of the supply needed in such cases is a very difficult, if not impossible problem. It is almost entirely a matter of experience and judgment, although it must be admitted that the amount of available experience in the subject is very small. The amount of air supply may be based upon the number of

people present, and an extra allowance made per person over what would be required if they were the only sources of vitiation, this extra amount to be determined by experience; or, an extra allowance may be made in bulk for all the special sources of impurity independent of the number of workmen present. An illustration of the former method is given in the design for the ventilation of the laboratories of the Massachusetts Institute of Technology, described in the "Sanitary Engineer," of October 30, 1884:

"For recitation rooms and lecture rooms 1,500 cubic feet per hour are allowed to each occupant. For physical laboratories, where the gaseous products of bunsen flames and electric batteries are likely to act as vitiating agents, 2,000 cubic feet per hour are allowed. For chemical laboratories, which are supposed to be supplied with effective hoods for the collection and removal of offensive or dangerous gases and fumes, and also under which any work evolving a considerable quantity of gas is supposed to be done, 3,000 cubic feet; and for the organic chemical laboratory, 4,500 cubic feet are allowed to each occupant per hour. To other rooms of the chemical floor, as the library, balance room and volumetric room, 2,000 cubic feet was apportioned to the individual, because of the proximity of chemical laboratories, and the desirability of being able to produce within them an excess of pressure, causing outward air movement."

With regard to the supply of 1,500 cubic feet in lecture rooms, it may be well to quote further—

"Good ventilation based on so low an estimate as 1,500 cubic feet per occupant would not be possible in most climates, and in the comparatively dry climate of New England that allowance makes the most efficient and economical use of the supply a necessity."

The reason for the distinction made between moist and dry climates, is that the impurities would not be so disagreeable, because less noticeable, in the latter, although with a given air supply they would exist the same in both cases. It seems a little unfortunate to make this distinction, when the avowed purpose of ventilation is not merely to obtain what is agreeable, but to go a step beyond, and attain to what is an absolutely healthy supply. If this is done there can hardly be any practical distinction for moist and dry climates. But the opinion is evident in the quotation, that a supply of 1,500 cubic feet per hour for each person is little enough for the lecture rooms of a college.

There is a class of buildings that has not yet been alluded to, and these may best be described as private buildings. Dwelling-houses are seldom mentioned in discussions of the air allowance for buildings, probably for the reason that a systematic air allowance is seldom, if ever, made for them. Nor is one always needed. This statement can be maintained on strictly scientific grounds, even in the face of the fact that ventilation or change of air is always necessary where human beings are present. The preceding analysis of the conditions influencing the amounts required in ventilation places us in a position to do so.

In addition to the all-important item—expense—natural ventilation and cubic space allowance both have a very great influence upon the ventilation of dwellings; so great, as in many cases to make systematic ventilation practically unnecessary. In many cases, but decidedly not in all. Dwellings differ largely in their requirements for ventilation, and judgment should be exercised in deciding when a thorough ventilating design is necessary, and when a reasonably healthy atmosphere can be maintained without it, or with only a few simple devices for the circulation of air. The lines on which such a judgment is based appear to be about as follows:

If there is any building for which regard must be had to the expense of ventilation, it is the dwelling. Public buildings are usually constructed by governments, corporations, or wealthy individuals, and the expense of a thorough system of ventilation is very small in comparison to the cost of the building, and, considering the importance of the matter, should be cheerfully met. Not so with the usual private house. It is built for the man who is toiling over a city desk, or with his hands, day after day to provide food for his family, and from the first foundation stone, to the setting of the chimney cap, the cost is never lost

sight of. For such buildings, the expense of ventilation must be brought down to the lowest practicable limit, or it will be entirely debarred. For private dwellings, in particular, elaborate schemes of ventilation are a failure, because too costly. Happily it is possible to obtain a fair amount of ventilation in many cases with very little outlay, either in first cost or in running expenses.

The usual home is large in proportion to the number of people it covers. Unlike the theatre or the concert hall, where the audience is closely packed, the sleeping room has rarely more than two in it, and even the family sitting-room in modern times does not generally contain over five or six persons. The proportion of cubic space in private rooms is usually quite generous to each person, and although any amount of cubic space does not in itself take the place of ventilation, yet this gives a better opportunity for natural ventilation to change the air than if the people were closely crowded. Natural ventilation is, as a rule, far more active in houses than in public buildings, and it may even become an efficient means of ventilation in some cases. The wall and window surface per individual is large in private rooms, and the natural ventilation is consequently greater.

Houses are generally constructed more lightly than larger buildings, and, especially with frame buildings, there is every opportunity for very active natural ventilation. I have sat in country rooms with the outside temperature well down toward zero and seen the carpet rise and fall with each wintry gust, as it forced its way under the floor. Such a house must surely be well ventilated, at least while the wind blows. It is, perhaps, one redeeming feature of what are expressively termed "skin" builders, that the houses they construct give every chance for natural ventilation. Speculation brown-stone fronts, put up a row at a time, are very expensive to warm, for this reason, but they are sometimes better ventilated than the more carefully-constructed building beside them, that the retired merchant has taken pride in erecting.

Natural ventilation, then, can be largely calculated upon to change the air in private houses, and, while it cannot be claimed to be always sufficient, yet there are many cases where more is hardly required. During the summer the windows are open most of the time. During the winter, the furnace, if properly designed and set, will supply sufficient fresh air for the inmates, not to speak of the circulation that is constantly going on through cracks and ill-fitting windows. Open fires, if used in addition to the furnace —would that they were more so—quite efficiently ventilate the rooms they are in.

Where close stoves are used, little can be claimed for them as ventilating agents, and it is undeniable that, even in country houses, rooms are sometimes met with that are as close as the stoves themselves.

More often, however, the natural circulation keeps the air in a tolerable condition, and a few simple devices will much improve the ventilation of stove-heated rooms. At all events, a systematic supply and removal of air, involving continual expense, is out of the question in the great majority of dwellings, so long as experience shows that they are habitable without it. Resource must be had to simple and cheap means of aiding the natural ventilation in its good offices.

The opportunity is tempting, here, of digressing into a discussion of the ways in which a fair ventilation can be cheaply obtained in houses by the use of the appliances for warming, and also of the various means of warming houses, but the aim of this paper is not to describe how to ventilate, but *how much* to ventilate. In this direction enough has been said to substantiate the statement, that, for the ordinary private house, a systematic air allowance per individual is almost an absurdity, and that for many of them a thorough systematic plan of ventilation is unnecessary, even were it not debarred by reason of the expense inseparable from it.

The only dwellings for which such a plan of ventilation is not out of the question are those inhabited by the wealthier classes. Houses for which a large sum has been paid at the start, and whose yearly expenses are so considerable as to make the cost of running the ventilation merely an item in the total amount.

Steam heating, by indirect radiation, is becoming quite common in this class of buildings, and the very fact of properly

warming by this method involves the flow of large volumes of air into the rooms. The same condition should accompany the use of furnaces, and would, if they were made warm-air furnaces instead of hot-air furnaces, as they are only too justly named.

By the use of outlet flues, properly heated, the air is forced out of the rooms, and excellent ventilation obtained. Ventilation can be maintained when it is too cold to have open windows, and yet too warm for using the heating apparatus, by heating the aspirating shaft or flues. But, even for these houses, it is easily seen that a systematic air allowance of so many cubic feet per hour for each individual is hardly necessary. For rooms to be occupied by dinner parties, or crowded during social gatherings, such calculations are advisable; not so for the ordinary household rooms. The fact is, that, as warming apparatus is now set up, the amount of air entering these rooms depends not on the number of people using them, but on the number of square feet of radiating surface that experience shows is necessary to warm them, and the amount of air that will flow over this. With the exception of the special rooms mentioned, this will practically give all the ventilation needed, at least during cold weather. For warmer weather, some way of exhausting the air must be provided.

Of course, where gas is used for illumination, some means of specially ventilating it should be used, or else rooms not supplied with systematic ventilation will become unhealthy during the evening. There are inexpensive ways of ventilating gas lights that should not be omitted in modern houses. But even if this is not done, and the absence of gas-light ventilation makes systematic ventilation more necessary, the point is that it is for a special source of vitiation, entirely independent of the number of inmates of the room.

The endeavor has been in the preceding remarks concerning the ventilation of dwelling-houses, to maintain the position taken at first, that, having full regard for the hygienic condition of our homes, admitting that human beings require a certain amount of fresh air every hour, yet a systematic air allowance based on the number of people present is not the necessary or the best way of treating the subject for houses, as it is for most public buildings. Rooms for entertainments, which are at times decidedly public rather than private in their use, are excepted.

This may not be admitted by all, and yet the amount of air supply, even in cases where a systematic ventilation is planned for houses, is and will be decided by certain practical considerations, that will give just as efficient ventilation as if something like the following course had been pursued:

That room will hold three or four people, and must have so many cubic feet of air per minute. This bedroom will accommodate two, and must have so much. Only the cook stays in the kitchen, we will give it half as much as the bedroom. Nobody lives in the hallway, so there can be no need of ventilating it at all.

CONTRIBUTIONS TO THE STUDY OF ANTISEPTICS.

By F. Boillat.

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The researches recorded in this paper were undertaken on account of the appearance of a pamphlet on disinfection by Dr. R. Koch. Koch states therein that of the substances at present employed as antiseptics, the only ones worthy of the name are chlorine, bromine, iodine, corrosive sublimate, with possibly potassium permanganate and osmic acid. This contradiction of generally-accepted facts is not, however, considered as strong as it appears. In medicine, and more especially in surgery, it is sufficient if the antiseptic employed is capable of preventing the formation of micro-organisms which are detected by examining the wound and its secreted matter under the microscope, and if the propagation of organ-

isms is arrested for a time long enough to allow the wound to heal, the antiseptic has fulfilled all its requirements. It has, however, never been determined why, by the use of a particular antiseptic, putrefaction and the development of micro-organisms is prevented, and it has been supposed in general that the substance destroys germs or arrests their propagation. It is evident, however, that substances such as phenol, chloride of zinc, acids, corrosive sublimate, ethereal oils, &c., all of which differ in chemical constitution, cannot possibly act on the vitality and development of bacteria in the same destructive manner. Moreover, a number of these micro-organisms, *e.g.*, the splenetic fever germs, form spores which, as may be expected, resist the action of antiseptics very powerfully. As soon as inquiries are made into the injurious effects of antiseptics on the development and vitality of organisms and their spores, it immediately becomes necessary to subject antiseptic agents to a proper classification. Koch, who has investigated the action of various antiseptics on definite species—*Monas prodigiosa* and *Bacillus anthracis*—by allowing the micro-organisms under examination to remain in the antiseptic medium for a longer or shorter period, and subsequently introducing them into a suitable nutritive solution, and regarding the appearance or non-appearance of their development and propagation as a criterion of their vitality, must necessarily have arrived at a different opinion with regard to the efficacy of many antiseptic substances. On reading through his work, however, one readily conceives that even his mode of judging the value of an antiseptic is one-sided. For instance, the spores of the splenetic fever organism, after being preserved for many days in a 1-per-cent. aqueous solution of phenol, or in a 5 per-cent. solution of calcium chloride, do not lose their capability of development; their propagation is, however, arrested, and this is of great importance for medical purposes; for although substances like chlorine, bromine, acids, &c., would effectually destroy bacteria and their spores, they would in most cases, when applied in the same, or even in less concentration, destroy the tissues of animal organisms. The application of such substances for the treatment of wounds is therefore not practicable. Until it is possible to find a specific poison for germs which will not injure the human organism, the antiseptics employed for the treatment of wounds will be those which are more or less injurious to the animal tissues, but severely injure the vitality micro-organisms, *i. e.*, arrest their development. The use of destructive disinfectants, such as alkaline solutions, &c., is adapted only, and that to a limited extent, to the preservation of lifeless objects. This explains why, *e. g.*, chloride of zinc, although it does not kill the spores of the splenetic-fever germs in a 5-per-cent. solution, is nevertheless a good antiseptic agent. Koch erroneously asserts that chloride of zinc does not possess the property of arresting the development of bacteria.

Most antiseptics are characterized by the circumstance that they coagulate dissolved albumen, forming permanent insoluble compounds therewith. On treating blood serum or egg albumen with a dilute solution of sulphate or chloride of zinc, Lieberkühn's zinc albuminate having the composition $C_{72}H_{112}N_{18}SO_{22} + Zn O_2H_2$, and containing 4.74 per cent. zinc, is formed, a similar reaction occurs when the wound is treated with the solution of a metallic salt. That chloride of zinc, corrosive sublimate, chloride of iron, &c., do not remain as such on the surface of the wound, but combine with the albumen of the tissues, there is no longer a doubt. In order to determine how far these metallic salts, when placed on wounds or brought into contact with albumen, act as antiseptics, it is necessary to investigate the behavior of micro-organisms on such albuminous metallic precipitates. Such experiments have, to my knowledge never been made, and at the suggestion of Professor Nencki I have undertaken this work in the interest of a rational study of antiseptics.

Samples of blood serum and egg albumen, the latter diluted with three or four times its weight of water were treated with an excess of solutions of phenol, chloride of zinc, sulphate of copper, and corrosive sublimate, and the resulting precipitates washed on filters until the wash water was free from the precipitant; 2 or 3 grains of the moist precipitate was then made up with water to a thin paste, and allowed to remain at the

ordinary temperature, loosely covered with bell-glass. Watch-glasses, containing fresh blood serum and Koch's nutritive gelatine, served to control the experiment. The results of these experiments are shown in the following table:

Albuminates.	Time after which the first Micro-organisms were observed. In Days.	Time after which putrefaction and bad smell were observed.	Remarks.
1. Serum........................	1	2	After the fourth day fungi appeared, which grew rapidly, covering the whole substance after ten days.
2. Nutritive gelatine.............	1	4	
3. Phenol albumen serum.........	2	6	
4. Phenol albumen (white of egg)	2	6	
5. Copper albumen......	28	40	After forty-six days the substance which had been light blue, changed and gave up the blue to the water. After thirty-one days fungi appeared which covered the whole of the substance in fifty-four days.
6. Zinc albuminate serum.........	31	46	After fifty-four days the substance assumed a dark color and strongly putrid smell.
7. Zinc albuminate (white of egg).	31	46	Ditto.
8. Mercury albumen serum.......	42	60	No fungoid growth.
9. Mercury albumen (white of egg)	45	60	Ditto.

In a second series of trials the same metallic albuminates (prepared from blood serum) were sown with a green coccus found on an infusion of coffee. It consisted of cocci of an average diameter of 1.0 micro-millimeter, partly isolated, and partly joined together in masses:

Albuminates.	Time after which the sown Fungus showed a distinct increase. In days.	Remarks.
1. Nutritive gelatine......	2	
2. Copper albumen.......	No growth	Even after the lapse of four weeks it was not possible to detect, either mucroscopically or microscopically an increase of the inocculated places.
3. Zinc albuminate.......	"	
4. Mercury albumen......	"	

In a third series of experiments splenetic-fever germs were dried on silk threads and brought into contact with the metallic albuminates.

Albuminates.	Time at which the Spores of splenetic-fever germs grew to threads. In Days.	Remarks.
1. Nutritive gelatine......	1	
2. Copper albumen.......	No growth	Even after four weeks no growth of the spores was observable.
3. Zinc albuminate.......	"	
4. Mercury albumen......	"	

The same experiment was repeated with splenetic fever bacilli. The latter rapidly shot out of the nutritive gelatine in threads, which contained spores, within the space of from three to four days. On the metallic albuminates, however, it was impossible to observe growth of any kind. From these experiments the following conclusions may be drawn:

1. The substances serving to control the first series of experiments exhibited fungoid growth in twenty-four hours, and showed distinct signs of putrefaction in from two to four days. The green micro-cocci, sown with nutritive gelatine, in the second series of trials increased perceptibly in two days, whilst the spores in the control substances of the third series of experiments had grown into threads.

2. The albumen precipitated by phenol, and subsequently washed, became putrid in the first series of experiments in forty-eight hours. This coincidence, remarkable on account of the permanency of the metallic albuminates, is explained in a simple manner. The coagulum of albumen produced by carbolic acid, when washed completely with water, was perfectly odorless, and on heating a large quantity of it with dilute sulphuric acid in a retort, to the boiling point, the distillate was perfectly free from phenol when tested with bromine. It is possible, therefore, to completely wash out the phenol from the phenol albumen precipitate, and this explains why the phenol albumen, like the substances used to control the experiment had become putrid.

3. The copper, zinc, and mercury albuminates proved to be unfavorable nutritive agents for micro-organisms. They would probably resist putrefaction for an unlimited period if they were not exposed to the action of atmospheric oxygen and water, by which action they appear to undergo gradual decomposition. It is an interesting fact that the antiseptic action of the inorganic metallic salts is the same as that of the corresponding albumen compounds. Mercuric chloride being the most powerful antiseptic, albuminate of mercury also resists putrefaction for the longest period.

My experiments explain the differences of opinion which exist between the statements of those authors who consider chloride of zinc a valuable antiseptic and the experiments of Koch, according to which, he fails to realize why chloride of zinc has ever received the name of a disinfectant. The metallic salts, when introduced into albuminous nutritive solutions or placed on wounds, immediately produce metallic albuminates, which compounds, although *per se* not poisons for germs, are no longer suitable for their nutrition, so that if, *e. g.*, chloride of zinc is placed on the wound, or introduced into the nutritive solution in sufficient quantity to convert the whole of the albumen into zinc albuminate, the latter would resist decomposition by fungoid matter for a long time. Thus, *e. g.*, Amuat, in his experiments for preventing the putrefaction of pancreas, used, for 30 grams of the latter, 300 grams of a 1 per cent. solution of chloride of zinc. There is no doubt that the 3 grams of chloride of zinc employed were more than enough to coagulate the albumen contained in 30 grams of new glanders. In order to test the action of chloride of zinc in arresting the development of germs, Koch made the following experiment. He added to 10 c. c. of the serum of blood a solution of chloride of zinc, so that the total liquid contained 1 per cent. of chloride of zinc; a second quantity of serum was treated so as to contain 5 per cent. of chloride of zinc in the total solution. Silk threads, with the spores of the splenetic-fever organism, were then introduced into the solutions and examined under the microscope. After the lapse of twenty-four hours the spores contained in both vessels had grown to threads, their vegetation having been the same as that observed with the substances serving to control the tests. It is not stated whether the quantity of chloride of zinc employed was sufficient to precipitate the whole of the albumen and retain an excess of chloride of zinc in solution. If, however, as appears probable, the quantity of chloride of zinc was only large enough to throw down the whole of the albumen contained in the serum, the experiment has no meaning. The chloride of zinc is decomposed into hydrochloric acid and zinc albuminate, and it is easily conceivable that the spores have grown to threads, having found enough nourishment in the unprecipitated portion of the albumen, and in the remainder of the

constituents of the serum. From a similar reason corrosive sublimate, which, according to the researches of Buchholtz, surpasses all other antiseptics when added to solutions of albumen, is converted into mercury albuminate, and no longer possesses the same antiseptic properties which characterize chloride of mercury. Koch injected 1 gram of a 1 per cent. solution of corrosive sublimate into a Guinea pig, and inocculated it on the same day with splenetic-fever bacilli. The next day the inocculated places were much reddened and swollen. The Guinea pig then received, on the morning of the second day, 2 grams of the same solution of corrosive sublimate. According to Koch's estimation, this quantity would be sufficient to prevent the growth of bacilli in a nutritive solution equal in weight to the whole body of the animal experimented with. The animal, nevertheless, died during the following night, from splenetic-fever. The explanation of this is very simple. The 3 milligrams of corrosive sublimate injected into the pig were sufficient to convert only a small portion of the soluble albumen into mercury albuminate. As the latter is readily soluble in an excess of albumen, and also in solutions of common salt, and is not a direct poison for micro-organisms, the injected solution of corrosive sublimate could not exercise any influence over the inocculated splenetic-fever bacilli.

In the course of my investigations I have tested the antiseptic action of another antiseptic agent, the use of which for surgical purposes, although considerable at first, has been abandoned, viz., iodoform and the chemical compounds related thereto. As the pancreas and the liver of animals contain large quantities of micro-organisms, and the soluble ferments contained in the glands of the stomach promote putrefaction, I used the pancreas of oxen, as being the most reliable criterion of testing these substances for their capability of arresting the development of germs. 20 grams of pancreas and 2 grams of iodoform were added to 100 grams of water; the mixture was well shaken, and allowed to stand at a temperature of from, 35° to 38° Centigrade, the whole had a strong odor of iodoform. After twenty-four hours' standing the pancreas became putrid, as though it had been in pure water. The experiment was then repeated with the following modification: 10 grams of perfectly fresh pancreas was intimately mixed with 2 grams of iodoform, and treated with enough water to cover it and prevent it from drying. The temperature was 35° to 38° Centigrade. After the lapse of twenty-four hours this mixture became as putrid as the first. As, according to the opinions of most experimentalists, iodoform, when applied to wounds, acts as an antiseptic, inasmuch as it is gradually decomposed with the liberation of iodine, and the latter is the real active agent, I tried to replace the iodoform by carbon tetri oxide, CI_4, a substance prepared a few years ago by Gustavson. Its preparation in a pure form and on a large scale was, however, very troublesome. I have, on the other hand, examined the antiseptic action of the three carbon chlorides, viz., carbon dichloride (C_2Cl_4) carbon hexachloride (C_2Cl_6), and carbon tetrachloride (CCl_4). Experiments were made also with the two bromotoluenes—the solid and liquid—with pyrogalloldimethylether and paracresol. The results, which are illustrated in the subjoined Table, show that cresol only arrests the development of fungoid germs, its action being equal to that of phenol. The other substances, which were used in the proportion of 1 to 100 of water, were perfectly inactive. In these experiments 20 grams of pancreas were treated with 100 grams of water, and digested with the substance under examination at 35° to 38° Centigrade.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—At the meeting of the Society, held Oct. 7, the Secretary announced the appointment of Mr. L. G. F. Bouscaren, Mr. Robert Moore and Mr. W. Howard White, as a board of censors to award the Norman Medal; the board to award the Rowland prize, will be Mr. Mendes Cohen, Mr. E. P. North and the Secretary of the Society. The deaths of Wilmon W. C. Sites, Member, and Thomas C. Durant, Fellow of the Society were announced. The members of the Society adopted by letter-ballot a resolution, presented at the Deer Park Convention, appointing a committee of five members to consider and report to the Society on the proper relation which the form of the head and rail should bear to the section of a car-wheel tread and flange.

The paper of the evening was read by Mr. Edward Bates Dorsey, it being a supplement to

his paper on "English and American Railroads Compared," presented at the Annual Convention of June 24. In this supplement Mr. Dorsey compared in detail the operating expenses of the systems of England and the United States. In point of traffic it seems that the American railroads average more than double freight, and only eleven per cent. less passenger traffic than the English. The Boston and Albany R. R. is shown to have a larger passenger and freight traffic than any of the large trunk lines of England.

The following gentlemen were declared elected to the classes named.

For Members—William Price Craighill, Lieut.-Col. of Engineers, U. S. A., Baltimore, Md.; Hiram Stevens Maxim, Managing Director and Mechanical Engineer of the Maxim Gun Co., London, Eng.

For Juniors—Bernard Frank Booker, Brooklyn, N. Y; formerly engaged upon Atchison, Topeka and Santa Fé R. R., and recently Division Engineer, Tampico Branch, Mexican Central R. R.; Thomas John Brereton, Maintenance of Way Department, Pennsylvania R. R., Altoona, Pa.; Joseph Maxwell Carrere, New York Steam Co., New York; recently engaged upon Burlington and Missouri River R. R.; Charles P. Matlack, City Engineer, San Antonio, Texas; formerly engaged with Keystone Bridge Company, on United Pipe Lines, Bradford, Pa.; on International and Great Northern R. R., Texas; on Mexican National R. R.

ENGINEERING NOTES.

Maximum Wind Pressure.—During the hearing by the Parliamentary Committee, of the case for the promoters of the Tower Bridge over the Thames, Mr. B. Baker, who was called to prove the stability of the proposed structure, gave evidence upon the phenomena of wind pressure as observed by him in connection with the construction of the Forth Bridge. Mr. Baker stated that, from recorded observations in the Firth of Forth, extending over many years, he has come to the conclusion that no pressure at all approaching 56 pounds per square foot can prevail over a surface of any magnitude. He declared that no such pressure has for many years occurred in the Thames valley, instancing, in proof of this assertion, the number of large gasholders scattered up and down the river side. If a hurricane of 56 pounds to the square foot had encountered any of these structures, Mr. Baker believes they would have been doubled up and blown across London, as they have no power of resistance to external pressures beyond the pressure of the gas from within, which he values at not more than 18 pounds per square foot. If, therefore, not the slightest damage of any kind has ever been done by wind to any of the London gasholders, which is a fact, it is a demonstration that they have never been exposed to a pressure of 56 pounds per foot. It is Mr. Baker's experience at the Forth Bridge works that a gale, registering by his improved instruments not more than 16½ pounds per square foot, completely stops all ordinary traffic on the estuary, preventing the running, even, of powerful ferry-boats. Mr. Baker believes that this pressure is rarely exceeded. He declines to place credit in ordinary anemometer readings, which sometimes show extreme velocities, and he points out that trains do not cease running in gales when anemometers will register 46 pounds pressure to the foot, though a pressure of 40 pounds of wind per square foot on its exposed side would certainly upset an ordinary train.—*Engineering News.*

A piece of very rapid well-boring is reported by Messrs. Le Grand and Sutcliff, at Brick Kiln Farm, Wolverton, near Stony Stratford, where they bored 50 ft. of artesian boring, of 5 in. diameter, in a single day of eleven hours. Previous to this, they say, 40 ft. was the greatest depth reached in one day.

Lieut. Hilliard, navigating officer of the Orion, has made a long and careful survey of the entrance to Alexandria. He finds that the channel to the east of the present Boghaz Pass requires very little deepening, by dredging or dynamite, and the ground being soft the expense would be small. The channel would be available night and day, and is desirable especially for the English Navy. As at present, the Orion is the only large ship of the Mediterranean fleet able to enter the harbor.

A new trans-Alpine line, the St. Bernard Railway, is likely to be commenced before very long, and to be, when completed, a dangerous competitor for the through traffic with the already existing route of St. Gothard. One of the principal features of the new project is that the indispensable tunnel under the Alps—at the Col Ferret—will be very much shorter than any other, either constructed or proposed to be constructed. The length will be only 9¼ kilometers (5¾ miles), while the St. Gothard tunnel is 15 (9¼ miles), the Mont Cenis 12, and those under the Simplon and Mont Blanc 20 and 19 kilometers respectively. The total length of the St. Bernard line will be but 138 kilometers, or 86 miles, making a saving between London and Brindisi over the St. Gothard route of 59½ miles.

At a recent meeting of the Berlin Physical Society, Dr. König produced a new apparatus for the measurement of the modulus of elasticity, which was constructed according to the suggestions of Herr von Helmholtz, and was utilized in the Institute for measurements of elasticity. The modulus of elasticity was determined by loading in the middle a bar of the substance to be examined, resting both ends on firm supports. The flexion which set in was measured by means of the cathetometer, and, its value being introduced into the formula of the elasiic theory, furnished the modulus of elasticity. A source of error in these measurements arose from the circumstance that the bar resting on edges was in part pressed in and sank, as a whole. This depression was the greater as the loading was greater, and it added to the magnitude of the deflex. To avoid this disturbance in the account, Professor Kirch-

koff, in 1859, placed horizontal mirrors on the two ends of the bar, and, by means of telescope and scale, observed at each side the change in situation of each mirror, a change which occurred in consequence of the deflexion under the loading in the middle, and which produced on both sides an opposite displacement of the scale. The sinking of the bar on account of the pressure on the edges, and even a slanting position on the part of the whole bar, exercised no influence in these measurements. The apparatus suggested by Professor von Helmholtz developed this principle still further. It had two perpendicular mirrors, with the reflecting surface directed inwards at the two ends of the bar; on one side stood a scale, on the other a telescope. The image of the scale fell on the opposite mirror, then on the second mirror, and thence into the telescope. If, now, the bar were loaded so that deflexion occurred, then the image in the telescope became displaced to the extent corresponding with the angular changes of the two mirrors. By glancing, therefore, into the telescope, the whole amount of deflexion might be very rapidly and conveniently measured, and the loading altered at pleasure. The commencement of the elastic after-effect might likewise be directly observed with great facility.

Probably few cities on the Continent have such a complete, and yet so novel, service of tramways as the free city of Hamburg. Scarcely a strasse of any importance is without its steam or horse tramway, whilst in a great number of the streets in Hamburg and Altona the peculiar feature is the adoption of a vehicle that can be run either upon the tram-lines as a tramcar, or upon the ordinary road as a carriage. The conveyance in question has five wheels: four ordinary coach-wheels, with a radiating leading axle, when used upon the paved roads, and when used upon the tram-lines a small flange wheel, under the control of the driver, is lowered upon the rail, when by its flange running in the groove of the rail, the car is kept on the metals, and assuming the curves to be properly constructed, no difficulty is experienced, whilst in the event of any obstruction upon the line, the matter of diversion of the car is exceedingly simple. There are three different tramway companies in the city, the most recent one being the Hamburg, Altona, and North-Western Tramway Company. Until recently, this company's terminus was the Millernthor, near St. Pauli, some distance from the center of the city. This state of things will, however, soon cease to exist, the company having obtained a further concession, which will enable them to run their cars to the Rodins Markt, the necessary works of line construction, depots, now being carried out by the contractor, Mr. John Fell, of Leamington; the engineer of the company being Mr. E. Pritchard, M. Inst. C. E.; and when completed, it is confidently assumed, a great increase in the receipts will be the result.

An American contemporary gives an account of a great irrigating canal scheme now in progress in Merced County, California. Merced, the capital of the county, lies in the heart of the San Joaquin Valley, which has been for some years the chief wheat-producing section of the State. The valley extends from the Sierra Nevadas on the east to the skirt of the coast range on the west, its greatest width being ninety miles, and its length from north to south about forty miles. The town of Merced is expected to make enormous strides when the canal is completed. The first side of the ditch suggests the earthworks of a fort, the ground being ridged up from 6 ft. to 8 ft. There are 300 men at work on the canal. The undertaking was begun on March 14th, 1883, and has been carried on continuously ever since. The canal will run across the country from the Merced river just above Snelling to Plainsburgh, ten miles below the city of Merced, on the Southern Pacific Railroad. The slope during this whole distance is a gradual one, and the canal is carried well up on a slight elevation, so that without artificial means the water will flow over the wide extent of level valley land which is to be irrigated. The entire length of the canal will be thirty-five miles, of which sixteen miles are now completed. In the portion which has been built there is one tunnel a trifle over a mile long, and another of 1600 ft. is now being excavated. The general grade of the canal is 1 ft. to the mile. Among the hands employed are 150 Chinese, who are excellent workmen. They receive $1 a day and board themselves, while the white men receive $20 a month and their board. The Chinese live in a camp by themselves and run their own commissariat. The company who have charge of the enterprise have expended already $700,000, and it is roughly estimated that the entire cost of the work will be double that sum.

The *Petit Marseillais* says that the underground telegraph line from Paris to Lyons and Marseilles is completed, and the wires will be used as soon as the instruments destined for them are placed in the offices at Paris, Lyons, and Marseilles. Underground lines will also be laid between Marseilles, Toulon, and Nice. Marseilles and Havre are already connected by a direct wire, and this not only secures rapid communications between the two ports, but places Marseilles in direct connection with the United States, as the Marseilles-Havre wire is to all intents and purposes the prolongation of the Franco-American cable which abuts at Havre.

According to American advices, the Panama Canal Company is in difficulties. Some time ago a New York Engineer was sent to Panama to examine into the affairs of the Canal Company in the interests of a New York syndicate, who proposed to contract for building the canal. It is sufficient to say that his advice was to wait for the crisis which was near at hand. Up to September, 1884, M. de Lesseps and his company had raised 111,000,000 dollars and expended 104,000,000 dollars, their liabilities being 153,000,000 dollars, their securities being sold at a discount. May 1st, 1885, less than 10 per cent. of their excavation, or 12,-876,500 cubic meters, had been completed, the total being estimated at from 125 to 150 million meters, and there is the dam for the Chagres

river, for which no foundation has been found after boring to the depth of 60 ft. The entire cost of the canal is estimated at not less than 530,000,000 dollars, representing liabilities amounting to 600,000,000 dollars. The *Financial News* estimates that upon this scale of liability there will be an annual deficit of ten millions of dollars.

IRON AND STEEL NOTES.

The Rolling Qualities of High Phosphorus Steel.—One of the points in connection with the Clapp-Griffiths process which has caused prominent engineers to hesitate in adopting it, has been the doubt as to the practicability of rolling high phosphorus steel. A test recently made at the Edgar Thomson Steel Works has, therefore, particular interest. Two 14-inch ingots from the Oliver plant were charged into a hot furnace, and after remaining in it for two and a-half hours, they were given four passes on the blooming train. The steel rolled exceedingly well, the reduction on the first pass being 2 inches, and on the second 1½ inch. After being rolled to 10½ inches square, the ingots were taken to the forge, and were reduced to 2½-inch by 14-inch slabs. The experiment, which was made to ascertain whether the steel would stand the heavy reductions of regular blooming mills, was entirely satisfactory. The chemical composition of the steel was: Phosphorus, 0.318; carbon, 0.1; manganese, 0.648; sulphur, 0.047; silicon, 0.008 per cent. We understand that on one occasion, by an accident, no manganese was added to the charge, and yet the steel rolled well, and since then steel has been made without adding ferromanganese, which rolled as well as steel made in the ordinary way.

RAILWAY NOTES.

The Expansion of Rails by Heat.—Major Marindin, in his report to the Board of Trade on the causes of the accident which occurred on August 2, on the Great North of Scotland Railway, between Martle and Inveramsay stations, states that the accident was due to the distortion of the line, the rail on the right side (and according to one witness, the corresponding rail on the left side) being bent for over an inch at a point 42 feet behind the mark of the wheel flange on the left rail. As there was no defect in the engine, and the driver's estimate of the speed—viz., 25 miles an hour—is probably correct, Major Marindin can only attribute the distortion of the rail to an expansion of the metal from the heat of the sun, as it is in evidence that the sun was exceedingly hot upon the day in question. He considers that the only way of guarding against such accidents as this would be by having a midday examination of the line in exceptionally hot weather, especially upon lines where the permanent way is not of the heaviest type. It will be remembered that in this accident, as the 12.25 P. M. up passenger train from Macduff to Inveramsay was running down an incline of 1 in 177, about 1½ mile north of Inveramsay, one of the carriages, probably the second from the engine, left the rails. The train was running at a speed of about 25 or 30 miles an hour, and the engine ran for about 235 yards before coming to a stand, when it was found that the leading wheels of the leading carriage were off the rails, the second carriage was upset on its right side, the third was off the rails across the line, and the rear vehicle, a break van, had broken away from the train, and was lying against the bank on the left side of the line, about 147 yards behind the rear passenger carriage. The couplings between the tender and leading vehicle were not broken, but the screw couplings between the first and second, second and third, and third and fourth vehicles were broken, and the side chains between the first and second vehicles were loose, between the second and third were holding, and between the third and fourth were broken at the hooks.

The number of tons of freight transported on American railroads in 1884 equaled 390,074,749, against 400,453,439 tons in 1883, the falling off equaling 10,378,690 tons, the rate of decrease being about 2½ per cent. The value of the tonnage moved in 1884, estimating its value at $25 the ton, equaled $9,751,868,725. The number of tons transported one mile in 1884 equaled 44,725,208,277 tons, against 44,064,923,445 tons moved one mile in 1883, the increase of service performed for the year equaling 660,284,238 tons moved one mile, the rate of increase being about 1½ per cent.

An American contemporary says: "The Pike's Peak Railway, which is expected to be in operation this year, will be the most notable piece of track in the world. It will mount 2,000 feet higher than the Lima and Oroya Railway, in Peru. It is now in operation to a point over 12,000 feet above the sea level. The entire 30 miles of its length will be a succession of complicated curves and grades, with no piece of straight track longer than 300 feet. The maximum grade will be 316 feet to the mile, and the average grade 270 feet. The line will abound in curves from 500 to 1,000 feet long, in which the radius changes every chain."

The rates per ton per mile for 1884 of freight carried on the United States Railways equaled 1.124 cent, against 1.236 cent for 1883, the falling off equaling 1.12 mill per ton per mile. Had the rates for 1883 been maintained for 1884, the earnings from freight would have been $553,694,042, in place of $502,869,901, the amount actually received. Had the rates of 1883 for the transportation of passengers and freights been maintained for 1884, the gross earnings of all the roads would have been $827,525,371, exceeding by $56,840,463 the amount actually received, and greater, by $3,752,447, than the earnings for 1883. It will thus be seen that the decline in the earnings for the past year was due wholly to the reduction in rates charged.

During the six months ending June 30th, 435 failures of tires and 169 failures of axles took place on British railways. Of the 435 tires which failed, 9 were engine tires, 8

were tender tires, 2 were carriage tires, 7 were van tires, and 409 were wagon tires; of the wagons, 307 belonged to owners other than the railway companies; 388 tires were made of iron and 47 of steel; 11 of the tires were fastened to their wheels by Gibson's patent method, 6 by Mansell's, and 1 by Beattie's, none of which left their wheels when they failed; 410 by bolts or rivets, two of which left their wheels when they failed, and seven by other methods, one of which left its wheel when it failed; 16 tires broke at rivet holes, 5. in the solid, and 362 split longitudinally, or bulged. Of the 169 axles which failed, 99 were engine axles, viz., 86 crank or driving, and 13 leading or trailing: 6 were tender axles, 2 were carriage axles, 60 were wagon axles, and 2 were axles of salt vans. Twenty-nine wagons, including the salt vans, belonged to owners other than the railway companies. Of the 86 crank or driving axles, 66 were made of iron and 20 of steel. The average mileage of 66 iron axles was 229,569 miles, and of 20 steel axles, 202,715 miles.

THE number of persons transported in 1884 by all the American lines was, according to *Poor's Manual* 334,814,529, against 312,686,-641 for 1883, the increase for the year being 22,127,888, the rate of increase equaling 7.8 per cent. The number of passengers carried one mile in 1884 equaled 8,778,581,031, against 8,541,309,674 in 1883, the increase equaling 237,271,387 persons carried one mile, the rate of increase equaling very nearly 3 per cent. The distance traveled by each passenger in 1884 equaled 26.24 miles; in 1883, 27.32 miles. The amount received per passenger per mile equaled 2.356c in 1884, against 2.422c. in 1883. Had the passenger rates for 1883 been maintained for 1884, the earnings from this source would have equaled \$212,617,233—a sum \$5,-826,532 greater than that received

THE FIRST FRENCH RAILWAY.—In two years' time France will celebrate the jubilee of the establishment of railways. The first French railway—that from Paris to Saint-Germain, which became the nucleus of the Great Western of France—was opened on August 27, 1837. Its construction was sanctioned just fifty years ago. The late Emil Péreire undertook to make this line of 18 kilometers (10 miles) at his own cost and risk. It had taken nearly three years to obtain the consent of the authorities, the contention of Thiers being that railways could never be more than a mere toy, while Arago also doubted their utility. The requisite capital of 6,000,000 francs was not easy to raise, although two bankers, Eichthal and Thurneyssen, had deposited the 200,000 francs caution money; but the difficulties were surmounted when Péreire won over the Rothschilds and Samson Davilliers. France has now 31,000 kilometers (19,220 miles) of railways, conveying 180,000,000 passengers a year, and the gross receipts are 1,150,000,000 francs. Two hundred and twenty-three thousand persons are employed on these railways, and the State derives a revenue of 83,000,000 francs from them.

THE approximate weights of the fast trains on the New York division of the Pennsylvania Railroad are given as follows by the St Louis *Railway Register*:—Engine, ready for service, 96,700 lbs.; tender, ready for service, 56,300 lbs.; two men on engine, 300 lbs.; one combined car, 30,000 lbs.; one parlor car, 50,000 lbs.; two passenger coaches, 80,000 lbs.; 140 passengers, estimated 21,000 lbs.; total, 342,300 lbs. Coal, estimated 5,000 lbs.; water, 3,700 gallons: average schedule speed, 48.01 miles; maximum schedule speed per hour, 55.08 miles; distance from Jersey City to Philadelphia, 89.06 miles.

THERE appears to be a basis of fact for the *National Zeitung's* assertion that the Chinese government has at last decided to adopt a "forward policy" in regard to railways, and that a contract has been signed with a Manchester firm for making a line from Taku to Tong-Chow. This firm has undertaken to supply the material and the rolling stock, while the Chinese government is to furnish the capital and the labor. The construction of the line will be left in the hands of the Manchester firm, which will also work it when open. The loan of 100 million florins, for which the Chinese government is now negotiating with several Dutch and German banking houses, is doubtless, thinks the *National Zeitung*, connected with this project. The London *Times* and other journals have lately been eloquent on the grand future for enterprise in China.

ORDNANCE AND NAVAL.

AN important addition is made to the unarmored fast cruisers of the Royal Navy by the launch on the Medway of the new steel cruiser Severn, twelve guns, 3,550 tons, 6,000 horse-power. The Severn, which has been built at Chatham Dockyard, is a more powerful vessel than the cruisers of the Leander type, and will also possess a greater steam power, her engines being estimated to produce an additional indicated horse-power of 1,000. The vessel will be mounted with twelve 6-in. steel breech-loading guns, on the Vavasseur system, twelve Gardner and Nordenfelt machine guns, and Whitehead torpedoes.

IT is said that a new substance for ships' armor has been satisfactorily tried. It is obtained from cocoanut cellulose, and has the property, when penetrated by shot and shell, or even after the explosion of a torpedo, of closing up as rapidly as it has been perforated, and thus preventing the influx of water into the ship's hold. Some important experiments have lately been made with the composition before a French commission at Toulon. The commission submitted the composition to a three-fold test: against shot, shell, and torpedo. The target was a cofferdam, made of a mixture of fourteen parts of pulverized cellulose, and one part of cellulose in fiber. This composition was compressed to a felt-like mass, of which one cubic meter weighed 120 kilograms, or one cubic foot—about 8 lbs. A layer of beams 4¾ in. thick represented the side of the ship, behind which there was a layer of the new material 2 ft. thick. Against this target a 7¼-in. solid shot was fired, which pene-

trated it, taking with it not quite one-fifth of a cubic foot of composition—a very small quantity, considering the size of the shot. But as soon as the shot had passed through the target the cellulose composition closed up again, and so firmly that a strong man was unable to force his arm through the opening made. A box filled with water was then fixed against the aperture, the contents of which ought to have acted in the same way as if the cofferdam had been washed by the sea. It was observed that a few drops of water began to percolate after the lapse of from ten to fifteen minutes, and even after the composition had become well saturated with water, only between three and five pints of water escaped per minute, which could easily be intercepted by pails. As soon as the cellulose had become thoroughly soaked and grown denser it offered greater resistance to the percolation of water, which finally almost ceased to flow.

A Scandinavian correspondent writes that in the early part of September some important experiments were to be carried out in the Sound, with the last engine of torpedo warfare, constructed by M. Nordenfelt, viz., a submarine torpedo boat, built at Karlsvik, near Stockholm. "This terrible adjunct of our modern destructive warfare is fitted with engines indicating 100-horse power, and will, it is said, easily attain a speed of fifteen miles an hour above, and thirteen miles below, the surface of the water. When the boat is about to attack a hostile vessel, she is sunk to the required depth by the admission of water into tanks, but it is only intended to be submerged to the depth of a foot or two when about to attack. On catching the shadow on the water of the unsuspecting war ship, the Whitehead torpedoes, with which she is equipped, are discharged against the bottom of the vessel, which will have the effect of sinking her. When the work of destruction is complete, the boat re-emerges from the water by the operation of special automatic machinery. The hull itself, which is constructed of Swedish soft steel, of a minimum thickness of ¼ in., is of the cigar pattern, and is only with difficulty visible, even when floating on the surface. The length of the boat is 64 ft., and the diameter about 8 ft., the engine room being 7¼ ft. in height, and the gross weight of the whole vessel when fully manned and equipped is 60 tons. A sort of bell-shaped glass cupola rises from the center of the boat, into which the steersman puts his head when under water, thus commanding an all-round view, and enabling him to direct the general movements of the craft. In case of accident, the vessel is divided into water-tight compartments, and extra pumping machinery is provided, to be used in the event of any portion of the automatic machinery failing to raise the vessel to the surface. The crew consists of three men, and the armament of four torpedoes, two being of the "fish" pattern, and two of the ordinary spar kind. Against such an insidious foe as the boat constructed by Nordenfelt, it is obvious that the ordinary wire netting for the defence of ironclads from the hitherto employed torpedo boats will be useless; and if the boat fulfils expectations, warfare with our present huge vessels promises to be in serious jeopardy."

Gunnery Experiments.—The authorities of the War Department desire to have it known that the damage done to a gun during the recent experiments with high explosives at Lydd was due to no fault of the gun, but to the explosion inside the bore, of a shell charged with the most violent of all explosive agents—blasting gelatine. The object of the experiments was to ascertain how far the tendency of such like varieties of the nitro-glycerine series to detonate on slight provocation could be brought under control, it being very desirable, if practicable, to use something more powerful than gunpowder for shell charges. The dangerous compounds were therefore packed and padded as carefully as possible in their shells, in the hope of overcoming or reducing the concussion of discharge when the gun was fired; but it was known that premature bursts were likely to occur at any moment, and it was also known that if one of the shells burst within the gun, however strong the gun might be, the gun must "go." So far as the results are allowed to transpire, it may be inferred that very few of the shells remained intact until they reached the target. Several broke up after leaving the muzzle, and one charged, as aforesaid, with blasting gelatine, burst inside, with the natural consequence of destruction to the gun. As the experimental committee have been congratulated on the absence of accidents, it may also be said that in all such hazardous investigation they keep well under cover and fire the guns by electricity. For some time past it has been the practice at Woolwich Arsenal to try test tubes of steel, an inch or so in diameter, to prove the quality of the metal, the artillerists being in want of a lining for their guns which shall resist the erosive action of the powder and projectiles, and some of these have failed at proof. This, also, has given rise to a report that guns have burst at the proof butts, and this report the authorities wish likewise to correct by this explanation.

Ten and Twelve-inch Armstrong Guns at Cadiz.—Major C. Jones, late R. A., gives an account in the current number of R. A. "Proceedings" of a trial of 10-in. and 12-in. guns supplied by Elswick to Spain, which took place at Cadiz, last December. It is not necessary to give all the details, because the guns, which were put in hand in 1882, are not of the newest pattern. They are both breech-loaders, made of steel and coils of iron, with Elswick obturators. The 10-in gun weighed 26 tons, the 12-in. 48 tons, having barrels something over 25 calibers long. The projectiles weighed 400 lbs. and 700 lbs. respectively, and were fired with charges of 200 lbs. and 310 lbs., having muzzle velocities of 2,025 and 2,000 feet-seconds and 11,374 and 19,415 foot-tons muzzle energy. Calculated perforation at muzzle, 19.5 in. and 23.5 in. of iron.

The mounting, and security of pivot and foundations having been tried, chilled projectiles were fired at armor targets. The back of the target before it was made up for firing,

had a sand-bag butt to prevent movement of the target and to stop the shot. The target consisted of four thicknesses of plates, each 10 c.m. (3.94 in.) thick, 15¾ in. in all, backed by 25 in. of wood, to which it was bolted. The 12-in. gun fired three rounds at this structure, of which the following table gives the details:

Round.	Weight of charge.	Weight of chilled projectile filled with sand.	Striking velocity at 246 yards.	Recoil.	Range represented by striking velocity.	Penetration.
	lbs.	lbs.	ft. per sec.	inches.	yards.	
1	220.0	690	1575	41.5	2950	Through target and about 18 ft. into sand butt.
2	220.0	695	1588	41.5	2850	Ditto.
3	154.8	689	1440	27.0	4080	Through target and about 10 ft. into sand butt.

The three projectiles struck the spots aimed at exactly, the plate being as shown in the table, 225 meters—246 yards—from the muzzle of the gun. The three projectiles were dug out from the sand at the back of the target. The first two were entire, the third one was partly broken. That the 12-in. gun shot with 1,575 ft. velocity should perforate 15¾ in. in four layers is only to be expected. The projectile having a sectional density expressed by $\frac{W}{D^3}$ =0.4, the rule of thumb of 1 caliber perforation for every 1,000 ft. velocity holds good very nearly—that is, the perforation due to 1,575 ft. velocity is not far from 18.9 in. It actually works out 18.3 in. for a single plate, and for one made up to four thicknesses, 20 in. The projectiles were therefore sure to perforate. The noteworthy feature is that they held together so well after perforation. They were selected haphazard from the supply, and therefore show that at Elswick a remarkably high degree of excellence has been reached in chilled or Palliser projectiles. The same thing has taken place at Woolwich. It is not a matter of unqualified satisfaction to us to notice this, because we fear it tends to keep back the development of steel shells. So long as soft iron armor is fired at, the most excellent steel can offer hardly any advantage over chilled iron for projectiles, beyond that of holding better together after perforation is accomplished. Even against some steel-faced armor or very soft steel, it appears questionable if steel shells offer any commensurate advantage for their great cost. Unfortunately, against really hard armor we have reason to believe that chilled iron could not compare with steel at all, and this class of armor is seldom fired at in this country

To return, however, to the Elswick guns, we have only to add that the trial is reported as satisfactory in all respects. The feature that chiefly strikes us is the excellence of the projectiles.

THE NEW TORPEDO BOATS.—A large sea-going torpedo boat, the first of the series of forty which the country owes to the recent popular agitation on "The State of the Navy," was tried in August in the Thames. The vessel has been built by Messrs Yarrow & Co., of Poplar, being one of twenty that the government has ordered of that firm. The trial was, according to present regulations, for two hours' continuous steaming at full speed, and during that time, and as nearly as possible in the middle of the two hours, six runs were made on the measured mile. A mean speed of 19¼ knots was realized, 19 knots being the guaranteed speed, with an air pressure in the stokehold of only 3¼ in., as shown by the air gauge. The boat is 125 ft. long, 13 ft. wide and 8 ft. deep. She has, naturally, far more accommodation than the first-class torpedo boats hitherto constructed, being able to berth well a crew of twelve or thirteen men forward, whilst there is comfortable room for the officers aft. Special care has been taken to provide efficient ventilation in the new boats, and it is hoped that the great discomfort hitherto found when at sea for any lengthened period will be materially reduced. There is one tube forward for ejecting torpedoes right ahead, and arrangements are made for firing four torpedoes from either side, or two from one side and two from the other at the option of the officer in charge. The number of torpedoes carried will be five, one in the bow gun and four in four guns for side firing. It will thus be seen that there are five torpedoes all ready to be discharged at a moment's notice. This is considered a far better arrangement than hampering the boat with a number of spare torpedoes, of which none will be carried. There will also be two machine guns, one being placed on the top of each conning tower. There are two conning towers, one forward and the other aft. Provision is made for steering the vessel from either of these towers, so that should one get damaged in action the other will be available. The four side-firing torpedo guns are fixed two to each conning tower in such a manner that they can be made

to revolve so as to secure any angle of fire, which plan was originated by the authorities of the Vernon. The impulse by compressed air is to be superseded by the simpler and equally efficient system of ejecting by gunpowder The engines are of the usual type fitted by Messrs. Yarrow in vessels of this class, the cylinders being 14½ in. and 26 in. in diameter by 16 in. stroke. The boiler is of the locomotive type, and contains the usual special features introduced by Messrs. Yarrow & Co. for torpedo boat work. The total heating surface is 1,200 square feet, and the grate surface, 30 square feet. The indicated horse-power on trial was not accurately obtained, but is estimated at 700, the steam-pressure being 123 lbs., and the engines running at 376 revolutions a minute. It was noticeable that throughout the two hours' trial the speed of the engine only varied within the small limits of 1½ per cent. more or less than 376. It is estimated that sufficient coal can be carried for a continuous run of 2,000 knots at a speed of ten knots an hour, the bunkers holding about twenty-three tons. This most recent addition to our torpedo fleet would undoubtedly prove a very formidable antagonist at sea, being sufficiently powerful to operate in any reasonable weather. She is the result of the accumulated experience of several years, and the country is to be congratulated in having got her and her sister vessels well to the fore before they are actually wanted.

BOOK NOTICES.

Publications Received.

Tidal Theory and Tidal Prediction. By E. A. Gieseler. Pamphlet.

The Minting of Gold and Silver. By Albert Williams, Jr.

Placer Mines and Mining Ditches. By Albert Williams, Jr.

Proceedings of the Institution of Civil Engineers:

No. 2044.—The Signaling of the London and North Western Railway. By Arthur Moore Thompson, M. Inst. C. E.

No. 2064.—The Copper Mines of Butte City. By David William Brunton, M. Inst. C. E.

No. 2069.—Rivers Flowing into Tideless Seas. By William Shelford, M. Inst. C. E.

No. 2084.—Spanish Tidal Flour-Mills. By Alexander Fairlie Bruce, Assoc. M. Inst. C. E.

No. 2097.—The Semi-circular Timber Roof-Truss. By Gilbert Richard Redgrave, Assoc. M. Inst. C. E.

Lectures on the Principles of House Drainage. By J. Pickering Putnam. Boston: Ticknor & Co. Price 75 cents.

There are three parts to this compact little work. The first part deals with Traps exclusively. Good and bad forms of construction are described; the evils to be avoided, and the way to avoid them, are presented and illustrated by good diagrams.

Wash-basins and Water-closets are fully described in the second part. The illustrations are numerous and good.

Part three deals with Soil and Drain Pipes. First, the materials are considered, and then the methods of jointing. The illustrations of this subject are not so numerous, but evidently sufficient for the purpose.

The work will prove valuable to builders or house designers.

The Mathematical Theory of Electricity and Magnetism. By H. W. Watson, F. R. S. and S. H. Burbury, M. A. Vol. I. Oxford: Clarendon Press.

The authors regard this treatise as an introduction to, or commentary upon, Clerk-Maxwell's work, and is, therefore, more widely useful to the average student. By this, however, is meant the student of mathematics, not of applied electricity.

The copies presented in the present volume, dealing with Electrostatics only, are: Green's Theorem; Spherical Harmonics; Potential; Description of Phenomena; Electrical Theory; Application to particular cases; The Theory of Inversion applied; Electrical Systems, in two dimensions; Systems of Conductors; Electrical Energy; Specific Inductive Capacity; The Electric Current; Thermoelectric Currents; Polarization of the Dielectric.

Modern Molding and Pattern-Making.—By Joseph P. Mullin, M. E. New York: D. Van Nostrand. Price $2.50.

This book is prepared for workmen by an artisan, and, like works of its class, is very concise. The above title is made to comprehend a large variety of acquirements.

The author begins with an essay on the duties of draftsmen, and explains wherein their labors are intimately related to those of the molders.

This is followed by descriptions of the machinery of the pattern shop. Then come Molding Spur Gears, Worm Gears, Sheave Wheels, Fly Wheels and Band Wheels.

Considerable space is devoted to cylinder work of different kinds.

Lathes, Mining Machinery, and Propeller Screws are the subjects of three separate chapters.

The work is illustrated with 165 cuts interspersed throughout the text.

Notes on the Chemistry of Iron. By Magnus Troilus, E. M. New York: John Wiley & Sons. Price $2.00.

This is a guide for the student who wishes to analyze the ores, products and slags of iron manufacture.

The work is in four chapters, the first of which deals with the elements to be sought for in the finished products. The second chapter gives specific directions for the quantitative determination of all foreign substances found combined in commercial irons and steels.

The third chapter is devoted to the methods to be followed in analyzing ores and slag.

Chapter four is a shorter one, and deals with the analysis of the gaseous products of iron manufacture.

The typography of the book is exceedingly good.

Principles of Economy in the Design of Metallic Bridges. By Charles B. Bender. New York: John Wiley & Sons. Price $2.50.

The method of the author for the comparison of bridge systems is to multiply the strain of each member of a fully-loaded bridge of a fixed number of panels by the length of the member, and add the products. Results thus obtained afford means of determining the relative economy of different designs.

In the present treatise this plan is applied to the framed arch, the cantilever and the stiffened suspension bridges.

As a thoroughly scientific essay upon an important subject, the work will doubtless be welcomed by the profession. It is well to remember that Mr. Bender, although he began his professional career in Europe, is one of the strongest advocates of the American system of iron trusses.

MISCELLANEOUS.

Boiler Steel.—The committee appointed by the American Master Mechanics' Association on boilers reported as follows, on the subject of steel for boilers: "The steel we now easily procure, and should use for both sheet and furnace, has but a small percentage of carbon, say from 15 to 20 per cent., and therefore, if fairly free from silicon, phosphorus, &c., will not harden, is mild and ductile, working under the hammer, even when cold, most freely, has a breaking resistance of from 62,000 to 65,000 lbs. per square inch of section, with an extension of 24 to 25 per cent. in length of 10 inches. A good shop test—in addition to bending over at a point in the test piece where it has been punched, either cold or after it has been heated or dipped in water—is to take a narrow strip through which a hole one-fifth its width is punched, and then enlarge the hole with a drift and flagging hammer to fully three times its original punched diameter without splitting the strip. It may be said that there is now no practical limit to the size that steel sheets of desirable quality and thickness can be procured. The Canadian Pacific Railway are making the straight portion of wagon-top boilers in one plate, and receive $\frac{7}{16}$-inch sheets 14 feet 8 inches by 8 feet, and last year, at Erie Pennsylvania, $\frac{3}{8}$-inch plates 16 feet long were bent cold in rolls to a curve of 30-inch radius, so that two sheets made one boiler 16 feet long and 60 inches diameter, with but two longitudinal joints in barrel; and Mr. Webb has rolled from ingot and used $\frac{7}{16}$-inch steel plate 11 feet 3 inches wide by 12 feet 9 inches long in one barrel ring. This practice is in line with the strong recommendation of the committee in 1871; but, as they point out, it requires special rolls to be built for bending the plate. Mr. G. S. Strong says he is now constructing a 56-inch straight boiler for 175 lbs. pressure, with all longitudinal seams welded. Welded seams, when iron plate and coke fire were used, give, at least, but an ultimate tension of 14 tons and $6\frac{1}{2}$ per cent. extension for 22-ton iron; therefore, it is not a matter of surprise that such a form of joint was seldom used, but now Mr. S. Fox, using 22-ton mild steel and common gas flame for heating the scarf, secures a weld having an ultimate tension of 21 tons and an elongation of $8\frac{1}{2}$ per cent. in a 10-inch length—that is, an increase of 50 per cent. in strength and 30 per cent. in ductility; although it should be noted that this excellent result is but one-third the ductility of the unwelded steel plate. Some careful experiments show that very mild steel, after annealing, loses somewhat in resistance to ultimate tension and in ultimate percentage of extension, or, in other words, annealing does not necessarily improve the natural plate. Nevertheless, if holes are punched in plate, or plate has been flanged or set when warmed, mild steel must be annealed, the annealing furnace temperature not being carried too high—a blood or cherry red is quite sufficient—the time in furnace not prolonged beyond the point that will secure thorough and equal temperature, and the cooling not too much hurried by contact with damp earth or a current of wind at low temperature. Neither should there be any attempt to soften off by cooling in a bed of sawdust or ashes. If the flanging, setting, or any other distortion, such as bending in the rolls can be done cold, there is no necessity for annealing either plates or bars, but all such setting is preferably done under the steady pressure of hydraulic tools." In the discussion on the report there were some objections, but the general opinion held was that steel was the proper material for the interior of fire-boxes.

The following, from the report of the *Astronomer Royal*, are the principal results for magnetic elements for 1884:—Approximate mean westerly declination, 18 deg. 8 min.; mean horizontal force, 3.931 in English units, 1.812 in metric units; mean dip, 67 deg. 29 min. 8 sec. by 9-in. needles, 67 deg. 29 min. 32 sec. by 6-in. needles, and 67 deg. 30 min. 9 sec. by 3-in. needles. In the year 1884 there were only five days of great magnetic disturbance, but there were also about twenty days of lesser disturbance.

The first annual dinner of the University College Engineering Society (London) was held last Tuesday evening at the Holborn Restaurant. The attendance was large. Among others present were Mr. Bryan Donkin, Mr. Rich, Mr. C. E. Stromeyer, and others. Professor Alex. B. W. Kennedy, president of the society, occupied the chair.

The United Asbestos Company is introducing a new lubricant, called the Salamander lubricant, which is offered as applicable for all purposes, and especially so where asbestos packing is used. The following is from the analysis of the lubricant, by Mr. R. H. Harland, F. I. C.:—Mineral matter (ash), .04 per cent.; vegetable and animal oils, none; free acid, none; melting point, 125 deg. Fahr.; solidifying point, 120 deg. Fahr.; flashing point, 384 deg. Fahr. The analysis of this grease proves it to be a pure hydro-carbon, perfectly solid at all ordinary temperatures, and having a melting point of 125 deg. Fahr. It is quite free from the slightest trace of acidity. It is a neutral grease; its consistency, combined with the fact that it will not corrode metallic surfaces, shows that it is a satisfactory material for lubricating. It would answer well with asbestos packing.

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MECHANICAL INTEGRATORS.

By Professor Henry Selby Hele Shaw, Assoc. M. Inst. C. E.

Proceedings of the Institution of Civil Engineers.

II.

Planimeters in which only Pure Rolling Motion is Assumed to take Place.

There have been many efforts to design instruments in which no slipping shall take place. These efforts have resulted in the production of various instruments which, though they differ in external form and mechanical action, yet rely upon the same mathematical principle of action as the planimeters already dealt with, the particular form of disk and roller, or sphere and roller, being taken. Thus, in every case there is a measuring roller, or its equivalent, the rate of motion of which has to be varied by some means or other. It is in the method by which this is done that this class of planimeters differs from the other. Instead of obtaining the variation of the measuring roller in bringing it into contact with circles of different linear velocity by sliding it over the surface of the disk or sphere, one or other of the two following principles are employed. A device equivalent either to (1) bringing in succession a series of measuring rollers into contact with the different imaginary circles; or (2) bringing circles of different linear velocity into contact with a single fixed measuring roller.

The disk-globe and cylinder-integrator of Professor James Thomson belongs to the former class. In this a sphere G (Fig. 30) rolls over the surface of the disk M, but is also in contact with a cylinder mm'. The motion of G in direction OY is that in which the roller would slip in the ordinary disk and roller, and does not affect the motion of rotation of mm'. On the other hand, the motion in direction OX, which is due to the turning of the disk, is entirely imparted to mm'. Thus, as G rolls along mm', the same effect is, in theory, produced as if a series of rollers m, m_1, m_2, etc., upon the same axis as the cylinder, were successively applied to the surface of the disk, and all slipping, at any rate from this cause, is avoided. The actual mechanism which has not been employed for a planimeter takes a slightly different external form in the harmonic analyzer of Sir W. Thomson's tide-calculating machine.

The devices which have now to be considered as solutions of the problem under consideration by the first method, are used in connection with the geometrical property of the sphere already discussed, and upon one similar to it.

Let M (Fig. 31) be the plan of a sphere rolling along the line OX, carrying with it, by a frame not shown, a cylinder (m)

which can roll about it so as to come into contact at any point q upon its horizontal great circle. Then the rotation of the cylinder may be employed exactly in the same way as the rotation of the roller on the integrator described on page 416, and shown by Fig. 28; but in the present case, instead of causing the roller to slip over the surface, the rolling of the cylinder is practically equivalent to bringing in succession a series of rollers m, m_1, m_2, etc., upon one axis in contact with it.

This principle has been employed both by Professor Mitchelson of Cleveland, U. S., and Professor Amsler. The mechanism of Professor Mitchelson's instrument is shown in Fig. 32. In this a flexible steel band or chain F, passing round a semi-circular arc D, forces the cylinder C to roll on the sphere G. The cylinder is carried by a frame E, which slides along the bar A, by which it is supported. The mode in which it is proposed to apply it to the ordinary Amsler planimeter is shown on a smaller scale, Fig. 32A, where b is the pole-arm, a the radius bar, and t the center of rotation of the latter.

Fig. 30

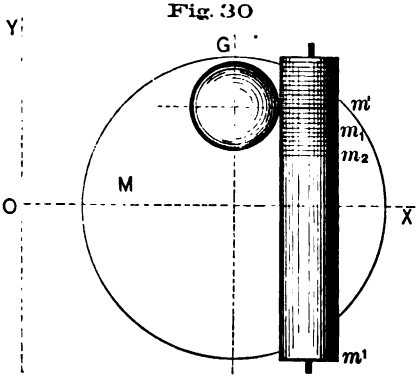
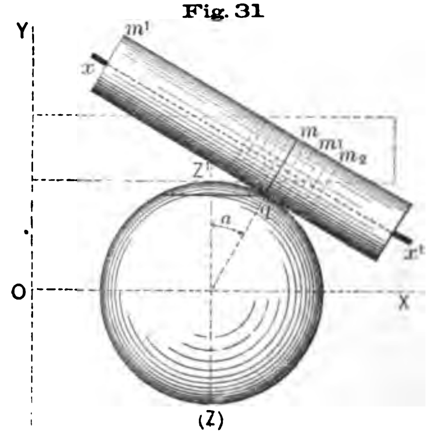


Fig. 31



Professor Amsler's planimeter on this principle is similar to the foregoing, except that instead of being carried by two guides as sleeves by a bar, the cylinder frame is supported on rollers from a frame above, the rolling friction on the latter being less than that of the cylinder on the sphere. Thus, the cylinder always moves to its required position. The motion of the spherical surface is obtained from a bevel-wheel upon its axis, which gears with a larger one formed upon the edge of a circular stand or support of the instrument.

A similar principle of the geometry of the sphere has also been employed in an instrument suggested in a paper in 1855 by the late Professor Clerk Maxwell, when an undergraduate at Cambridge. Instead of the cylinder in Fig. 31, let a sphere m' roll around on the sphere (M), as shown in Fig. 33. Then, from the property of the sphere, which is proved at length in the above paper, the turning of the sphere m' about its axis of rotation xx, relatively to the turning of M along OX, is proportional to the tangent of the angle a in the figure. In the other case it will be remembered that the turning of the cylinder or disk was proportional to the sine of the same angle. By suitable means the principle can be employed in the construction of a planimeter. Two forms of such planimeter are shown in the paper, and though they are both in the form of the linear planimeter, and are scarcely suitable for practical application, yet the matter is dealt with in a way worthy of the inventor. It is evident that this is another case of bringing the equivalent of a series of rollers m, m_1, m_2, into contact with the sphere, though these are no longer of one size, but vary from a diameter zero to a diameter of the size of that of the sphere m.

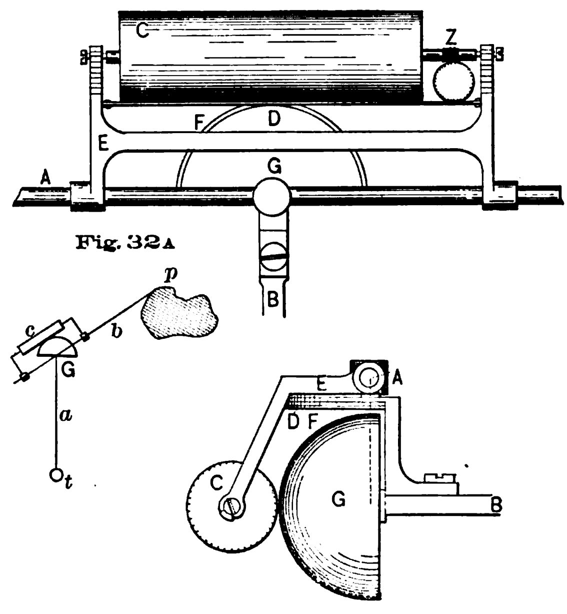
Coming now to the instruments in which the alternative device adopted for the avoidance of slipping is by bringing

into contact with one roller different circles of the disk M, or of its equivalent.

This may be done in the following way: Instead of allowing the cylinder (m) to roll on the sphere M (Fig. 31), and so to change the radius of the imaginary rolling circle (whose diameter is qq') on which it rolls, suppose that the cylinder is kept in contact as shown by the dotted lines, and the axis of rotation zz' of the sphere is turned, as, for instance, would happen if a sphere in combination with rollers were used as suggested for an anemometer by Mr. Ventosa, through an equivalent amount, *i. e.*, through the angle a. This will give the same result as far as the rotation of the cylinder is concerned, but with an important difference. The cylinder (in Fig. 31) or sphere (in Fig. 33) is no longer needed, and may be replaced by the original measuring roller, whose axis has a fixed position parallel to OX. It will be seen that this device practically amounts to bringing different circles on the sphere M into contact with the measuring roller (m), with the great advantage that exactly the same circle on the sphere M is scarcely likely to again roll in contact with the roller (m), though of course the radius may be the same. This method has been recently proposed by the author, and the mode of carrying it out without involving slipping, by what is called the "sphere and roller mechanism," which mechanism has been explained and developed at length in a paper before the Royal Society. It need here be only remarked that the planimeter there described, and afterwards exhibited to the British Association at Montreal, was of the linear form, and of little practical use; but the author has since completed a polar planimeter and exhibited it before the Royal Society.

Fig. 32

Fig. 32A



One one more area planimeter remains to be mentioned, and this is the one invented and brought before the Physical Society by Mr. C. V. Boys. The principle of action is briefly this: A wheel or roller, which is not supposed to slip sideways on the diagram, has its plane of rotation kept always at an angle a to the axis OX of the figure to be integrated, such that

$$y = \text{ordinate of the curve}$$
$$= \tan a \times K$$

where K is a constant, and y is the ordi-

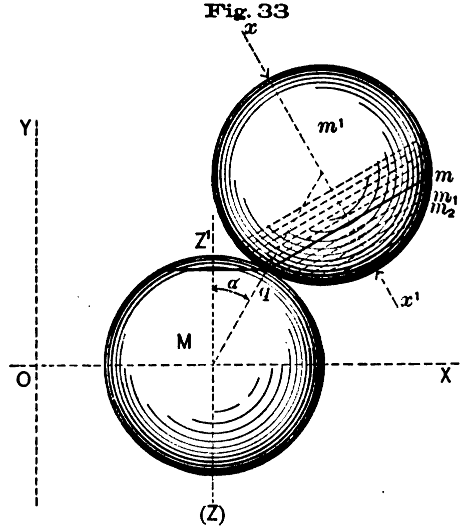
nate with respect to OX of that point on the curve which the pointer of the instrument is at the same instant tracing. If the component of a small motion of the wheel parallel to OX is Δx, and the component of the same movement parallel to OY is Δt.

$$\text{Then} \quad \frac{\Delta t}{\Delta x} = \tan \alpha = \frac{y}{K}$$

$$\Delta t = y \Delta x \times \frac{1}{K},$$

or the distance moved by the wheel parallel to the axis OY becomes the measure of an element of area. It is easy to see that the height moved by the wheel becomes a direct measure of the area of the figure. Various examples of the action of this planimeter, called by the inventor the tangent integrator, are given by Mr. Boys; but the action is obviously limited, and an investigation of the theory reveals the fact that it is only a special case of the general problem, not only of the method of applying circles of varying diameter to one roller, but of the sphere and roller mechanism itself. This will be rendered clearer by stating that, in order to employ the component parallel to OY, the roller was made to work against a cylinder, which, by its turning, acted as the measuring roller. Evidently the length of the cylinder limited the travel in that direction. The cylinder was carried bodily along in the direction of its axis (corresponding to OX), and made to effect its own turning, the amount of turning varying with the tangent of inclination of the wheel, and this was sufficient in the application to the steam-engine integrator to be hereafter described, where the longitudinal motion of the cylinder could be made proportional to the stroke. Mr. Boys endeavored, by various means, to obtain continuous motion in both directions, one being equivalent to bending the ends of the cylinder round, and so attempting to solve the difficulty by what he has termed a "mechanical smoke ring." The author, however, by approaching the matter from a different point of view, designed the sphere and roller integrator, which is nothing more or less than the inversion of the mechanism of Mr. Boys. In this the roller of Mr. Boys is replaced by the sphere, and instead of the two motions, one of the cylinder about its axis, and one of the cylinder longitudinally, the two rollers are used. It may be easily shown that the turning of the plane of rotation of the roller of the tangent integrator is equivalent to changing the axis of the sphere in the sphere and roller integrator.

Fig. 33



Moment Planimeters.

The moment of an area, and its moment of inertia about a given line, may be obtained mechanically upon similar

principles to those by which a simple area was obtained. If ABCDE, Fig. 1, be the figure whose moment of area and moment of inertia are required about any line OX; then, taking any element of area AB, if y=height of upper portion SB, then the moment of area of the element SB about OX

is $$m = \text{area of SB} \times \frac{y_1}{2}$$

$$= \tfrac{1}{2} y_1^2 \triangle x.$$

Similarly, the moment of inertia of the element is

$$i = \tfrac{1}{3} y_1^3 \triangle x.$$

The sum of an infinite number of such expressions as these, when $\triangle x$ become infinitely small, gives respectively the moment of area and the moment of inertia of the whole figure according to the expressions.

$$\text{Moment of area} = M = \tfrac{1}{2}\int y^2 dx,$$
$$\text{Moment of inertia} = I = \tfrac{1}{3}\int y^3 dx.$$

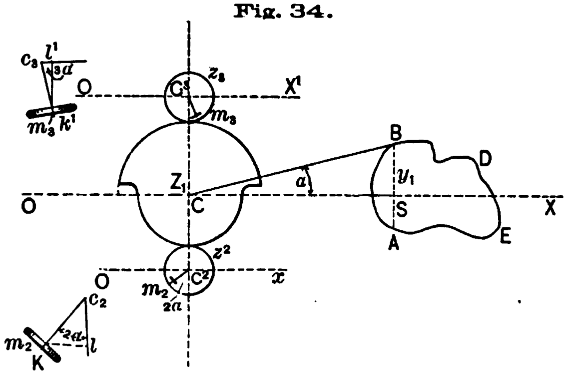
Now, there are two possible ways of obtaining these results mechanically. One of these ways is by applying for the purpose the suggestion made by Sir William Thomson in connection with the disk globe and cylinder integrator of Professor James Thomson, of using a train of such mechanisms to obtain the integration of a simple linear differential equation. By certain simple arrangements,

The first mechanism would give $\int y dx$,
" second " " " $\int y^2 dx$.
" third " " " $\int y^3 dx$.

This method need not be further considered here, since, so far as the author is aware, it has never been carried into actual practice. It may be, however, said that the mechanical difficulties in the way of causing the measuring wheel or roller of the first mechanism to actuate the second, and the roller of the second to actuate the third, without introducing serious error, are not easy to overcome, and require a very easily working piece of apparatus. The author has discussed the applications of the sphere and integrator for the purpose, in a paper to the Royal Society.

The other principle is to cause the measuring roller to be directly turned at a rate which is made to vary, not as in the simple planimeter with the value of the ordinate (y), but with its second or third power. Though no method of directly doing this has apparently yet been suggested, yet the same result is practically effected by the beautiful application of a mathematical principle in the "moment integrator" of Professor Amsler.

Fig. 34.



Let the pole-arm CB (Fig. 34) be attached to a toothed segment (z_1), one portion of which gears with a toothed wheel z_2, the radius being as 2 to 1. Let the center C of z be carried along OX, while the center of C_2 of z_2 is carried along a line ox parallel to OX. Let m_2 be a roller acting in every way as the measuring roller of the Amsler planimeter, whose axis is carried in the plane of the wheel z_2, its direction passing through the center C_2. When the pole-arm coincides with OX, let the plane of rotation of the roller m_2 be parallel to OX, and

its axis parallel to OY. When the pole-arm is turned through an angle $SCB=\alpha$, the angular motion of the wheel z_2 is twice that of the arm; thus the roller m_2 takes the position shown in the figure.

This is so because $\dfrac{\angle^r \text{ motion of } z_2}{\angle^r \text{ motion of } x_1}=$

$$\frac{\text{radius } z_1}{\text{radius } z_2}=\frac{2}{1},$$

$$\therefore \angle Kc_2l=2\alpha.$$

Suppose the pointer p to move through the width of the element SB at a height $=y$, and with it z_1 and z_2, the roller m_2 being in contact with the diagram surface. Then, by what was proved in the case of the Amsler planimeter, and adopting the same notation.

$$\frac{\text{Turning of } m_2}{\text{Motion of translation of } m_2}=\frac{2\pi rn_2}{\Delta x}=\frac{lc_2}{c_2K}$$
$$=\cos 2\alpha$$
$$=1-2\sin^2\alpha,$$

but $\dfrac{SB}{CB}=\dfrac{y_1}{R_1}=\sin\alpha$ (where $CB=R_1$),

$$\therefore \frac{2\pi rn_2}{\Delta x}=1-2\sin^2\alpha=1-\frac{2}{R_1^2}y_1^2,$$

or $\quad n_2=\left(\dfrac{1}{2\pi r}\right)\Delta x-\left(\dfrac{2}{2\pi rR_1^2}\right)y^2\Delta x.$

When the complete travel of the curve has been made, the sum of a series of quantities similar to the first becomes zero; so that, by making the constant $\left(\dfrac{1}{\pi rR_1^2}\right)$ equal $\frac{1}{2}$, the reading of the roller gives the value—

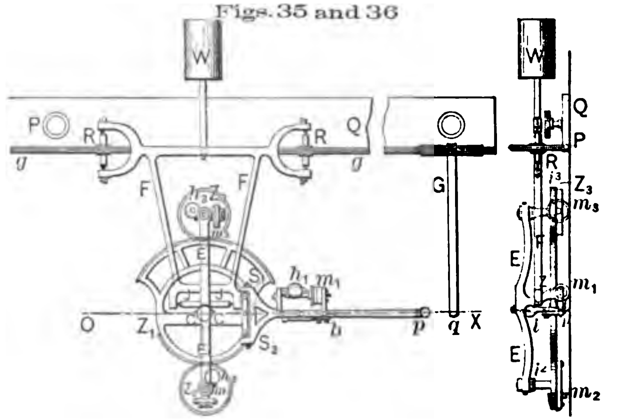
$$M=\tfrac{1}{2}\int y\,dx,$$

or the moment of area of the figure BDEA.

For the moment of inertia the segment of z_1 is used, the radius of which is three times that of another wheel z_3, with which it gears. The action of a roller m_3, carried by the wheel z_3, is exactly the same as that of m_2, except that its angular motion is three times as great as the pole-arm CB, instead of twice as great, as in the case of the other roller.

By reasoning similar to that already adopted, and taking the plane of rotation of m_3 perpendicular to OX in its initial position, instead of, as in the former case, parallel to it—

Figs. 35 and 36



PLAN SIDE ELEVATION

$$\frac{\text{travel of } m_3}{\text{motion of translation of } m_3}=\frac{2\pi rn_3}{\Delta x}=\frac{l'c_3}{c_3k'}$$
$$=\sin 3\alpha$$
$$=3\sin\alpha-4\sin^3\alpha.$$
$$\frac{SB}{CB}=\frac{y_1}{R}=\sin\alpha,$$

Therefore $\dfrac{2\pi rn_3}{\Delta x}=3\sin\alpha-4\sin^3\alpha=$

$$=3\frac{y_1}{R_1}-4\frac{y_1^3}{R_1^3},$$

Or $n_3=\left(\dfrac{3}{2\pi rR_1}\right)y_1\Delta x-\left(\dfrac{4}{2\pi rR_1^3}\right)y_1^3\Delta x,$

which, when the pointer is taken around the curve, gives, with suitable values of the constants,

$$n_3=\int y\,dx-\tfrac{1}{3}\int y^3dx$$
$$=\text{area of BDEA}-\text{moment of inertia of BDEA}$$
$$=\quad A\quad -I.$$

or $\quad I=\quad A\quad -n_3.$

The instrument, Figs. 35, 36, has an area planimeter attached to it, so that, by reading the rollers m_1 and m_2, and subtracting the results, the moment of inertia is obtained.

The details of the moment planimeter shown (Figs. 35 and 36) are easily explained. A guide PQ of steel has a groove *gg*, which is placed parallel to the axis OX by means of the gauges G, one, as shown, being at each end, which are adjusted with their points *q* upon the line OX. The rollers RR run in the grooves *gg*, and support a frame FF, which carries, by means of an axle JJ, the frame EE. This frame supports the toothed segment z_1, and the two toothed wheels z_2, z_3, upon vertical axis. The former between centers, one of which is shown, Fig. 36, *i*, the latter by steel axles within the column $i_2 i_3$. The pole-arm carries, in addition to the pointer *p* at the end, the roller m_1 with its dial h_1, forming an ordinary planimeter, and is itself carried on the centers $s_1 s_2$. The two other rollers and dials are shown as $m_2 m_3$ and $h_2 h_3$ respectively. The weight W serves to balance the instrument, so as to avoid undue pressure on the paper, and the motion is so smooth as to enable a curve to be traced with the greatest ease and accuracy.

Attention has been called by the late Dr. Merrifield and others to the valuable applications of this instrument for purposes of naval architecture, but so far as the author is aware, no account has been given in this country of its applications in civil engineering, as proposed by Professor Amsler. The following brief account of the methods in the case of calculating the contents of embankments, cuttings, etc., is therefore given from an abstract for which the author is indebted to the kindness of Dr. A. Amsler:

Let Fig. 37 be the plan of a portion of an embankment or cutting, the character of which is supposed to be the same throughout, viz., of uniform width of roadway, and uniform side-slopes, the surface of the ground, the gradient, and the horizontal curvature of the roadway, being restricted in no way. AA′ represents the center line of the railway; B_1B_1' and B_2B_2' its two borders; C_1C_1' and C_2C_2' the intersections of the side-slopes with the surface of the ground. Suppose now the embankment or cutting to be divided into thin layers by vertical planes, perpendicular to the center line AA′ of the roadway; PQ and P_1Q_1 may be the intersections of two adjacent planes with the plane of the diagram.

Fig. 37

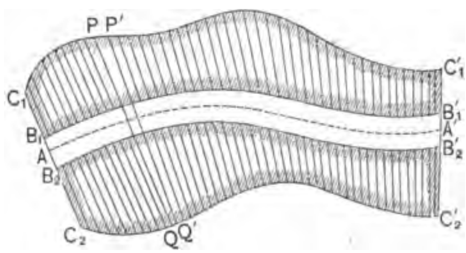
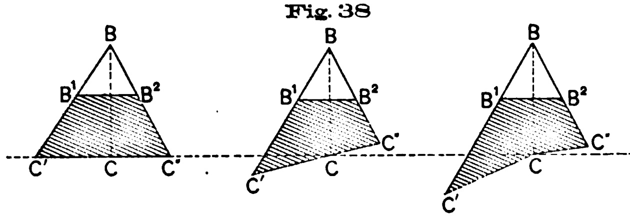


Fig. 38



Then if p = area of section PQ,

$\triangle s$ = interval between PQ and and P_1Q_1 measured upon the center of gravity of the section.

Total volume of embankment is (from one of the properties of Guldinus)—

$$V=\int p ds,$$

the integral extending over the whole length of the embankment under consideration.

There are three cases dealt with in the Paper of Professor Amsler, corresponding to the three forms of sections, I, II, or III, Fig. 38.

The first of these, I, is simple enough, since the center of gravity of the section always coincides in plan with the center line of the roadway, and the plan of operation is as follows:

Let Fig. 39 represent a longitudinal section of a portion of the embankment of uniform gradient, developed into a plane; the straight line E′EE″ represents the top of the embankment; G′GG″ the profile of the ground; the straight line A′AA″, which is parallel to E′EE″, is the locus of the imaginary vertex of the trapezoidal cross-sections. The level line MN is the line to which the offsets of the profile of the surface of the ground refer. BG shows the intersection of a vertical cross-section with the figure, and AG the intersection of a plane perpendicular to the top of the embankment (and also to the line A′AA″) with the figure.

Let $i=\angle AGB=\angle MNA=$gradient;

$y=AG$ =distance of vertex to bottom of embankment;

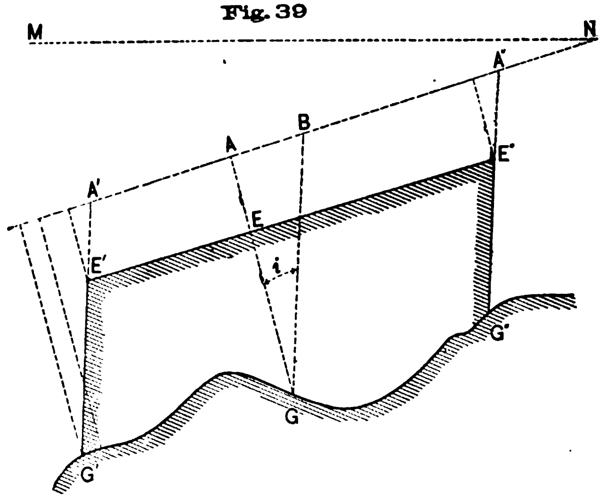
$y_0=AE$ =distance of vertex to top of embankment;

$2\beta=$angle at vertex at A.

It may be easily proved the area of the section made by the plane AEG is—

$$p=(AG^2-AE^2)\tan\beta=(y^2-y_0^2)\tan\beta$$

Fig. 39



but since $V=\int p dx$

Therefore volume $=\tan\beta\int(y^2-y_0^2)\,dx.$

And thus, if β is known, the volume of the portion E′E″ G″G′ (Fig. 39) is easily found with the mechanical integrator, thus:

Take A′AA″ as the axis of moments, and adjust the rail of the instrument so as to be parallel to it. Start the pointer anywhere on the shaded figure, and trace round it; the travel of the roller m_2 being denoted by M, the scale of the drawing longitudinally being:

$1''=m$ feet,

and vertically $1''=n$ feet;

then volume $=V=20\ mn^2\tan\beta\times M$ cubic feet.

It only remains to insert a known

value for $\tan \beta$, which is easily done, thus:

Let Fig. 40 be a perspective view of the sections AEG and BG (Fig. 39), where:

$$\angle C_1BC_2 = 2\,\alpha.$$

Then from the diagram $\frac{C_1G}{AG} = \tan \beta$;

also $\frac{C_1G}{BG} = \tan \alpha$ and $\frac{AG}{BG} = \cos i$.

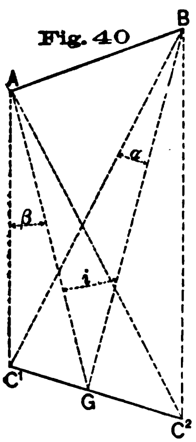
Therefore $\tan \beta = \frac{\tan \alpha}{\cos i}$,

or $$V = \left(20\, mn^2 \frac{\tan \alpha}{\cos i}\right) M,$$

where $\angle \alpha$ and $\angle i$ are known constants.

To complete the calculations for the whole route separate portions are taken, with the various proposed gradients.

Fig. 40



The above formula is exact for the integrator shown in Fig. 36, as arranged for English measures, a complete revolution of the measuring roller being taken as a unit of reading.

It is to be noted that nothing is supposed as to the curvature of the center line of the roadway horizontally, as it is supposed to be developed in the figure. Also, that the aggregate error arising from the assumptions that the cross-sections are exact trapezoids will in most cases be very slight, on account of the errors in cuttings and those in embankments partly compensating for each other, in addition to the cutting and filling in each section, as shown in Fig. 41, where the small triangular portion in dotted lines C′DH represents the amount taken off the former, and added to the latter.

Alterations of the proposed roadway, otherwise involving tedious calculations, simply necessitate an alteration in the line A′AA″, and a repetition of the mechanical work of the integrator, but need no fresh diagram. In preparing the drawing, allowance should be made for ditches along the roadway in cuttings, which is easily done, as shown in Fig. 42, where B_1B_2, which equalizes the amounts taken and left, must be considered as the roadway line. In the case shown in Fig. 43, the excess of the embanking over the cutting is approximately equal to the layer above the dotted line C_1C_2. The contents of this layer could be measured either by considering it as an embankment, and treating it as such, or by the simpler—and for a first estimate sufficiently accurate method—of assuming its section to be a parallelogram. The area of the shaded portion (Fig. 43) is then simply to be measured, and the result, multiplied by the length of the road, gives the required contents. The supposition that the slopes CD and C′D′ are the same is also sufficiently accurate.

The foregoing is the first method described by Professor Amsler, and is extremely simple, but obviously only approximately accurate. The two other methods are capable of giving very accurate results, and are dealt with by him at considerable length. Only a short account of them will be given here.

The first thing to be noted is that, as a rule, the center of gravity of the section will not really coincide in plan with the center line of the roadway, but will curve at the line SPP′S′, Fig. 44, AA′ being the true center line. Thus, in the expression $\int p ds$, the value of ds does not coincide with dx, as hitherto assumed. From the figure it is seen that:

$$\frac{\Delta s}{\Delta x} = \frac{R+e}{R},$$

where R is the radius of curvature of the center line.

Therefore $$\Delta s = \Delta x\left(1 + \frac{e}{R}\right),$$

or $$V = \int p ds = \int p dx + \int \frac{pe dx}{R},$$

and this expression must be used.

The first of the two methods assumes the base of section to be inclined, but not broken (Fig. 38, II), and the side-slopes, gradient, and radius of a given portion to be constant. A diagram is prepared, as shown in Fig. 45, in which the dotted lines now represent intersection of the sides of embankment with the surface of the ground, which do not, as before, coincide with the contour of the center line.

From this figure $y_0 = \mathrm{AE}$
$y_1 = \mathrm{AC}_1$
$y_2 = \mathrm{AC}_2$
$2\beta = \angle$ at vertex A.

Then, by similar reasoning to that previously employed, it may be proved that:

$$\mathrm{V} = \frac{\tan\beta}{2}\mathrm{U} + \frac{\tan^2\beta}{6\,r}\mathrm{W},$$

where

$$\mathrm{U} = \tfrac{1}{2}\int (y_1^2 - y_0^2)\,dx + \tfrac{1}{2}\int (y^2_2 - y_0^2)\,dx - \tfrac{1}{2}\int (y_1 - y_2)^2\,dx$$

$$\mathrm{V} = \tfrac{1}{2}\int (y^3 - y_0^3)\,dx - \tfrac{1}{3}\int (y_2^3 - y_0^3)\,dx - \tfrac{1}{3}\int (y_1 - y_2)^3\,dx.$$

Fig. 41

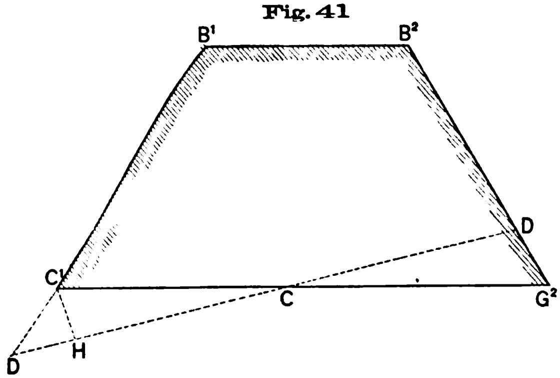
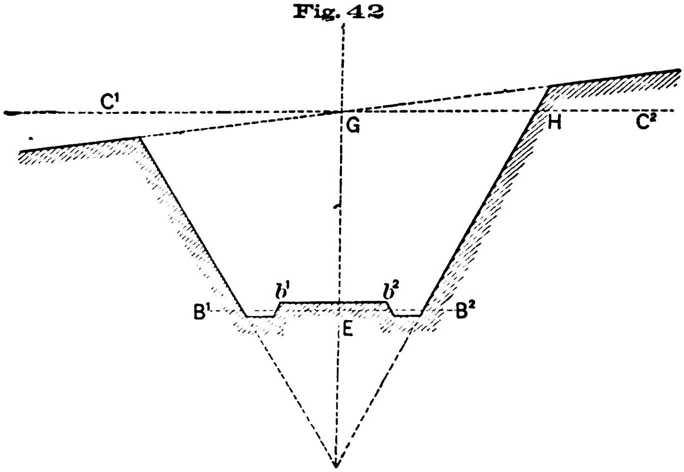


Fig. 42



Area of element section

$$= p = (y_1 y_2 - y_0^2)\tan\beta$$

and $$ep = y_1 y_2 (y_1 - y_2)\frac{\tan^2\beta}{3}$$

$$\therefore \mathrm{V} = \tan\beta \int (y_1 y_2 - y_0^2)\,dx + \frac{\tan^2\beta}{6\,r}\int y_1 y_2 (y_1 - y_2)\,dx.$$

By a simple transformation this expression is brought into such a form as to allow of mechanical integration. The final formula being:

Another simple diagram has to be prepared, and by means of three operations of the integrator, the values of U and V are given thus:

$$\mathrm{U} = 20 \times mn^2\,(v_1 + v_2 - v_3)$$

$$\mathrm{W} = mn^3\,[320\,(u_1 - u_2 - u_3) - 100\,(w_1 - w_2 - w_3)]$$

where m and n have the significations formerly explained, and u, v, and w are the respective readings of the area, moment of area, and moment of inertia rollers in each of the three operations.

Considering the great amount of calculation thus saved, and the accurate nature of the results, this second method, although involving rather more labor than the first, is a very important one.

The third method, which deals with the broken base, is much the same in principle as the second, but the expressions become more complicated, whilst six

Fig. 43

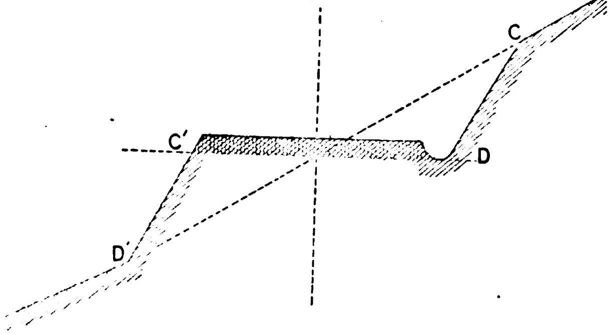
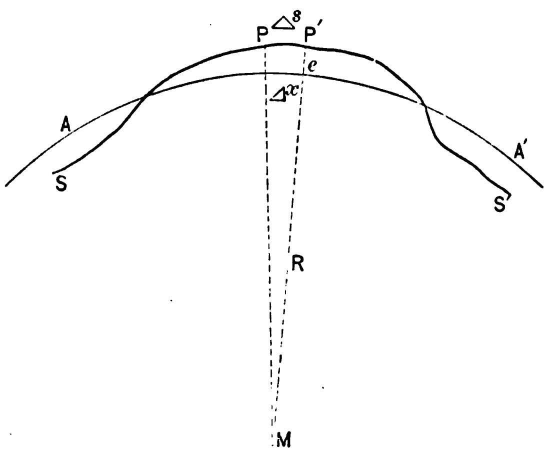


Fig. 44



readings of the measuring rollers are involved. The case of an embankment, consisting partly of a cutting, is completely and accurately worked out by this method.

The Paper of Professor Amsler concludes with an example of the application of his integrator to the problem of the strength of a girder.

Continuous Integrators.

Any piece of mechanism which continuously adds up results may be regarded as a continuous integrator. Of such instruments, revolution counters as employed in meters of various kinds, form the simplest example, and correspond in action to the devices already described, by which the linear measurement of a boundary is performed. These will not be further referred to, and it is only necessary to consider those computing mechanisms which, dealing with the result of two simple unit measurements, correspond in principle to area planimeters.

It appears that Poncelet, before the year 1838, suggested the employment of a continuous integrator for computing the two factors in dynamometrical measurements. This was described by Morin in 1838, as applied in his "compteur" for registering the work done by a team of horses, dragging a loaded carriage at any given velocity over any length of road. The principle employed was that of the disk and roller, the use of which

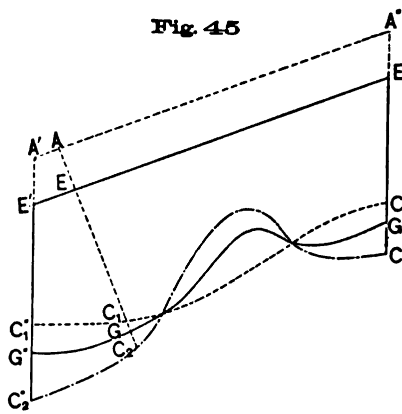
as already shown, had been suggested for application in a planimeter more than twenty years before. In the case in question, the disk was turned by an endless cord or band from one of the wheels of the carriage, while the position of the roller on the disk was caused to vary with the tractive force, and its reading thus gave the product of force and space, or the actual work done.

In 1840, a Committee of the British Association, consisting of Professor Mosely, Mr. Enys, and Mr. Hodgkinson, was appointed to procure the dynamometrical apparatus of Mr. Poncelet, and to obtain a series of experiments on the duty of steam-engines by means of that apparatus, the sum of £100 being placed at their disposal for the purpose. The report of this committee, in 1841, describes at length the "constant indicator" of Professor Mosely, which was in reality a continuous steam-engine integrator. It was entirely a new instrument, except that the principle of the disk and roller was employed, and also the traction springs of General Morin. The pressure of steam was allowed to act upon a piston so as to vary the position of the measuring roller, while motion was given to the surface of revolution by means of the stroke of the engine. The surface of revolution was a cone, which was substituted for a disk, as by this arrangement the rapidity of the changes of velocity due to corresponding changes in the position of the integrating wheel is diminished in the same proportion in which the sine of one-half the angle of the cone is less than unity. The force necessary to drive the integrating wheel is diminished in the same proportion, and therefore the chance of an error arising from the slipping of the edge of the integrating wheel on the surface which gives it motion. The reports of the committee in 1842, 1843, and 1844 (which was joined in the first of these years by Dr. Pole), show that the action of the above instrument was, as far as could be determined in its application to a single-acting Cornish engine, very satisfactory, but apparently no mention has been made of it, or results obtained from it, in any succeeding report.

Various other steam-engine integrators have been brought forward since then, most of them acting upon the same principles; amongst these is the recent power-meter of Messrs Ashton and Story, which is described at considerable length in the American edition of Weisbach's Mechanics as apparently something new. It is, however, the same instrument as Moseley's integrator, except that it employs a spiral instead of a straight spring, and returns to the use of the flat disk.

The disk and roller has also been since the time of Morin applied in many dynamometers, which thus become really "ergometers" or "work" measurers. In a series of articles which recently appeared in *La Lumière Electrique* those of Hirn, Megy, Bourry, and Darwin, are described as having a "totalizer" or integrator of this kind attached to them.

Fig. 45

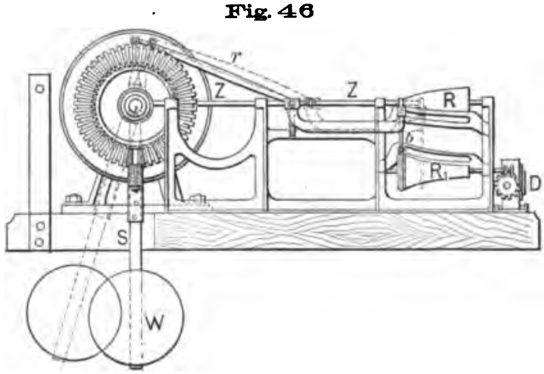


The position of the measuring roller varies with the force exerted or transmitted, and the motion of the disk with the revolutions of the motor or machine. Thus by suitable counting apparatus the continuous product is given of force and space, or work done.

A cone has sometimes been used instead of the disk with dynamometers, as in that of Baldwin and Eickemeyer, used at the Centennial Exhibition in 1876 for testing mowing machines, in which, instead of a measuring roller, a cylinder is placed with its axis parallel to one side of the cone, while an endless band of round cord is rolled along between the two surfaces, so as to transmit the varying motion of circles of different radius on the cone. Fig. 46 shows the apparatus of Mr. Roury applied to a transmission dynamometer. In this the force is transmitted through a differential train of three bevel wheels, the middle one of

which is attached to the spindle (S), which supports the weight (W), and is suspended from the main shaft by the joint at (*k*), about which the whole can turn. Thus, the deviation of the weight from the vertical (as shown by the dotted lines) changes with the force. The change of position of the spindle (S) causes a band (*b*) to move along the surfaces of revolution RR_1, the upper one, R, being turned from the shaft by the spindle (ZZ). It is to be noted that the distance of the band (*b*) from its zero position is not directly proportional to the force represented by the change of position of the weight, and, therefore, the surfaces must be formed with a certain curve, found by construction, in order that the dial and counting apparatus at D may correctly give the product of the two variables, force and space, and so the work transmitted through the dynamometer.

Fig. 46



It cannot be said that continuous integrators of this kind are at present practically employed to any great extent. There are probably two reasons for this. One is the want of durable and reliable instruments. The other, the question as to how much, and to what degree they are really needed.

With regard to the first of these, it is evident that in all the arrangements hitherto considered (with the exception of Baldwin and Eickemeyer device) there is that slipping of surfaces in contact, which, though of little effect as far as wear goes in the limited operations of a planimeter, becomes a very serious consideration when continuous action is required to be maintained. The only integrator of the second, or non-slipping class, which, as far as the author is aware, has yet been practically applied, is the "power-meter" of Mr. Vernon Boys. This instrument is shown in Figs. 47 and 48, and acts upon the same principle as Mr. Boys' integrator. The piston C, subject to the varying pressure in the engine-cylinders, with which the barrel A is connected by the connections at B and B', is moved up and down against or with the tension of the spring D; its rod acting on the arm *g* causes the plane of rotation of the roller G to take positions more or less inclined to the axis of the cylinder H. This cylinder H is moved to and fro with the stroke of the engine by means of the cord L, Fig. 48, and the roller G being in frictional contact with it causes it to turn round to a greater or less extent, according as the plane of G is more or less inclined to the axis of H. The amount of its revolution is registered by the counting apparatus in I (Fig. 48), to which the axis of H is geared, and is thus a measure of the power of the engine, for it gives the product of the tangent of the angle to which G is inclined and the distance moved through by H, that is the product of pressure of steam into the stroke of the engine. The steam being (as originally in Moseley's and also in subsequent integrators) supplied both above and below the small piston, the absolute pressure is given. Thus, in the present case, as the change of pressure on C at the beginning and end of each stroke causes the rod of *g* to be alternately above or below the axis of H, so the motion of the cylinder to and fro will always cause the cylinder H to turn in

one direction, and thus to continuously integrate the work done. This device only enables a reciprocating movement of the cylinder H to be made, and the author has already mentioned the device of the sphere and rollers, which by the inversion of the higher pair of Mr. Boys, enables continuous motion to be obtained, and is suitable for application in dynamometers, electric-motors, and other purposes.

With regard to the want of such instruments, a very strong case was made out by the committee, already mentioned, in their report in 1841, where the application of a continuous integrator to steam-engines was alone discussed. The application has been made to electric-motors, and in trials of motors and machines generally, and there is little doubt if continuous integrators combining the three qualities of durability, accuracy and cheapness could be produced, that in these days of increased regard for measurement of all kinds, there would be a much larger and increasing application of them.

Fig. 47

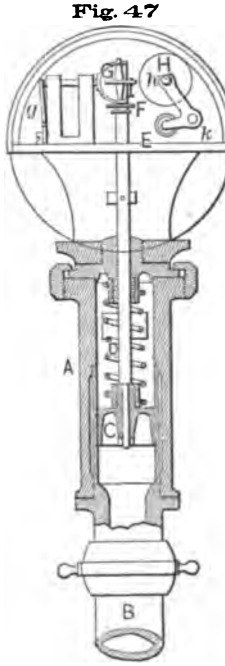
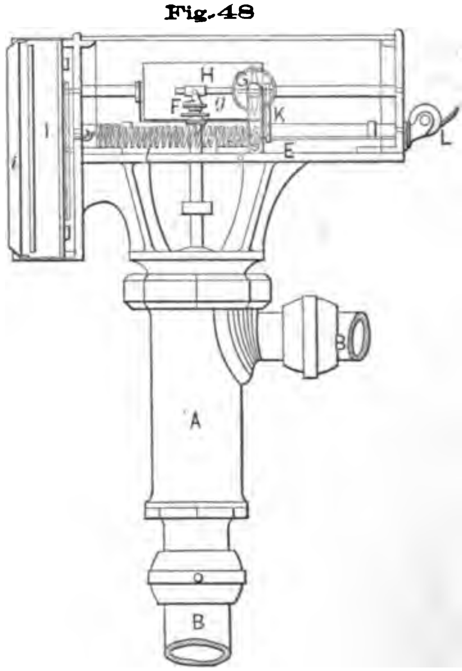


Fig. 48



Limits of Accuracy of Integrators.

In all calculating machines, accuracy of the result must be the question of first importance. Assuming the theory relied on in the various instruments for the mathematical operation to be correct, the accuracy depends primarily upon the mechanical arrangements, though in the case of planimeters it also depends upon the skill and care of the manipulator, and involves the question of a personal error. This latter point need not be considered, partly because this occurs more or less in all results obtained by observers, but also because it is less than might be at first anticipated, from the fact that in tracing the pointer around the curve there is no reason why the error due to moving it on one side should exceed that due to moving it on the other side, that is, why equal errors of opposite effect upon the final reading should not be made.

It has been seen that the action of all integrators, except mere revolution counters, depends upon the motion of the measuring roller, or its equivalent, over surfaces of various forms, therefore the above-mentioned mechanical arrangements resolve themselves into an examination of the nature of the frictional contact of two surfaces. It was for this reason that integrators have been classified according to the nature of this frictional contact, and it now remains to in-

vestigate the nature of this, to show to what the classification leads, to give the direct results of experiments upon the subject, and also the indirect results obtained from the instruments themselves.

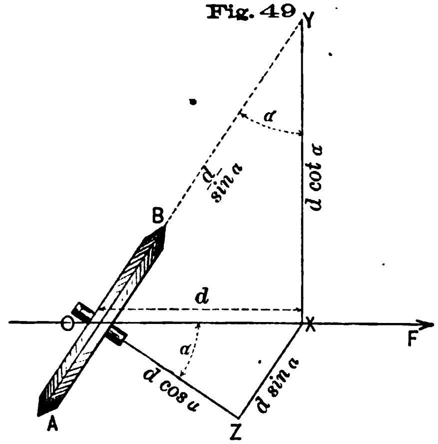
Planimeters and integrators generally have been divided into—

I. Those in which the frictional surfaces slip as well as roll over each other.

II. Those in which slipping of the surfaces is supposed not to take place.

The order of this arrangement was adopted upon historical grounds, and also because the former class is at present by far the most important; but it

Fig. 49



would be more convenient, upon mere grounds of mechanical simplicity, to invert the order.

Let AB (Fig. 49) be the plan of the measuring roller. Suppose a force applied in the direction OX, making an angle (α) with the plan of the axis of AB.

Let d=OX=distance through which the force acts.

1st. Suppose that frame which carries the measuring roller is free to move in any direction horizontally, but maintains the plane of rotation of the roller vertical, then the application of a force along OX, at the center of AB, will cause it to roll along the line coinciding in direction with the plan of the center line AB of the roller, that is, along the line OY. This will always be the case, except when this force is applied in the limiting case in the direction perpendicular to the plane of AB (*i. e.*, when α=O).

Thus, the distance in this case traveled by the center of AB, which is the same as the path rolled by it, is

$$\text{OY}=\frac{d}{\sin\alpha}.$$

and the distance moved through by the center at right angles to OX is

$$\text{XY}=d\cot\alpha.$$

The latter value is the one usually taken or recorded by the instruments at present in use, but depends directly upon the former.

Next, suppose the frame carrying the roller is constrained either by guides, as in the linear planimeter, or by the radius bar of the polar planimeter, or otherwise, to move in the direction of OX, that is, in the direction in which the force acts. When the center of the roller has reached the point X, that is, when the force has been exerted through a distance OX,

Then OZ=distance slipped by AB= $d\cos\alpha$.

XZ=distance slipped by OB= $d\sin\alpha$.

Upon the degree of accuracy with which the above conditions are fulfilled depends the correctness of the working of all integrators; for not only do these two cases entirely cover the action of the two classes of planimeters, and the corresponding continuous integrators, but one of the limiting cases in each, viz., that in which the force acts in the plane of rotation of the wheel (when $\alpha=90°$), represents the conditions under which the wheel of the boundary measurer or opisometer is employed. It may be therefore said that the theory of mechanical action of integrators is based upon one or other of the following assumptions, in which the limiting case (namely, when $\alpha=90$), is included.

Class I.—That the rolling of the planimiter, when slipping is allowed, is

$$N_1=k_1 d\sin\alpha.$$

Class II.—That no slipping takes place, which amounts to the assertion that

$$N_2=k_2\frac{d}{\sin\alpha},$$

N_1 and N_2 being the readings in each case, and k_1 and k_2 suitable constants for the instruments.

It is easy to see that the first of these is really the assumption made for all instruments in Class I.; but in the various instruments in Class II., it is only with the planimeter of Mr. Boys that it becomes directly obvious that the above assumption is made. With the others, though it is less evident, nevertheless, it will be found, on examination, to be equally true that the second supposition is really made, and that upon its truth the correct action of all instruments in the second class depends. The forces acting in each of the two cases must therefore be taken into consideration and the mechanics of the problem examined.

Proceeding in order of simplicity, Class II. will be examined first.

Let AB in both cases (Fig. 50) be the plan of the measuring roller.

Let S = reaction of surface upon which AB rolls, that is, the force with which it is kept in contact with it;

μ = coefficient of friction between roller and surface;

P = OC = reaction of surface, which must be brought into action in a horizontal direction to cause the roller to turn on its axis.

Class II. (*Fig.* 50).—Suppose the frame in which the roller is carried to be free to move in any direction horizontally, let a force be gradually applied at the center of the roller AB in the direction perpendicular to the plane of rotation. This will produce no effect as long as it is less than the maximum resistance, which can be opposed by friction between the edge of the roller and the surface upon which it rests, that is, as long as

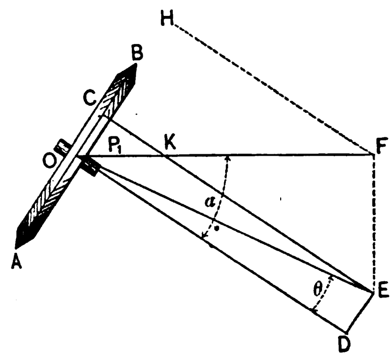
$$R = OD \text{ (Fig. 50) } < S\mu.$$

When the force R is equal to $S\mu$, and acts within the angle θ, the roller AB will move with uniform motion along the line of action of the force without turning. The same thing will hold if, instead of the force acting perpendicularly to the plane of rotation, it acts at some oblique angle to it not exceeding a certain value measured from the normal. The limiting value of this angle depends on the resistance of the friction of the axle of the roller AB to turning. Let this angle be (θ), and draw OE perpendicular to OC, meeting the circle drawn with O as center and radius OD in E, and let

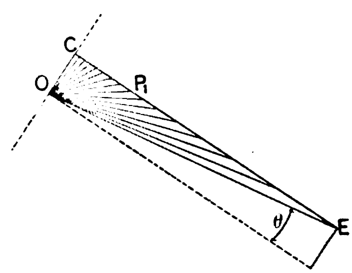
$$\theta = \text{angle EOD}.$$

When the line of action of the force falls without the angle θ, as, for instance,

Fig. 50



CLASS 2.



when it takes place in direction OF, the roller will still slip along the line of the force, but the roller will now also turn. The component in the plane of rotation will now, however, be of a magnitude such that the motion of rotation of the roller is no longer uniform. Since only uniform motion is being considered to take place, the conclusion is, that when the force acts at an angle greater than θ to the axis of rotation (*i. e.*, when $a > \theta$) it must never be so great as to cause the roller AB to slip, and therefore only a motion of pure rolling can take place. By proper mechanical devices the roller can be made to turn very easily, and angle be kept very small.

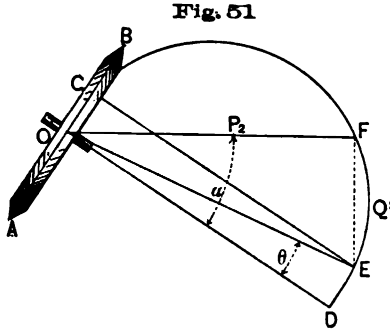
The magnitude of the force which must be applied in any position of the roller to effect this motion, is

$$P_1=\mathrm{OK}=\frac{\mathrm{OC}}{\sin\alpha}$$
$$=\mathrm{F}\ \mathrm{cosec}\ \alpha,$$

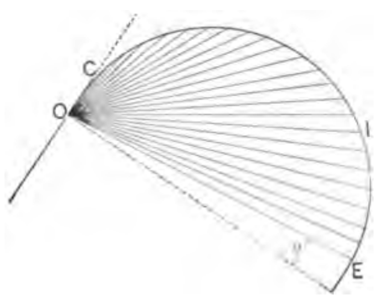
and is at once given by the intercepts drawn from O to CE in the construction, shown in Fig. 50, for any other value of $\dot{a}$.

Class I. (Fig. 51)—Suppose that the

Fig. 51



CLASS 1.



frame does oppose restraint, and that this restraint is such as to always cause the center of AB to move in the direction in which the force acts. Let OF (Fig. 51) lie in this direction, making the angle α with the axis of AB, draw EF perpendicular to OF from the point E, then by the triangle of forces. The force required to move AB is

$$P_2=\mathrm{OF}=S\mu\cos(\alpha-\theta).$$

The reaction which is supplied by the frame is

$$Q=\mathrm{EF}=S\mu\sin(\alpha-\theta).$$

By describing a semicircle upon OE a construction is at once given, as shown in Fig. 51, in which, by drawing radial lines from O, the value of P_2 for any angle is at once given by the intercept. The above investigation is by no means a complete one, for this would require a discussion of the moments acting, but having obtained the above result by the more complex method, the author considered it unnecessary to introduce a more detailed proof than has been given.

Comparing the two foregoing cases by means of the diagrams in Figs. 50 and 51, Diagram AB, it is clear that the forces acting always differ, except in the limiting cases; then

If $\alpha=90^\circ$ $\quad P_1=P_2=F=\mathrm{OC};$

$\alpha=0^\circ$ $\quad P_1$ is $<S\mu;$

P_2 is $\gtreqless S\mu.$

In order to examine to what extent the assumpltions hold good upon which the accuracy of integrators rests, the experimental determination of the following points is needed.

I. Case of boundary measures and limiting case of both classes of area and moment integrators.—The conditions of rolling of two surfaces when the force acts in direction of the plane of rotation.

II. Class I. of area and moment planimeters.—Whether the rolling is proportional to the slipping, as assumed.

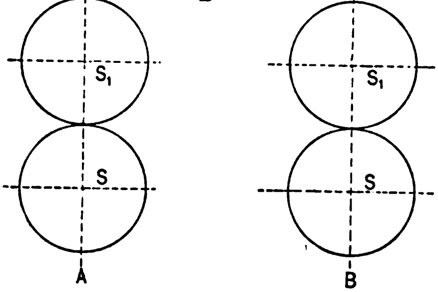
III. Class II. of area and moment planimeters.—Whether there is any slipping in this case, and if so, to what extent.

Case I. There are two separate problems under this head; one, where the planes of revolution of the two rolling surfaces coincide; and the other, when they do not coincide.

Dr. A. Amsler has experimentally investigated the former thus: Two accurately turned disks, SS', (Fig. 52), each 200 millimeters in diameter, were placed with their edges in frictional contact, so that the mark shown on each coincided at the point of contact. By means of the apparatus (Fig. 53) the lower one S was turned by means of a band (BB') through 1700 revolutions one way (thus turning the upper one S_1), and then 1700 revolutions in the opposite direction. The marks which should have coincided if no slipping had taken place, were now found to be as in Fig. 52, the marks being a distance $=\varepsilon_n=0.05$ millimeter apart. The relative error is thus $=\dfrac{\varepsilon_n}{2n\pi 200}=$ (about) $\dfrac{1}{40{,}000{,}000}$. This experiment is

only of value to show the error when the wheel travels back by turning in the opposite direction, and at the most, shows that the error is nearly the same in both directions, and does not prove anything with regard to the action of the measuring roller of the boundary measurer; concerning this point, observations were wanted upon the results of the first 1700 revolutions. A very good way of exam ining the point would be to note the diver

Fig. 52



gence of the marks in x revolutions when S drives S_1, then cause S_1 to drive S back through the same number of revolutions, and it would be seen whether the divergence was due to a difference in the periphery of the wheels, or to a slipping of the surface of contact. During Dr. Amsler's experiments no one was allowed to enter the room, in order to avoid alteration of temperature. He found that the heat radiated from the human body, or even from a lighted candle placed at some distance, had a perceptible influence on the result.

Fig. 54 shows a most important case, in which the plane of rotation of the surfaces in contact do not coincide with that of the measuring roller (m) being actuated by the disk M. Fig. 55 shows Dr. Amsler's apparatus for examining this case, in which, if the edge of the roller has any width, there must be slipping action, even though the force always acts in the direction of the plane of rotation.

The roller m which rests on the upper surface of the disk, which latter has its edge divided, and is in juxtaposition with a vernier (v). The axis of the roller is fixed, and its edge is thus kept always vertically under a microscope (K). The position of the disk is noted, and it is then moved forward about 8 revolutions (or exactly 2,900°), which gives the roller about 130 revolutions, and a mark is observed on the latter. Then in theory the result of giving 8 more revolutions to the disk in the same direction should be to bring the same mark of the roller under the microscope, Practically the successive motions of the disk will be a little different, so that the second advance of the disk will not be exactly the same as in the first case. The same mark on the roller is, however, always brought under the microscope, and the difference in turning of the disk is what is noted.

In the following table—

i=number of experiment,

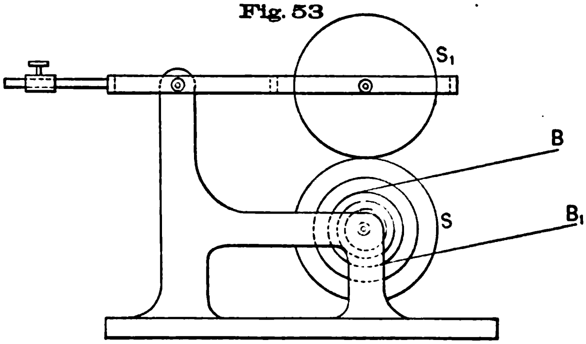
φ=angle by which the disk differs from last reading,

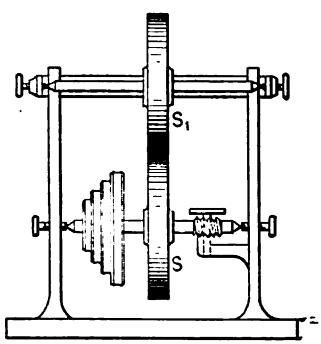
so that the second column gives the positions of the disk at the end of successive advances in which the roller is made to take 130 complete revolutions, the third column shows the travel of disk in minutes (2,900° having, of course, to be added to the readings). The fourth gives the difference between these and a mean value. The last gives the ratio of these differences to the travel.

i.	$\phi_i - i\,2,900°$.	$\phi_i - \phi_{(i-1)} - 2,900°$.	$m-(\phi - \phi_{i-1}$	$\frac{m-(\phi_i - \phi_{(i-1)})}{2,900°}$.
	′	′	′	
0	2.58	47.22	−0.06	−0.0000003
1	49.80	47.28	−0.12	−0.0003007
2	37.08	47.34	−0.18	−0.0000010
3	24.42	46.98	+0.18	−0.0000010
4	11.40	47.22	−0.06	−0.0000003
5	58.62	47.04	+0.12	+0.0000007
6	45.66	46.98	+0.18	−0.0000010
7	32.64	47.34	−0.18	+0.0000010
8	19.98	46.86	+0.30	+0.0000017
9	06.84	47 34	−0.18	−0 0000010
10	54.18			

Case II. To test the results when a roller partly rolls and partly slips, Dr. Amsler used the apparatus shown (Fig. 53) in plan and elevation. In this C is a carriage, running upon four wheels, on the base (B), which has parallel grooves planed in it; the travel of the carriage being limited by two stops at the ends. Upon the surface of C the measuring

Fig. 53





roller (m) rests, being attached to a plate A. By means of the graduations on A the axis of (m) can be set at any required angle with reference to the direction of motion of the carriage. The frame supporting the roller is carried on the disk by means of pivots, so as to allow (m) to rest on the surface of C with the constant pressure of its weight.

If a=angle of axis of (m) in the direction of the motion of the carriage;
s=motion of the carriage;
u=turning of the roller;

Then $u=s \sin a=s \cos\left(\frac{\pi}{2}-a\right)=s \cos \beta$.

If φ=actual reading of vernier v;
and $\varphi=\varphi_0$ when $a=90$ or $\beta=0$;
then $u=s \cos(\varphi-\varphi_0)$,
$u_1=s \cos(\varphi_1-\varphi_0)$,
$u_2=s \cos(\varphi_2-\varphi_0)$, etc.;

Then $\tan \varphi_0=\frac{u_1 \cos \varphi_2-u_2 \cos \varphi_1}{u_2 \sin \varphi_2-u_1 \sin \varphi_1}$,

$$s=\frac{u_1}{\cos(\varphi_1-\varphi_0)}=\frac{u_2}{\cos(\varphi_2-\varphi)}.$$

In the following Table, which represents the results of experiments when the disk was covered with a surface of pear-tree wood, carefully polished (paper being, however, found to afford almost as good results):

i=as before, the number of the experiment;
B_i=angle of inclination of roller for experiment (i);
u'_i=motion of roller as observed for experiment (i);
u_i=motion of roller as calculated.

i.	$\beta_i = \phi_i - \phi_0$. °	′	″	Experiment u'.	Calculated u_i.	$u_i - u'_i$.
0	0	0	0	9,649	9,649	0
1	8	9	36	9,552	9,551	−1
2	9	36	0	9,511	9,514	+3
3	26	9	36	8,657	8,661	+4
4	27	36	0	8.550	8,551	+1
5	44	9	36	6,916	6,922	+6
6	45	36	0	6,750	6,751	+1
7	62	9	36	4,505	4,506	+1
8	63	36	0	4,290	4,290	0
9	80	9	3	1,650	1,649	−1
10	81	36	0	1,415	1,410	−5

Class III.—The actual conditions of motion when a force smaller than the component of $S\mu$, acts obliquely to the plane of rotation of the measuring roller, do not appear to have been made the subject of direct experiment. It is apparently always tacitly assumed that no slipping takes place. But this crucial point cannot be thus left to mere conjecture, and the author has designed a method of carefully testing this, which he has not yet been able to properly carry out. From a few rough observations, there seems little doubt, howevei, that some slipping always does take place, and that its amount is, in the limiting cases, by no means inconsiderable.

Lastly, a few words may be said concerning the work of Professor W. Tinter and of Professor Lorber. The former has examined most carefully no less than nine different planimeters, from which he concludes that the different angles at which the measuring roller of the polar planimeters has little effect upon the result, and that, taking one turn of the measuring roller as $f=100$ square cm., the average error in the reading was only from $=0.00075$ to 0.0013, according as the center of rotation was without or within the area to be measured. The work of Professor Lorber is so extensive and elaborate that it is impossible to do more than give in the most brief form the results at which he has arrived after many thousands of experiments. He concludes that error in the reading is always represented by an equation of the form—

Fig.54.

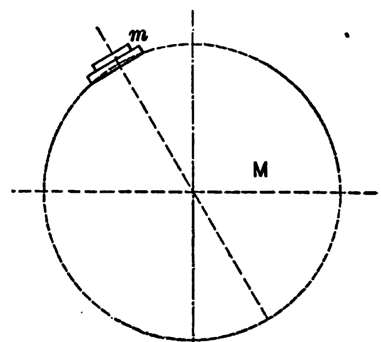
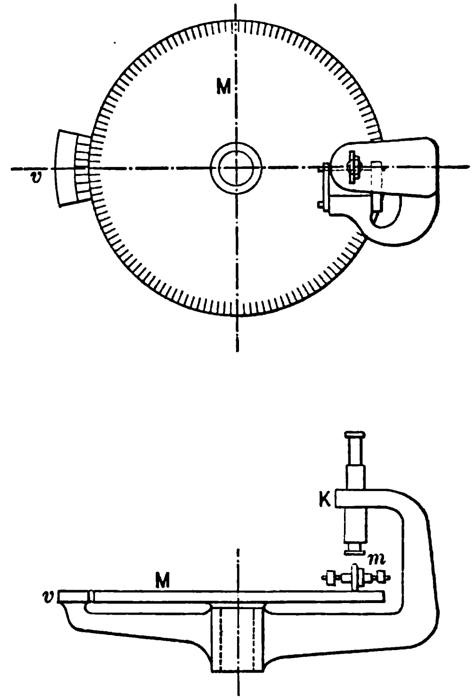


Fig.55



$$dn = K \cdot + \mu\sqrt{n},$$

where dn = the error in the reading, K and μ being constants.

n = the reading of the measuring roller;

the above equation gives rise to one of the following form:

$$dF_n = Kf + \mu\sqrt{Ff},$$

where F = actual area to be measured,

and dF_n = the error in the result expressed in terms of the area.

The following are the results given in his latest paper:

Linear planimeter $dF = 0.00081f + 0.00087\sqrt{Ff}$

Polar planimeter $= 0.00126f + 0.00022\sqrt{Ff}$

Precision polar planimeter $= 0.00069f + 0.00018\sqrt{Ff}$

Freely swinging planimeter $= 0.00060f + 0.00026\sqrt{Ff}$

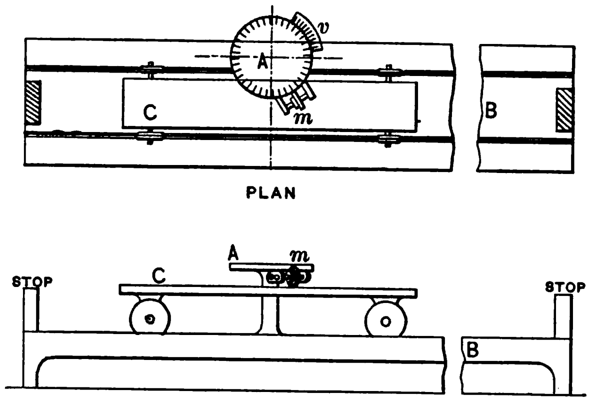
Simple plate planimeter $= 0.00056f + 0.00084\sqrt{Ff}$

Rolling (Coradi) planimeter $= 0.0009\ f + 0.0006\ \sqrt{Ff}$

The degree of accuracy represented by these results may be inferred from the fact that in one case of the last planimeter, when

$f = 100$ the relative error $= \frac{dF}{F} = \frac{1}{13{,}330}$.

Fig. 56



SHRINKAGE OF EARTHWORK.

By P. J. FLYNN.

Transactions of the Technical Society of the Pacific Coast.

Some years since, experiments were made in Dharwar, in India, by Mr. J. H. E. Hart, C. E., on the shrinkage of earthwork, and the results of these experiments agree with the results arrived at by Mr. Specht (Transactions Technical Society, May, 1885), namely, that the embankment at first gave a volume larger than that of the pit from which the material with which it was constructed was excavated.

Mr. Hart had three pits excavated, and the material from each pit was formed into a bank. The first pit, in black cotton soil, was $49' \times 4\frac{1}{2}' \times 2' = 416$ cubic feet. This was formed into an embankment which measured $50' \times 6' \times 2' = 600$ cubic feet. After the bank was measured, some of the earth from it was filled back into the pit, up to the level of its edge, and the balance remaining measured and found to be equal to 191 cubic feet. We have here, first—An increase from pit to bank of 184 cubic feet; and, second, again an increase from bank to pit of 7 cubic feet.

During the rainy season (and the rains in Dharwar are heavy), as the material in the pit sank it was filled up to the level of its edge, and at the end of the rainy

season the balance was measured, and found to be reduced to 22.5 cubic feet. This showed an increase of the made ground over the pit by 5.3 per cent. The second and third experiments were made in red gravelly soil, or moorum, which is an iron-stone gravel, mixed with about 50 per cent. of earth.

In the second experiment a pit $49\frac{1}{2}' \times 4\frac{1}{4}' \times 2' = 420$ cubic feet, formed a bank which measured $49\frac{1}{2}' \times 6' \times 1\frac{3}{4}' = 520$ cubic feet, giving a first increase of 24 per cent.

A third pit, $49\frac{1}{2}' \times 4\frac{1}{2}' \times 2' = 445$ cubic feet, was excavated, and the material again filled back into pit, leaving a balance of 111 cubic feet=25 per cent., as a first increase. During the rains the pit was kept filled to level of edge, and after the rains the balance was 55 cubic feet, giving an increase of $12\frac{1}{2}$ per cent.

Although these experiments are on a small scale, still they are of some use as information on a branch of engineering that has not been experimented on as fully as the subject requires. The experiments, however, would be more useful were full information given as to how the embankments were constructed and the pits filled, that is, as to whether wheelbarrows or baskets were used for the carriage of the material, and the earth deposited in layers and rammed; or as to whether the earth was simply thrown out from the pit and thrown back again after measurement in bank.

From the large increase of embankment over the excavation, the inference to be drawn is that the material was not rammed.

Embankments in India are often constructed by basket work, the material being carried in saucer-shaped, wicker baskets, containing less than a cubic foot. In the construction of embankments to retain water, this basket work is done in thin layers of less than nine inches in depth, the earth being roughly leveled up as it is deposited from the baskets, and then well punned with wooden or cast-iron rammers weighing about 12 pounds. In addition, the constant trampling of the men, women and children employed in carrying the baskets, so consolidates the bank as to make it impervious to water. The layers of earth are sometimes watered. Embankments constructed in this manner shrink or settle very little after they are finished. They are, in fact, an approach to puddle work, though not nearly so expensive.

The writer has constructed embankments with a grading machine, tipping from wagons from grade, wheelbarrows, hand-cars, carts, scrapers, and punned basket-work, and of all these he believes that punned basket-work settles the least, and is the best suited for hydraulic work, and the next best work to it for a similar purpose is that done by scrapers.

Thousands of embankments, and some of them counted amongst the largest and oldest dams in the world, have been constructed in India by basket-work, without any puddle wall or puddle lining; and some of them, that have been looked after and kept in repair, are as good, if not better, at the present day than when they were originally constructed, hundreds of years since.

This kind of work is done much cheaper there than earthwork in this country. The writer has constructed embankments in the Panjab, the lead being from 100 to 200 feet, for three rupees per thousand cubic feet, that is, at the rate of four cents per cubic yard.

More than forty-five years since, Mr. Ellwood Morris, C. E., made some experiments, on a large scale, on the shrinkage of earthwork. The embankments were formed in layers by carts and scrapers, and one winter intervened between the commencement and completion of the work. The results of the measurements are given in the following table:

Nature of soil.	Excavation, Cubic Yards.	Embankment, Cubic Yards.	Compression.	
			In Cubic Y'ds.	Proportional.
Yellow clayey soil............	6,970	6,262	708	0.1015
" "	25,975	23,571	2,404	0.0925
Light sandy soil..............	10,701	9,317	1,384	0.1293
On the whole...............	43,646	39,150	4,496	0.1030

The total average compression in embankment being a little more than 10 per cent. of the excavation. Other observations, on a smaller scale, showed that gravelly earth shrank about one-twelfth. The results of these experiments, along with the experiments on rock, are given in the table at page 464.

With a few exceptions the results of these experiments have been heretofore used, and are still in use to the present day, in American, English and Indian engineering practice.

As a rule, books of reference in the English language give the shrinkage of different materials, without making any allowance on account of different methods of construction and different heights of bank. For instance, the shrinkage of earth, in general, is given at about 10 per cent. Now, if 10 per cent. be sufficient for the shrinkage of a bank of that material, and 30 feet in height, constructed from the end of bank to the full height by "tipping" from wagons, surely a similar bank only 12 feet high, built up in layers, and consolidated by good scraper work, will shrink much less than 10 per cent.

In no other branch of civil engineering, since the time when railroads were first commenced, has such an immense quantity of work been carried out, and expenditure incurred, as in earthworks; and in no other branch of engineering, of equal importance, have so few experiments, on a scale adequate to the interests involved, been published. In other branches of engineering, long, tedious, and expensive experiments are carried out without any other return resulting from them than the information they give; but experiments on earthwork could be carried out on a large scale, as actual work, and with little, if any, additional expense more than the contract price of the work.

In the experiments that have been made there is a want of general agreement, and in some cases the results obtained, in similar materials, differ so much from each other as to point more to errors made by some of the observers than to errors resulting merely from the different methods of construction. This is well illustrated in the table which I now give, showing the percentage of increase or diminution from cut to fill. Some of the materials are mentioned more than once, with a slight change in name, but I deem it better to give the author's own words, descriptive of the material, than to make a selection of the materials under a fewer number of names.

The experiments of Henz, Von Kaven and Graeff, given p. 464, are taken from Mr. Specht's paper, already referred to. The experiments of Henz, quoted in the that table, are stated to give the *permanent increase* in volume from cut to fill, and to be the result of a *large number of observations* of actual work.

From an inspection of the table it will be seen that Henz gives a permanent *increase* in volume of from 1 to 6 per cent. —sand and clays—for materials of the same description as those for which Mr. Morris and other engineers allow a *shrinkage* of from 8.5 to 12.5 per cent.

Then, again, for rock, Henz allows an increase of from 8 to 12 per cent., and Von Kaven an increase of from 8 to 10 per cent., but, for the same material, Morris allows 42 to 60 per cent., Searle 60 per cent., Trautwine 66 to 75 per cent., and Molesworth 50 per cent. of increase.

The writer is at present engaged on the construction of the south jetty of Oakland harbor for the United States Government. In the carefully-laid dry masonry of this work, where all badly-fitting stones are rejected for face work, and the stones are too large to be laid by hand, and require a derrick for that purpose, the voids, before chinking is done, are more than 12 per cent. of the laid face work. There is no method of railroad construction by which an embankment can be made so as to have as few voids as this jetty. On the contrary, however, by the usual methods of construction, the voids will be found to be from six to seven times more than the quantity given by Henz.

The difference in rock between the German, and American and English experiments is very great, but it will not be difficult to prove that there is something wrong with the German rock experiments. Trautwine gives the average weight of granite at 170 pounds to the cubic foot, and he also gives the weight of a cubic foot of roughly-scabbled, dry rubble granite masonry at 125 pounds. There is, therefore, an increase in volume of 36

Material.	Authority.	Percent. of Increase + or Diminution — of Emb'nkm't to Excavation.	Remarks.
Sand	Hewson	−10	
Very light sand	Graeff	−10	
Sandy soil	Henz	+1 to +1.5	
Sandy loam	Specht	+9	After fill is finished.
Light sandy earth	Morris	−12.5	
Light sandy soil	Molesworth	−11	
Gravel and sand	Vose	−9	
Sand and gravel	Trautwine—Searle	−8	
Earth	Miss. Levees, 1882		⅛ addition to height of bank.
Earth	Simms	−10	
Earth (scraper work)	Canadian Pacific R. R.		Shrinkage of bank 10%.
Earth (grading machine)	Canadian Pacific R. R.		Shrinkage of bank 15 to 17%.
Earth (carefully tamped)	Graeff	−9 to −20	
Loam & light sandy earth	Vose	−12	
Loam	Trautwine—Searle	−12	
Clay and loam	Von Kaven	+2	After fill is finished.
Clay and light soil	Henz	+3	
Clay and earth	Vose	−10	
Yellow clayey earth	Morris	−10	
Gravelly earth	Morris	−8.5	
Gravel	Molesworth—Vose	−8	
Clay	Hewson		⅛ addition to height of bank.
Clay	Trautwine—Searle	−10	
Marl	Henz	+4 to +5	
Hard clay	Von Kaven	+5	After fill is finished.
Hard clay	Henz	+6 to +7	
Clay before subsidence	Molesworth	+20	
Clay after subsidence	Molesworth	−8	
Puddled clay	Trautwine	−25	
Wet soil	Searle	−15	
Loose vegetable surf. soil	Trautwine	−15	
Chalk	Molesworth	+30	
Soft sandstone	Von Kaven	+3	After fill is finished.
Rock	Von Kaven	+8 to +10	After fill is finished.
Rock	Henz	+8 to +12	
Rock	Vose	+50	
Rock	Graeff	+50 to +60	
Rock	Rhine Nahe Railroad	+25	
Rock	Trautwine	+66 to +75	
Rock, large fragments	Searle	+60	
Hard sandstone rock, large fragments	Morris	+42	
Blue slate rock, small fragments	Morris	+60	
Rock, large blocks	Molesworth	+50	
Rock, medium fragments	Searle	+70	
Rock, medium unselected	Molesworth	+25 to +30	
Rock (metal)	Molesworth	+20	
Rock, small fragments	Searle	+80	
Rock fragments (loose heap)	Trautwine	+90	
Rock fragments (carelessly piled)	Trautwine	+75	
Rock fragments (carefully piled)	Trautwine	+60	
Rock mixed ⅓ to ¼ clay	Von Kaven	+9	
Rock with considerable clay	Graeff	0	

per cent. from solid rock to dry masonry. In order to reduce the increase to only 8 per cent., given by Henz, the voids in his rock embankment would have to be less than one-fourth of that of the dry granite masonry mentioned. In railroad construction, as generally carried out, this is not possible. Well-dressed granite or limestone uncoursed masonry contains more than 8 per cent. of mortar. General Gilmore, in his work on "Limes, Cements, etc.," states that ordinary masonry, in *courses* of 12 in. to 20 in. rise, contains about 8 per cent. of mortar.

If the percentage of increase allowed for rock is to be accepted as a fair sample of the accuracy of the experiments of Henz and Von Kaven, as a whole, then the conclusion to be arrived at is, that any estimates based on them must be inaccurate and lead to serious errors.

The disagreement of the German authors from the American and English authors as to shrinkage, is certainly remarkable. The Germans give, on the whole, a permanent increase for sands, clays, and similar materials, but, on the contrary, the Americans and English give a diminution. For rock, the latter give an increase from cut to fill many times more—in some cases nine times more—than the former.

American and English authorities also differ materially on some points, as shown in the small table below, which gives the percentage of increase for rock of different sizes:

therefore amount to 44, which is equivalent to an increase in volume of 79 per cent. from the solid rock. This agrees with Searle and Trautwine.

In Gillespie's "Roads and Railroads," he gives a quotation from Gayffier, which says:

"A cubic meter of broken stones, placed in a water-tight box, which they just fill, can receive in the empty spaces between the fragments a volume of water $\frac{48}{100}$, or nearly one-half of the whole, the actual solidity of the stones being therefore only $\frac{52}{100}$. This does not vary for stones from 1 to 8 inches in size."

This is equivalent to an increase of 92 per cent., and is practically a close agreement to the result obtained by Trautwine, with a loose heap of rock fragments, which gave an increase of 90 per cent.

As an instance of large percentage for shrinkage, I may mention the construction of the Western Division of the Canadian Pacific Railway, where 10 per cent. was allowed for the subsidence of earthen embankments constructed by scraper work, and 15 to 17 per cent. for similar banks constructed with a grading machine.

Another instance of still larger percentage is that on the construction of the levees of the Mississippi (1882), where 17 per cent. is allowed for shrinkage of *embankment*, and this, be it remembered, is for an embankment which is intended to keep out water. The specifications do not state the method of construction, and

	Molesworth.	Searle	Trautwine.
Rock, large fragments..........	+50	+60	+66 to +75
Rock, medium fragments..........	+25 to +30	+70	....
Rock, small fragments..........	+20	+80	+60 to +90

In this table it will be seen that Molesworth makes the voids to decrease with the decrease in the size of rock, whereas Searle and Trautwine make the voids to increase with the decrease in size of the rock.

Under the heading of "Volume of Interstices in Concrete," Molesworth gives, for five descriptions of small stone, the "percentage of total"—that is, the percentage of interstices to total volume. The mean of these five is 44. In a volume of 100, including voids, the voids

no mention is made of a grading machine, but it is stated that "a sufficient number of dumping men be kept on the levee to spread the earth as it is wheeled or carted in."

In sandy loam Mr. Specht found the volume of embankment, after completion, to increase more than 9 per cent. over the excavation. Morris and other authorities found that similar materials had an ultimate shrinkage of over ten per cent. Therefore, to cause an equal shrinkage of 10 per cent. from cut to fill,

in Mr. Specht's work, his *embankment*, after completion, would have to shrink about 18 per cent. In other words, a bank of loam, 10 feet high, built up in layers by scraper work, would have to settle about 2 feet.

An examination of the table at page 464 will show other serious differences amongst the authorities given, in the percentage allowed for shrinkage.

In such experiments any near agreement is not to be expected, but at least it is reasonable to expect a general agreement tending one way from the same materials in similar banks. The experiments conducted in this manner should all show, for similar materials, either a diminution or increase, and not a diminution in one case and an increase in the other.

In the practice of American and English engineers there is considerable divergence amongst themselves as to the *subsidence* or settlement of embankments, which *subsidence* again depends on their shrinkage in volume. Some authors give the percentage of shrinkage without any reference to settlement of bank, and in these cases the inference to be drawn is that the settlement is the same as the shrinkage. Other authors give rules for the settlement which do not agree with the shrinkage.

In order to show the want of accord in the rules given, I give three examples from three well-known authors. Searle, in his "Field Engineering," gives the ratio of shrinkage to cut for different materials, and adds, with reference to settlement:

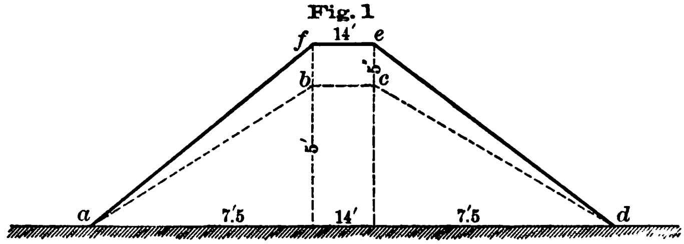
"The lineal *settlement* will be about in the ratios given above."

An investigation will show this rule to be correct.

Alexander L. Holley, C. E., in his "Railway Practice," states:

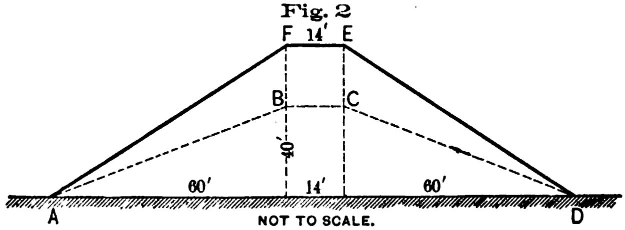
"The shrinkage of earthwork is stated to be as the cube of the depth; hence the necessity of due provision in lofty banks."

Fig. 1



NOT TO SCALE.

Fig. 2



NOT TO SCALE.

For the purpose of illustration, the shrinkage and settlement are assumed for a bank 5 feet high, and from this the settlement of a bank 40 feet high is found. The bank shown in Fig. 1 (not to scale) is 14 feet wide on top, 5 feet deep, and has side slopes $1\frac{1}{2}$ to 1.

After ultimate shrinkage the five-foot bank is assumed to have settled 3 inches, therefore the area of shrinkage will be *a*, *b*, *c*, *d*, *e*, *f*, *a*, on fig., equal to $21.5 \times .25 = 5.375$ square feet, which is a reasonable shrinkage, being only 5 per cent. of the area of the bank.

Now, as $40^3 : 5^3 :: 512 : 1$.

Therefore, the shrinkage of the 40 feet bank will be:

$$5.375 \times 512 = 2,752 \text{ square feet.}$$

This is equal to A, B, C, D, E, F, A, Fig. 2; and, as the shrinkage is all vertical, the area of shrinkage divided by mean width =settlement.

Therefore the settlement $= \frac{2752}{74} = 37$ feet, being almost the full height of bank.

If the shrinkage of the 5-foot bank be taken at only 2 per cent., then, by the rule of the cube of the depth, the settlement of the 40-foot bank will be 14 feet. From these examples it will be seen that this rule does not hold good, and that the shrinkage is not as the cube of the depth. Mr. Holley evidently copied this rule without investigation.

There is a sort of scientific look about the rule, but an investigation will show that it is nothing more than a most useless and misleading rule of thumb.

To show how rules are sometimes glanced over and accepted without investigation, it may be mentioned that this rule was given, several years since, in a work which, in other respects, is accurate, namely, the "Treatise on Civil Engineering," in use at the Roorkee Civil Engineering College, and, in all likelihood it is continued in it to the present day in the last edition. It was not until long after the writer left the college that he tested its accuracy and found out its fallacy.

Vose, in his "Manual for Railroad Engineers," quotes the experiments of Morris for shrinkage, after which, and under the heading of "Subsidence," he states:

"It has in some cases been specified that the embankments, when completed by the contractor, should be finished to the full height from three inches above the intended height, upon a bank 5 feet high, to nine inches upon a 40-foot bank, and intermediate heights being in proportion."

Vose here gives a rule that in a certain case, when a 5-foot bank subsides 3 inches, a 40-foot bank will subside 9 inches. He also states that banks shrink a certain percentage *in proportion to their volume.* As already shown, the shrinkage of a 5-foot bank, and corresponding to a subsidence of 3 inches, is 5.375 square feet, which is five per cent. of the area of bank, 107.5 square feet.

Now, a 40-foot bank (Fig. 2) has an area of 2,960 square feet, and 5 per cent. of this=148 square feet=its shrinkage; and, as this shrinkage occurs only in the vertical direction, $\frac{148}{74} = 2$ feet=the depth of settlement: and not 9 inches. From this it will be seen that the rules for shrinkage and settlement, as given by Vose, do not agree with each other.

I think I have shown that there is a great want of general agreement in the results of experiments on shrinkage, and also in the rules for settlement as given by writers on the subject.

Holley says: "Correct allowance is made for the settling of the material of the bank, and time is given for this settling to occur before the ballast is brought on or the rails and sleepers laid. The shrinkage of earthwork sometimes disturbs the grade at a rate of several feet rise or fall per mile—in normal grades of 60 feet, on the New York and Erie Road, a distance of 500 feet was found to rise at the rate of 75.4 feet per mile, this distance being approached and succeeded by the regular grade of 60 feet. In another place, for the distance of 200 feet, the rise was found to be at the rate of 116.7 per mile, with a level of 100 feet length both above and below—the average grade over the whole mile being 60 feet. These cases are similar to what occurs where railway earthwork is not properly settled before being brought into use."

On the assumption that the settlement is in proportion to the shrinkage in volume, I will now show that, even when due allowance for settlement is given to the bank to make up for the shrinkage in volume *from cut to fill*, still cases happen where the bank will settle more than that allowance, and it is possible that the banks mentioned by Mr. Holley had, when they were first built, the full allowance for settlement usually given.

It is well to note here that there is a marked difference, in some cases, between the shrinkage of an *embankment*, and the shrinkage of the material of which it is composed, *from cut to fill.*

Trautwine says: "Although earth, when first dug and loosely thrown out, *swells* about $\frac{1}{5}$ part, so that a cubic yard *in place* averages about $1\frac{1}{5}$, or 1.2 cubic yards, when dug; or 1 cubic yard dug is equal to $\frac{5}{6}$, or to .8333 of a cubic yard in place; yet, when made into an embank-

ment it gradually subsides, settles, or shrinks into a less bulk than it occupied *before being dug.*"

Molesworth, in his "Pocket Book," states that clay, before subsidence, increases in volume 20 per cent. from cut to fill, but that after subsidence it shrinks in bank 8 per cent. less than the volume in cut. The *embankment* therefore shrinks from a volume of 120 to a volume of 92. This amounts to a *total shrinkage of embankment* of 23 per cent.

Mr. Specht's experiments support these statements as to the first or temporary increase from cut to fill. He found that "53,350 cubic yards in cut gave 58,350 in fill," in "sandy loam with small amounts of adobe and hard pan," which is an increase of 9.4 per cent. Mr. Specht remarks, with reference to this increase: "This question must not be confounded with that of final settlement, which sometimes even continues for a long time after the embankment is finished."

The temporary increase of volume in embankment, mentioned below, has reference to banks built up with some rapidity, to full height and without layers.

In the table, page 464, it will be seen that, for materials similar to Mr. Specht's embankment, American and English engineers give a permanent shrinkage from cut to fill of from 8 to 12.5 per cent., being an average of about 10 per cent.

This final shrinkage, as well as the first increase, being allowed, it is evident that the *total shrinkage of an embankment from its first completion* to its *permanent settlement*, must equal the *first increase* in bank beyond the volume in cut, plus the *final shrinkage* from cut to fill, that is, in the case just given, $9.4+10=19.4$ in a volume of 110, which is equivalent to 17.6 per cent.

In order to have a permanent depth of 40 feet after settlement, an embankment in the material just described would require to be built 48.5 feet in depth at first.

If the banks mentioned by Mr. Holley were intended to be, say as an instance, 46 feet high after final settlement, and that the material of which they were constructed increased at first 5 per cent. more than cut, and had a final shrinkage of 10 per cent. less than the cut, then the total shrinkage of 14 per cent. would require the depth at first to be 53.5 feet. If, however, 10 per cent. be allowed only for shrinkage, the bank will at first be 51.1 feet deep, and as the actual settlement of this at 14 per cent. will be 7.2 feet, the bank at final settlement will be only 43.9 feet deep instead of 46 feet, which is 2.1 feet lower than intended. This is as much as the greatest settlement of bank already mentioned by Mr. Holley, and is due entirely to the fact that no increase in depth of bank was allowed in proportion to the first increase in volume from cut to fill.

Is it not probable that a great deal of the settlement noticed in earthen banks is to be accounted for as just explained?

For *balancing* cuts and fills, only the permanent difference of volume in cut and settled bank is to be taken into account, but for settlement of bank an additional allowance has to be made for the temporary increase in volume from cut to fill.

Cases occur where it would not be advisable to allow for the full shrinkage of a bank during construction, and it is here that the element of *time* is of importance. Hewson, in his work on "Levees," says:

"Sand, however loosely it may be shoveled together, fills its space closely, and, therefore, whether wet or dry, settles at a very small diminution of the original bulk. In *time*, too, the process of this settlement is short."

"Spon's Dictionary of Engineering" states:

"The ultimate settlement of embankments of gravel or chalk does not require more than two or three years, but clay embankments may, in some cases, continue to shrink for ten years."

To give an instance where it would not be advisable to allow for the full shrinkage. Let an embankment, on a maximum or ruling grade of 1 in 100, be built across a hollow, and let its depth gradually increase until at a distance of 200 feet from beginning of fill, its depth is 40 feet. From experience with similar material, it is known that the embankment will shrink about 10 per cent. within two years, of which 5 per cent. takes place within six months, and another 5 per cent. within a further period of 18 months. The road is to be in full operation within about six months from construction of the embankment. Now, if

an allowance is made in height of bank for ultimate shrinkage of 10 per cent., then its height at greatest depth will be 44.4 feet; but as, within six months, it will shrink only 5 per cent., then its height at that time will be 42.2 feet—that is, 2.2 feet higher than grade. This 2.2 feet additional height is gained in 200 feet, and it increases the grade of road from 1 in 100 to a steeper grade than 1 in 50, but as the ruling grade is to be 1 in 100, the allowance for ultimate settlement during the construction of the bank is not admissible. If, however, shrinkage for only 5 per cent. be allowed at first, and the bank built up to 42.1 feet in height, it will at the end of six months have settled down to 40 feet and be even with the ruling grade. After this, as the bank gradually settles, it can in the regular course of track repairs, be brought to grade by the trackmen with additional ballast.

As to the effect of *time* in the consolidation of banks, the writer is of opinion that an embankment, built of loam with a slight admixture of sand, does not after several years so shrink and consolidate as to equal for hydraulic work to a comparatively new bank built in layers of less than one foot thick and rammed. The effect of time does not consolidate the bank so much as punning during construction. This opinion is based upon considerable experience in cutting through banks of the material mentioned.

Very old banks are not here referred to, as the writer has had no experience with them.

In the original construction of that part of the Grand Trunk Road from Lahore to Wuzeerabad, in the Panjab, provision was made for drainage—by bridges and culverts—only at the well-defined drainage channels. After the completion of the embankments, the drainage works were found insufficient to carry off the flood waters, as before the construction of the embankment, more water was passed away over the surface of the country than by the well-defined drainage channels. In times of very heavy rainfall the embankment dammed back this water, which rose behind it, flooded the country on the up-stream side of the bank, breached the road, carried away bridges and impeded the traffic.

In the construction of bridges and *metaled gaps* to carry off the flood waters, the writer had to cut through the embankments in more than twenty-four places, making an aggregate length of more than 8,000 feet in a distance of fifty-eight miles.

In places where the bank was built without punning, it was found that after taking off about two feet in depth of the top, the body of the bank appeared like material newly deposited, and it was easily excavated. On the other hand, the banks which were built in layers and punned, were found to be well consolidated and much more difficult to excavate. The difference in the banks, though built of the same material, was very marked, the punned banks, even when comparatively new, being much more difficult to excavate than the others.

After each rainy season the tops and slopes of the unpunned banks were much more cut up and fissured, and required more repairs than those that were punned.

The banks were forty feet wide on top, with side slopes of two to one and from four to twelve feet high.

In some cases, during floods, the unpunned banks were breached by leakage, before the water reached their tops, but not in a single instance was the punned bank reached by the leakage. Where, however, the punned banks were breached, the water first rose above their tops, and in flowing over them and falling to the down stream level, cut through them.

After a bank was constructed, the heavy and continuous traffic passing over it so consolidated its top that it acted as a roof, and passed off the rain-water to the side slopes. The rain-water was thus prevented from entering the body of the bank and contributing to its shrinkage.

From the experience gained in this work, it is the opinion of the writer that time does not so compact and solidify an old, unrammed bank as to make it equal for the purpose of impounding water to a rammed bank, even when the latter has been built more recently; and it is also his opinion that the top of a bank for impounding water should not be used as a roadway until after the lapse of sufficient time to allow it to take its ultimate shrinkage.

A few words here in explanation of metaled gaps. They are simple, inex-

pensive road-works, to pass off surface flood waters, and to save bridging.

It was found that the bridging on the Grand Trunk Road required to pass off the flood water, amounted to more than 8,000 feet in length. The great expense and the length of time required to complete this work prohibited bridging. To provide ample outlets for surface drainage, portions of the embankment varying in length, according to the requirements of the flood waters, from 100 to 1,700 feet at ground level, were excavated, and from ground level at each end, slopes of one in thirty were made to top of bank. The slopes and the portion on level of country were metaled to a width of eighteen feet, and on each side of the metal, a brick wall, two feet deep and one foot wide, was built, the top of the walls being on a level with the metaling. A row of guide-posts 100 feet apart were fixed on each side of the metaling to guide travelers and vehicles to keep on the roadway during floods. During floods, which continue for only a few days in the year, these gaps passed off the surface flood water at a low depth—less than two feet—and at such a low velocity as to permit traffic to be carried on in safety through the water, though at great trouble and inconvenience.

During floods the gaps were fords, and at other times they made a good metaled road.

After the flood subsided, the ordinary drainage of the country was passed off by the well-defined and deep channels which had bridges or culverts built over them.

This cheap expedient of gaps for passing off surface flood-water permitted the opening of the road, during the rainy season, several years before it would otherwise have been done had bridges been built at the time, instead of gaps, for carrying off all the surface flood-water.

I will now show that under certain forms of longitudinal section, and one very likely to occur in broken ground, the use of certain formulæ in the computation is sure to cause a serious error in the result. Gillespie says, in his "Roads and Railroads":

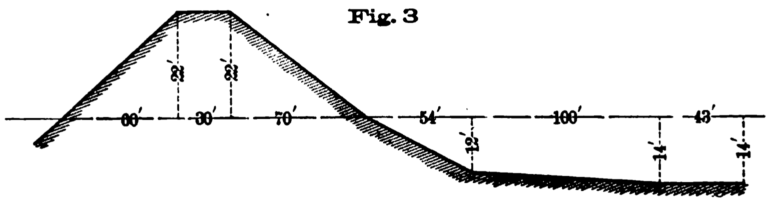
"Average end areas is the most usual method in this country" for computing earthworks.

Morris, in his "Earthworks," says, in reference to same formula:

"This rule has been by far the most used of any in our country. With tables of cubic yards it is very expeditious, and has found numerous advocates amongst engineers on account of its simplicity and convenience.'

Searle states that this formula is approved by statute to be used in the public works of the State of New York.

Fig. 3



LONGITUDINAL SECTION.

The diagram represents the longitudinal section of part of a line of railroad, the ground at all points transversely to the center line being level, so that all cross-sections will be on level ground.

The cut is 18 feet wide at base, with side slopes 1 to 1, and the fill 14 feet on top, with side slopes 1½ to 1.

In order to make an experiment on shrinkage, the engineer takes out the cut and builds the bank to the sections required. The end of the bank is assumed to stand vertically at A. The quantity in cut completes the embankment for 197.06 feet in length, as shown in diagram. After final measurement, the engineer computes his quantities by the formula of average end areas. He finds the volume of cut=3,096 cubic yards, and the fill=2,784 cubic yards. The quantity in cut appears, therefore, to exceed the fill by 312 cubic yards, showing a shrinkage of 10 per cent. In fact, however, there is no shrinkage. If the computations are made by the *prismoidal formula*, the volume of the cut

will be found to be equal to that of the fill, 2,708 cubic yards, so that the apparent shrinkage of ten per cent. was entirely due to the use of an incorrect formula.

If, again, the computation be made by the formula of *average end heights*, in which a section is taken with a height equal to the average of the two end heights, the volume of the cut will be =2,514 cubic yards and the fill 2,670 cubic yards, showing an increase of 156 cubic yards=6 per cent., which increase does not exist, as the cut and fill exactly balance, as before explained.

The use of the latter formula of mid-sections gives a deficiency equal in amount to half the excess found by the use of average end areas.

Another formula by *mean proportionals*, which gives less than the correct result, is sometimes used. The prismoidal formula is the only one of the four given which is sure to give correct results.

The error will be even more than shown above, when the ground at cut slopes also across the center line of road.

The error is greatest when one of the end areas=O, as seen in the column of cut given below. In similar cross-sections, in ground where the end heights do not materially differ, the error is small, as will be seen in column of fill given below. Error in the results, however, can be avoided only, in so far as the computations are concerned, by the use of the prismoidal formula.

The following table gives the volume of cut and fill by the three formulæ used above:

	Cut Cubic Yards.	Fill Cubic Yards.
Prismoidal formula......	2,708	2,708
Average end area........	3,096	2,784
Average end heights.....	2,514	2,670

There is a tendency among workmen to leave the side slopes curved in cross-section. For instance, embankments are sometimes finished with the side slopes concave as shown in Fig. 4, and cuttings, especially in hard ground, are left with the side slopes convex, as shown in Fig. 5.

Fig. 4

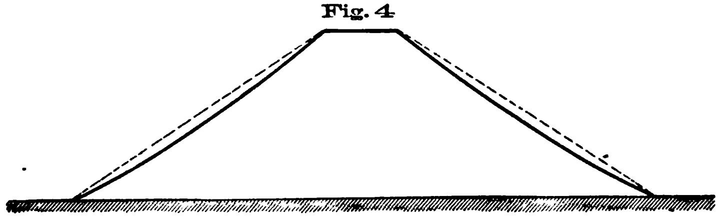
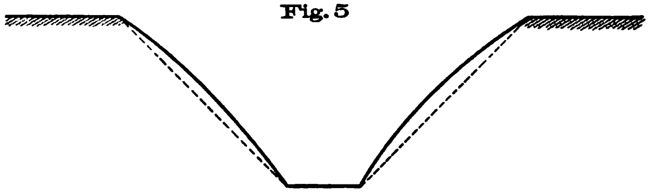


Fig. 5



It is therefore advisable, in cross-sectioning the work for final measurement, to take levels and measurements at frequent intervals, and before doing this work the adjustment of the level and the length of measuring instruments, chains, tape, etc., should be looked to.

In descriptions of *shrinkage* experiments on earthwork, the following information would be useful, and, in addition, such other information as might be deemed of use in arriving at a correct result:

1. The description of material and locality.

2. Method of construction, such as by scrapers, wheelbarrows, tipping from wagons, etc.

3. Longitudinal section, and also cross-sections of cutting and embankment.

4. Average depth of layers, and if rammed.

5. Foundation of bank, that is, description of ground on which the embankment is constructed.

6. Dates of commencement and completion of bank and of measurement.

7. Rainfall, if any, and a general description of weather during period of work.

8. The formula used in computing the quantities.

9. If base of bank has been flooded, the depth of flooding, period during which flood lasted, and state of bank at the time, should be given.

ELECTRICITY IN WARFARE.

Lecture delivered before the Franklin Institute, November 18, 1885, by Lieut. B. A. FISKE, U. S. Navy.

The first practical plan for using electricity in warfare was devised and executed by Col. Samuel Colt, the inventor of the revolver, who, in 1841, wrote to President Tyler, stating that he had invented an apparatus by means of which he could destroy in a moment the most powerful man-of-war, even though miles away, and asking that the Government give him opportunity for demonstrating the truth of his statement.

The truth of his statement he demonstrated a year later by exploding a "torpedo" under water, in New York harbor, by means of electricity. Following up this success, he totally destroyed, by similar means, an old gunboat named Boxer, and six weeks later, in sight of the President, Gen. Scott, and others, he destroyed a schooner in the Potomac, distant five miles. Congress at once appropriated $17,000 to continue his experiments and perfect his apparatus; and in two months from this time he blew up the big Volta in New York in the presence of forty thousand spectators. All of these vessels were at anchor; but in the spring of 1843 Col. Colt demonstrated the further value of his invention by blowing up a vessel while under way in the Potomac, he being at Alexandria, five miles distant.

These performances seemed, in that day little short of magic, and their practical value was evident to many, yet they stopped short at this point and were never resumed. Col. Colt endeavored to get further assistance from the Government, but without success. From a variety of reasons, the authorities regarded his plans with disfavor, and Colt, disheartened and distressed, was compelled to abandon his invention just at the time when its success was assured but its details unperfected.

But the Russians caught his ideas, and in the Crimean War we find the harbor of Sebastopol defended by torpedoes, many of which were operated by electricity. A few years later, in our Civil War, the electrical torpedo was extensively used by the Confederates in the protection of their harbors. In the Franco-Prussian War, torpedoes were so successfully employed in the defence of harbors that hostile ships did not even attempt to enter them; and in the Turco-Russian War, the mere suspicion that a harbor was defended by torpedoes was enough, in many instances, to keep a hostile fleet at a surprising distance—for the hostile fleet could not know just how far beyond a harbor the torpedo defence extended.

Since the experiments of Col. Colt, the electrical torpedo has gradually improved, keeping pace with the progress of the sciences of electricity and engineering. Yet the original plan is the basis of the most elaborate and perfect instrument, and to the brain of Col. Colt we owe the surest defence we have for the cities and harbors of our extensive coast.

At the present day an electrical torpedo consists of a strong, watertight vessel of iron or steel, which contains a large amount of explosive, usually gun-cotton, and a device for detonating this explosive by electricity; and the most important form is that form which is anchored in a channel, and which is made

to explode when an enemy's ship passes near it. These torpedoes are usually called "submarine mines."

In our war, most of these torpedoes were not operated by electricity, but by mechanical means, which consisted, for the most part, of levers protruding outside the case, and so connected with the explosive inside, that when a lever was struck by a passing vessel, a hammer inside was caused to fall upon a cap, just as when the trigger of a musket is pulled, a hammer is caused to fall upon a cap. These mechanical torpedoes were used with considerable success by the Confederates, and they caused tremendous damage to the Union ships; but they had a number of defects, on account of which they have been almost entirely supplanted by the more complete, though much less simple, electrical torpedo.

One of the defects of mechanical torpedoes was the danger attending the operation of planting them, another was the danger attending the operation of raising them, another was the fact that it was impossible to tell whether they remained in order, save by the expensive and suicidal plan of seeing if they would explode; and still another defect was, that a mechanical mine did not know a friendly ship from a hostile one, but would sink either with absolute impartiality.

Now, it is clear that an electrical torpedo is free from all these defects, because it cannot be exploded unless a current of electricity be sent through it from a battery on shore or on board ship, so that the only thing to do when it is desired to make it harmless is simply to disconnect the battery; and when it is desired to put the mine into operation again, the only thing to do is simply to reconnect the battery; moreover, the condition of the mine can at any time be determined by sending a very minute current through it, even though the torpedo be miles away, and buried in water many fathoms deep.

In order to understand how electricity can explode a torpedo, it is sufficient to know that when a current of electricity goes through a wire it heats it to some extent, and if the wire be small enough, the heat produced is sufficient to make the wire white hot. Clearly, if powder or fulminate of mercury be in contact with a white-hot wire, the powder or fulminate will be ignited, and if this powder or fulminate be enclosed in a tight vessel, together with powder or gun-cotton, a tremendous explosion will ensue. Therefore, in order to explode a torpedo, the only thing necessary is to send a current of electricity through a very small wire, preferably of platinum, which is within the torpedo and in contact with the explosive.

That part of a torpedo which includes this small wire and its connections is called the fuse. In practice, the fuse is a separate thing, and is screwed into the torpedo case, the wire from the battery coming in through the fuse through a water-tight gland, so that, after the fuse has been screwed in, the torpedo, as a whole, is water-tight, and may be left under water for months without injury. If the explosive charge is wet gun-cotton, as is now usually the case, the fuse contains fulminate of mercury, and is surrounded by a small charge of dry gun-cotton. The heat of the fuse-wire explodes the fulminate, the explosion of the fulminate causes the instant explosion of the dry gun-cotton, and this, in turn, causes the instant explosion of the whole charge of wet gun-cotton in the torpedo.

But it is evidently not sufficient to be able to explode a torpedo; we must also know when to explode it, that is, we must know when the hostile ship is in such a position that she will be destroyed if the torpedo is exploded. Clearly, we can know this if we have a chart showing the exact position of each torpedo, and have also an arrangement by which we can know at any instant the bearing of a ship coming up the channel from two stations properly situated. If the ship comes into such a position that she is in the line of one of the torpedoes as seen from each station, she is clearly on the intersection of those lines, and is therefore directly over that torpedo, and the only thing necessary is to touch the electric key controlling that torpedo, thus sending an electric current through its fuse and causing its instant explosion. Another way is to have the telescopes at the two observing stations so fitted that when both point at any torpedo the current is automatically sent through that torpedo. Therefore, if the observer at

each station keeps his telescope bearing on an advancing ship, electricity will do the rest, and the ship will sink when she comes within the radius of destructive effect of any one of the torpedoes.

But what can be done at night, or in a dense fog? Evidently these systems will not answer then, because a ship cannot then be seen; and for this reason torpedoes now are frequently made absolutely automatic, so that whether the time be day or night, clear or foggy, the mere fact of a ship's striking a torpedo is sufficient to insure her instant destruction. This result is secured by the use of what is known to electricians as an automatic circuit closer, that is, a device which, when subjected to certain conditions, automatically bridges over the distance between two points which were separated, thus allowing the current to pass between them. In submarine torpedoes it is usual to employ a small weight, which, when the torpedo is struck, is thrown by the force of the blow across two contact points, one of which points is in connection with the fuse, the other in connection with the battery, so that the current immediately runs over the bridge thus opened and through the fuse. In practice, these two contact points are connected by a wire, even when the torpedo is not in the state of being struck; but this wire is of such great resistance that the current is too weak to heat the wire in the fuse. Yet, when the weight above mentioned is thrown across the two contact points, the current runs across this bridge instead of through the resistance wire, and is then strong enough to heat the wire in the fuse and explode the torpedo. Now, the advantage of having a wire of high resistance between the contact points, instead of having no wire between them, is that the current which then passes through the fuse, though too weak to fire it, shows by its very existence to the men on shore that the circuit through the torpedo is all right.

But instead of having the increased current, caused by striking the torpedo, to fire the torpedo directly, a better way is to have it simply make a signal on shore, and at the same time throw in a firing battery. Then, when friendly vessels are to pass, the firing battery can be disconnected, and when the friendly ship bumps the torpedo the working of the signal shows not only that the circuit through the fuse is all right, but also that the circuit-closer is all right, so that, had the friendly ship been a hostile ship, she would certainly have been destroyed.

The action of a torpedo placed in a harbor and connected by a submarine cable to an electric battery on shore, is thus shown to be quite a simple thing. But it should not be gathered from this that the protection of a harbor is a simple thing, or one that could be accomplished in a few days on the outbreak of war. It should be remembered that each torpedo contains from 100 to 1,000 pounds of gun-cotton, that hundreds of torpedoes will have to be used, that the amount of gun-cotton required will be enormous, and that it will be wanted in a hurry, but that the operation of making gun-cotton cannot be hurried; it must be remembered that to make the torpedo cases required will consume much time, that the making of the fuses is a matter requiring the greatest possible care and calculation, that the operation of properly laying down these heavy, and yet delicate, torpedoes in deep water, even in the best possible weather, is an operation requiring great nautical and engineering skill and practice. Yet, even after the best system of submarine mines has been laid down, it will be of no use, unless an enemy's boats can be kept from countermining, and unless the operating rooms be furnished with the best instruments and thoroughly protected against capture, by fortifications, guns and ships. It is no uncommon thing to hear men of intelligence airily say, "Oh, in time of war we can defend our harbors by torpedoes." I do not think I am exaggerating when I say it would take a year to put the torpedo defences of our harbors into proper shape, even if Congress were to appropriate the money to-morrow. And, after that, a corps of men would have to be formed and thoroughly trained in laying down and taking up torpedoes and cables, making splices in submarine cables, testing circuits, managing electric batteries, etc., etc.

In order to detect the presence of torpedoes in an enemy's harbor, an instrument has been invented by Captain McEvoy, called the "torpedo detector," in which the action is somewhat

similar to that of the induction balance, the iron of a torpedo case having the effect of increasing the number of lines of force embraced by one of two opposing coils, so that the current induced in it overpowers that induced in the other, and a distinct sound is heard in a telephone receiver in circuit with them. As yet, this instrument has met with little practical success, but, its principle being correct, we can say with considerable confidence that the reason of its non-success probably is that the coils and current used are both too small.

Besides these stationary torpedoes or mines anchored in harbors, there is another class, called spar torpedoes, which are carried on spars protruding from ships or boats, and which consist, for the most part, of cases of steel holding about 30 pounds of gun-cotton, and fitted with electric fuses similar to those for exploding submarine mines. The torpedo on the end of the spar is connected by insulated wires with an electric battery in the boat or ship, and it is designed to be shoved under a hostile ship by a determined effort and exploded there. The current is sent through the fuse by pressing an electric key at the proper moment, or a simple automatic circuit-closer may be put inside the torpedo, so that when the torpedo strikes the ship a break in the circuit is automatically bridged over in the torpedo, thus allowing the current to heat the platinum wire in the fuse and explode the torpedo.

Still another class of torpedoes are movable torpedoes. These contain not only explosive and means for exploding it, but also machinery for moving the whole through the water, and for steering it. A movable torpedo is, therefore, really a torpedo boat and torpedo combined. In one class of movable torpedoes, such as the Lay torpedo, the motive power is usually carbonic acid gas or compressed air, the steering and firing being done by electricity. One wire usually suffices, a simple step-by-step device accomplishing the operation of sending the current through different circuits in the boat, the current through one circuit causing the throttle of the engine to open, the current through another causing it to close, another putting the helm to starboard, another putting the helm to port, another firing the torpedo, &c. Therefore the operator on shore or on board ship can, by moving his switch in the proper manner, cause the torpedo boat to go ahead, stop, turn to the right or left in such a way as to go directly toward the object of attack and then explode in contact with her bottom.

In another class of movable torpedoes the electric current not only steers the boat, but also propels it, there being an electric motor—or engine—inside, which, when supplied with the requisite current from an electric machine on shore or on shipboard, revolves rapidly, thereby causing the propeller of the boat to revolve, in the same way that a steam engine causes the propeller of a steamboat to revolve. The current for steering may either be sent along a separate wire, which may be inside the other one, or a device may be used by which one wire will suffice for everything.

Movable torpedoes being usually cigar-shaped, so that their section is circular, or nearly so, the electro-motor for an electric movable torpedo which will most nearly fit the space intended for it—in other words, the shape which permits the largest size for a given space—is clearly one in which the distance from the center of the armature to the outside of the magnets is constant, so that the field magnets form nearly a circle embracing the armature. The exterior form of the motor called the Griscom motor evidently seems to fulfill this condition well. The most successful movable electric torpedo thus far tried is the Sims torpedo, with which experiments have been conducted under Gen. Abbot's superintendence for some time, and with very good results. Now, as all men-of-war and all forts are to be supplied with means for generating electricity on a large scale, it is clear that an electric movable torpedo can be easily adapted to naval and military requirements, the mechanism of the torpedo being exceedingly simple, and the only thing to be done to put it into operation being to connect it to a suitable electric machine, and introduce a keyboard, by which the direction and strength of the current can be controlled. The torpedo will then be ready to move at any instant, and will have a supply of power practically inexhaustible.

Recent experiments in England have

shown that the Whitehead torpedo—over which control ceases after it is fired—is not so formidable a weapon when fired at a ship under way as many supposed, for the simple reason that it can be dodged. Now, an electrical torpedo, over which control is exercised while it is in motion through the water, cannot be dodged, provided it be given sufficient speed. For effective work against ships capable of steaming fifteen knots per hour, the torpedo should have a speed of twenty knots. Now, there is no theoretical difficulty in the way of doing this, for a speed of 11 knots has already been recorded, though an electric torpedo, to get this speed, would have to be larger than a Whitehead having the same speed; and it may be conceived that a torpedo carrying, say, 50 lbs. of gun-cotton, capable of going 20 knots per hour—so that it would pass over a distance of 500 yards in about 45 seconds, and yet be absolutely under control all the time, so that it can be constantly kept pointed at its target—would be a very unpleasant thing for an enemy to meet.

Our civil war introduced another use of electricity into warfare, and gave birth to the art of military telegraphy. At first, the telegraph was used only in communicating along the regular telegraph routes, but eventually a corps of military telegraphers was formed, and instant communication became possible between detachments in the field. The advantages of the telegraph in conveying information and orders with dispatch was found to be so great that foreign nations took the hint, and to-day a telegraph train is essential to all armies. No well-equipped force in the world is without means for rapidly connecting by the telegraph different headquarters with each other, and with the different parts of an army; and in the Franco-Prussian war it was the telegraph, combined with the railroad, which made possible that wonderful speed and certainty of mobilization and manœuvre that caused the swift destruction of the armies of France.

In military telegraph trains, miles of wire are carried on reels in specially constructed wagons, which carry also batteries and instruments, some of the wire being insulated, so that it can rest on the ground, and thus be laid out with great speed; while other wire is bare, and is intended to be put on poles, trees, etc., For mountain service, the wires and implements are carried by pack animals. Regularly trained men are employed, and they are drilled in quickly running lines, setting up temporary stations, etc. In the recent English operations in Egypt the advance guard always kept in telegraphic communication with headquarters, and with England; and after the battle of Tel-el-Kebir, news of the victory was telegraphed to the Queen and her answer received in forty-five minutes.

The telephone, also, has been used in military operations, and with great success; having an evident superiority over the telegraph for some purposes. One use of the telephone, in fact, is to assist the telegraph in cases where by reason of the haste with which a line has been run, the current leaks off, so that the receiving instrument will not work. The only thing necessary is to use a telephone to receive the message, and to use as a transmitter a simple buzzer, or automatic circuit breaker, controlled by an ordinary key. In many cases, where the ordinary receiving telegraph instrument refuses to work, the more delicate telephone thus used will work very satisfactorily.

In endeavoring to use the telegraph and telephone for strictly naval purposes between ships, a number of obstacles have been met which have not yet been overcome, though many ingenious devices have been attempted. It might seem at first sight as if, in the case of ships at anchor, telegraphic and telephonic communication among the ships and between the ships and shore would be an easy thing to establish. It is easy, and it has been established in the case of ships moored so that they cannot swing; but when a ship swings by a long chain, most of which is embedded in the mud, it becomes very difficult to devise an automatic means to keep the wire from being fouled and broken.

In England, in the case of lightships, the difficulty has been surmounted, or rather avoided, by making the cable by which she rides hollow, and running an insulated wire along the long tube thus formed inside. This plan has been found thus far to work very well, even in the worst sort of weather.

But the problem is much simplified

when temporary communication only is desired between ships at anchor, between a ship and the shore, or between a ship and a boat which has been sent off on some special service, such as reconnoitering, sounding, etc. In this case, portable telephones are used, in which the wire is so placed on a reel in circuit with the telephone, that communication is preserved, even while the wire is running off the reel.

The telephone has been used to some extent on shipboard for communicating between different parts of the same ship, giving orders to the magazines, battery, etc. The vibration of a ship is seldom so excessive as to impair the working of the microphone transmitter; and, in cases where it is excessive, the original magneto-transmitter has been found to work perfectly, getting rid entirely of trouble due to vibration, and at the cost of only a little of the loudness of tone as heard in the receiver.

The telegraph and telephone are both coming largely into use in artillery experiments and drills, as in tracking a vessel as she comes up a channel, so that her exact position at each instant is known, in determining the spot of fall of a projectile, etc. In these cases, two observers, at the ends of a base line electrically connected, know at each instant the bearing of the vessel or the projectile from both ends of the base line, and the intersection of these lines of bearing is, of course, the position of the vessel or the projectile at that instant. In the case of a vessel, this gives the range for the guns, and in the case of a projectile, it shows by how much the projectile missed the target. In getting the time of flight, also, of projectiles, electricity is of value, since it enables the observers to get the exact time of start, a thing impossible to get with stop watches; but by making the projectile break a wire in circuit with a chronograph, the precise instant of start, to a thousandth of a second at least, is automatically registered.

An application of electricity, somewhat analogous to this, has been in use for years in what are broadly called velocimeters, that is, instruments for ascertaining the velocity of projectiles. There are scores of patterns, but in all wires are cut by the projectile at different points of its flight, and the breaking of each wire causes the breaking of the electric current going through it, and this causes the appearance of marks on a surface moving along at a known speed. Now, knowing the rate at which this surface moves, and observing the distance on the surface between the marks caused by the breaking of the first wire and the marks caused by the breaking of the second wire, we can compute the interval of time which elapsed between breaking of the two wires; and now, knowing this interval of time, and knowing also the distance between the two wires, we can calculate at once the velocity of the projectile in going from one wire to the other.

Within the past few years, electricity has come to be employed to a great and increasing extent in firing great guns, both on ships and in forts.

In the case of ships the advantage of electrical firing lies in the fact that a gun can be fired more quickly by electricity than by other means. The ordinary means of firing are, as is well known, percussion and friction, and in each of these systems the moving parts have to be of such strength that considerable force must be exerted by the arm, in order to operate them. Moreover, for prudential reasons, the line leading to them from the hand must be kept very slack, lest a lurch of the ship should cause such a movement of the man's body as to bring a strain on the line sufficient to cause the accidental firing of the gun. Therefore, when the gun captain finds the sights on the gun bear on the target he must then move his arm through a considerable distance, and exert considerable force, before the gun will be fired. It may seem as if the time were insignificant, but when it is considered how rapidly a ship rolls, and how swiftly a modern ship moves, and when it is remembered how small an error in elevation suffices to make a projectile fall short of, or go over, a ship which subtends a very small angle in a vertical plane, it will be seen that a fire which comes instantaneously with the coincidence of sights and target, is better than one which does not. Now, in firing by electricity, the gun captain holds a sort of push-button in his hand, which he simply presses, when the sights bear on the target; and practical experiments abroad have shown that

greater accuracy is attainable by this method than is attainable by the old methods.

But not only are modern vessels of war fitted with means for electrically firing individual guns, they are also fitted with means for firing any desired number together, the whole broadside even; for the effect upon an enemy of a blow from a whole broadside at once, is very much greater than the effect of the same number of guns fired at different times. The simplest way of doing this is to lead the electrical wires from all the guns to an armored fighting tower, where a cool officer, under the personal direction of the commander, with an electrical keyboard and a good system of sights, can deliver the whole fire of the broadside at once upon an enemy. This officer being clear of the smoke and away from the excitement and bustle of the battery, and with a view of the whole horizon, can clearly handle the battery with its maximum effectiveness; and, moreover, as soon as the guns have been laid at the range and in the direction at which it is intended to fire them, the men can lie down and be protected in a measure from the murderous fire of machine guns. Suppose that two modern ships are about to engage in a naval duel on the high seas, and that they are advancing towards each other at a speed of 14 knots an hour each, so that they are coming together at a rate of about 46 feet per second. The ships will, of course, begin to play on each other with their machine guns as soon as they get within range, so that as soon as the great guns are ready, the men had best be made to lie down on the deck, in rear of their guns. Now, the battery of each ship will probably be composed of guns firing projectiles of from 100 to 1,000 lbs., and each commander, knowing how much damage can be done by one well-delivered shot, knowing how long it takes to reload a modern gun, and appreciating the great speed at which the ships are approaching, will probably reserve his fire until sure that he has a good chance of making every shot tell. He will hardly fire at 2,000 yards range, knowing that his chance of hitting the rapidly moving target is small, and knowing that it will be only about one minute before the enemy will be at 1,000 yards range. Now, at 1,000 yards range, the machine guns of both ships will be working vigorously, and each ship will have an excellent target, but a target improving so rapidly that a commander would not wish to throw away all the shot of his great guns until a closer range is reached. One gun, for instance, might be fired now, having been previously laid for this range, of course; but probably it would be best to reserve nearly the whole broadside to be delivered simultaneously as the ships rush by each other, at pretty close range. With the heavy guns and carriages of the present day, it would be impracticable to keep a gun pointing at a ship under these circumstances, since both range and bearing are changing so rapidly. Therefore, the guns must be pointed in certain directions and laid for certain ranges, and fired when the enemy reaches the spots thus defined.

In the case of a fort, the value of firing by electricity is evident, since it can be employed in connection with the instruments used for determining at each instant the position of an approaching vessel or army. Different guns being laid at different ranges and in different directions, an officer in the operating rooms can keep track of the exact position of the enemy, and when the enemy reaches one of the predetermined points, all he has to do is to touch an electric key and fire the gun or guns controlled by that key, knowing that those guns are not only pointed in the proper direction, but also are elevated for the correct range. It would seem difficult to improve upon the accuracy obtainable by this method.

Whitehead torpedoes are now so arranged that they can be ejected by pressing an electric button; and the firing keyboard in the fighting tower of a ship indicates at any instant what guns and torpedoes are ready for firing. The fighting tower should also be fitted with engine and steering telegraphs, and indicators for showing whether the commander's orders to the engine and steering apparatus have been understood and executed, so that the commander can tell at a glance the exact state of affairs at battery, engine, and steering apparatus, and can have all the separate departments of his ship under absolute personal control.

Within the past three years, the supe-

riority of the incandescent electric light over all other lights for ship use has become more and more evident, and nearly all first-class merchant steamships launched, and all men-of-war, are fitted with electric lights. In men-of-war, this light has even more advantages than in merchant ships, by reason of its safety in illuminating magazines, shell rooms, torpedo storerooms, etc., and the ease and speed with which it can be used in signaling at night.

For some time the electric light was declared unsuitable for naval use, the reason being urged that if the wire carrying the current should be shot away in action, the whole ship would be plunged into darkness, and that the same thing would happen if any accident should befal the dynamo generating the current. The answer to this very sensible criticism is that—1st: different circuits must be arranged for different parts of the ship, so that an accident in any part of the ship will affect that part only; 2d: that the wires carrying the current must be arranged in duplicate, so that even in case one wire is shot away, the lights will burn as brightly as ever, since the other wire remains, and is of sufficient size to carry all the current; 3d: it is very easy to repair a break in a copper wire, even if it be shot away, the only thing necessary being to bridge over the break by a wire a few inches long, whose ends can be clamped, spliced, or even twisted on to the broken conductor on each side of the break. As to the danger to be feared from accident to the dynamo and engine, this can be guarded against in two ways—1st: by placing the dynamos and engines in a place as well protected as that occupied by the main boilers and engines of the ship, preferably near the engines, below the water line, and under a protective deck, and this place should be provided in laying down the original plans of the ship; 2d: by not placing dependence on one dynamo and engine alone, but by dividing the total work among three or four, so that in case of accident to one, the ship will have the benefit of all the others. To this end, all the dynamos should, of course, be of the same electromotive force, and feed into the same mains, from which all lamps draw their supply, and which are fed by feeders from the dynamos at different points, so that accident to the mains in one part of the ship will affect that part only.

But it is the arc light, used as what is called a search light, that is most valuable in warfare; a search light being simply a very powerful arc light, so arranged with a catadioptric mirror, that it throws out a very concentrated beam of light, and so mounted that this light can be cast in any desired direction, and made to illuminate any locality which it is desired to inspect. I believe the first use made of search lights was in the siege of Paris in the Franco-Prussian war, when the French employed them to discover the operations of the besieging army at night. Their value, from a military point of view, was so evident that other nations took the hint and introduced them into their military and naval services. Since then, their use has been extending, more probably for ship use than for use on shore, though they are also employed considerably in forts guarding the approach to harbors, to watch for the approach of hostile fleets, torpedo boats, or countermining expeditions at night.

For the use of an army in the field, search lights are mounted on suitable wagons, together with portable boilers and engines, so that they can be taken on the march, moved to any part of the field, the top of a hill for instance, and employed at night whenever desired. It is clear that a search light might often be very useful in doing such work as searching for wounded after a battle, examining the nature of the ground in a comparatively unknown country, watching the enemy when a surprise might be anticipated, guarding against a sortie from a beleaguered fort or city, etc., on many occasions, in fact, when the darkness of the night might be a hindrance to the operations of an army or an element of insecurity.

On board men-of-war, the principal use of the search light has been in watching for the attack of torpedo boats; but the experiments in Bantry Bay last July showed that it was often useful in detecting the approach of hostile ships, in aiding the sighting of guns at night by bringing the target out clearly, and in embarrassing the operations of a torpedo boat or ship in trying to force a passage up a channel, by dazzling the eyes of the

enemy one instant and leaving them in total darkness the next.

It was related that in Africa some three years ago an attack on a fort was frustrated by a single search light. The barbarians advanced to the assault brave and determined, under cover of darkness, but were suddenly terrified by finding themselves enveloped in an awful and miraculous light which turned night into day. Halting for an instant, they covered their eyes to shield them from its dazzling whiteness, then suddenly turned and fled in panic.

But even had they not been barbarians, even had they been trained soldiers of a highly civilized people, how bewildered they would have been to find themselves suddenly in such a brilliant light that their eyes were blinded so that they could not see to pick their way; and then, in a few seconds, just as the pupils of their eyes had contracted under the influence of the light, to find themselves suddenly in total darkness!

Such alternations of dazzling light and black darkness would render the march of an attacking force over a broken country toward a fort a very difficult undertaking, for it should not be forgotten how dependent we are upon our eyes, for our most simple actions, and how much our eyes assist us, even in the dark.

It may be pointed out here, that the search light, like all other military instruments, must be used with judgment. It should not be used, for instance, when it is important to keep your position secret from the enemy, unless the advantages of observing him outweigh the disadvantage of disclosing your own position, or unless you can immediately extinguish your light, and change your position. In order to use the search light effectively on board ship, there should be four on each side of ship.

Search lamps are of much higher power than any other lamps that have ever been made, and the effect of the white cylinder of light, as it touches up the different points of the land or sea over which it is made to rapidly pass, is striking and beautiful in the extreme. The people of Philadelphia may remember the naval search light with which I used to illuminate the city last autumn, and do not have to be assured of its power and beauty. The best idea that I got of its illuminating effect was got one night when I went to the tower of the Pennsylvania Railroad depot, and watched the light stationed at the Electrical Exhibition building on Thirty-second street. The ray of light, when turned at right angles to my direction, looked like a silver arrow going through the sky, and when it was turned on me, I could read the fine print of a railroad time-table at arms' length.

For signaling at night, incandescent lights and search lights have both been used, the search lights being employed to reach long distances, or to reach points hidden by hills or other intervening objects. In using the search light in this case, the beam is thrown into the sky, and signals are made by showing long and short flashes, or by interrupting the beam in accordance with any preconcerted code. Flashes from the search light which I had at the Electrical Exhibition last autumn were seen from a distance of thirty miles.

In using incandescent lamps for night signaling, the simplest way is to arrange a keyboard with keys marked with certain numbers, indicating the numbers of lamps arranged in a prominent position, which will burn while that key is being pressed. If it be desired to signal the number 5348, for instance, meaning, let us suppose, "Prepare to receive a torpedo attack," it is only necessary to press in succession keys marked 5, 3, 4, and 8, and the light of 5, 3, 4, and 8 lamps will successively blaze out and expire. Other codes, of course, can be used; the Morse code, for instance, in which a dash can be denoted by two lamps and a dot by one. One lamp could, of course, be used, the dot being denoted by a short flash and a dash by a long flash; but it is found best, practically, not to use time intervals in optical telegraphy.

Electric lights have been used considerably of late in photographing the bores of great guns. Views can be thus obtained of any part of the bore, showing whether the gun has been accurately bored and rifled, and showing how the metal is standing the erosion of the powder gases.

One of the needs arising in modern warfare is means for handling heavy ordnance with speed and precision, and for bringing up ammunition. Guns, car-

riages, and ammunition have increased so much in weight, that it has become more difficult to handle them quickly than it used to be, and yet the speed of ships has so increased that there is more necessity for handling them quickly. The electric motor will certainly be used for handling ordnance on board ships not very heavily plated with armor, for it is clear that a small wire is a much more convenient way of conveying energy to a motor of any kind, and is much less liable to injury in action than a comparatively large pipe for conveying steam, compressed air, or water under pressure; and, besides, the electric motor is the ideal engine for work on board ship, by reason of its smooth and silent motion, its freedom from dirt and grease, the readiness with which it can be started, stopped and reversed, and its high efficiency.

In forts, Col. Hamilton, U. S. A., says that heavy guns must be worked by dynamo-electric machinery, since some other power than manual power must be employed, and since electricity is better than steam, compressed air, or water under pressure, because it can be conveyed from a central source by a single wire. Now, in forts, it is clear that the dynamos for search lights and incandescent lights, and for generating power for the electric motors for handling the guns, could be placed in a well-protected spot, and the wires leading therefrom could pass to the guns through underground pipes, where they would be well protected from projectiles, and the guns could be pointed in any desired direction with speed, silence and precision, by the simple turning of a lever or the moving of a switch.

It is probable that, in the near future, every man-of-war and every fort will be fitted with a complete "electrical system," well protected from projectiles, which will include dynamos capable of supplying a very large amount of electrical energy to a system of mains, from which all the incandescent lights, all the search lights, and all the motors of different sorts can draw the supply of energy requisite for their needs.

Electrical launches have been tried abroad to some extent, and with results which—though not completely satisfactory—give promise of success in the future. These boats are propelled in the same way as other boats—that is, by a propeller revolved by an engine—except that the engine is an electrical, instead of a steam engine. The electrical engine draws its power from what are called storage batteries carried in the boat. These storage batteries are first charged by a dynamo ashore or on board ship, and then are capable of rendering up to the motor the electrical energy stored in them, so that the motor is made to revolve, and thus cause the revolution of the propeller and the advance of the boat through the water. These electrical launches have carried hundreds of people, and have made a speed of about eight knots per hour, and the electric engine has worked perfectly well; but there has been considerable trouble with the storage batteries. Now, these storage batteries are improving every day, and as their defects do not seem irremediable, and as a great number of electricians are devoting study and experiment to them, we may hope that storage batteries will soon be efficient and durable. This done, the electrical launch will certainly replace the steam launch in warfare, by reason of the quickness with which an electrical launch can be got ready, as there are no fires to be lighted, and, above all, by reason of the noiselessness of the electro-motor, and of the fact that in an electrical launch no flame ever flares up above a smoke pipe. With steam torpedo-boats this flame frequently betrays them—that is, if the noise of the exhaust has not already done so. In using electrical launches in warfare, two sets of storage batteries will, of course, be necessary, so that one set can be replaced by another, and used while the first set is being recharged.

A novel application of electricity has recently been made in what have been named "electric sights." It is well known that it is difficult to get a sight at an object in the dark—first, because the object itself cannot be distinctly seen, and, second, because the front sight of the gun cannot be distincly seen. Now, Mr. Gaston Trouvé has recently invented an electric sight no larger than the ordinary front sight of a musket, which consists simply of a filament of fine wire in a glass tube, covered with metal on all sides save at the back. It looks much like an ordinary sight, except to a man

looking along the barrel towards the back of the lamp, but a man so looking sees a fine, incandescent wire. The battery is said to be no larger than a man's finger, and to be attached to the barrel near the muzzle by simple rubber bands, so arranged that the act of attaching the battery to the barrel automatically makes connection with the sight, and so arranged, also, that the liquid of the battery is out of action except when the musket is brought into a horizontal position for firing.

To throw a good light upon the target, the same inventor has devised a small electric lamp and projector, which is placed on the barrel near the muzzle by rubber bands, the battery being held at the belt of the marksman, with such connections that the act of pressing the butt of the musket against the shoulder completes the electric circuit, and causes a bright cylinder of light to fall on the target, thus bringing it out into strong relief, and enabling the marksman to get as good a shot as in the daytime.

An application of electricity coming somewhat into use abroad in Continental armies is in connection with ballooning, which has received there the benefit of considerable attention and experiment. It is well known to all here that balloons have been much used for observing the movements of the enemy from a distance, and now it is reported that not only have balloons been successfully fitted with telephones, for communicating at once to people on the ground the nature of the information gathered, but also with small search lights, by which the ground can be illumined for a considerable distance at night, and the enemy's manœuvres discovered. Incandescent lamps have also been sent up in small, translucent, captive balloons, and under small, ordinary captive balloons, and signals have been transmitted to a distance by making and breaking the current according to a preconcerted code.

Yet another way in which electricity will undoubtedly be used in any future war between civilized powers is in submarine boats, or boats which go beneath the surface of the water, to attack ships below the water line. Though it cannot be said that submarine boats have ever yet been successful, still, submarine boats are being improved, and men have always been found willing to risk their lives in them. Some have been devised of late years, in which the propelling power is electricity derived from storage batteries.

In submarine diving the telephone has been used with success in maintaining constant communication between the diver and his attendants above water; and the incandescent lamp, suitably protected, has also been lowered into the water to light him at his work. Submarine diving will certainly play a part in future wars, the diver descending to cut an enemy's torpedo cables, or to inspect or repair damages to submarine mines or to ships.

Progress is now beginning on what have been christened "electrical guns," in which the cartridge contains an electric fuze, which is ignited by pressing an electric push-button on the gun, instead of containing the ordinary percussion primer, which is struck by a hammer or bolt when the trigger is pulled. At present this invention has not reached the practical stage, and the necessity for a battery to fire the cartridge is decidedly an objection. Yet we should remember that the battery required is very small, that it needs very little care, and that it will last a long time. We should also note that an electric gun possesses the great advantage that a better aim can be got with it than with one fired by a trigger-hammer, for the reason that the hard pull of a trigger causes a movement of the barrel, except in the hands of the most highly-skilled marksman. Now, this hard pull of the trigger is a necessity, since the hammer or bolt must have considerable mass in order that it may be strong enough to strike the primer with sufficient force to explode it, and having considerable mass, it must have, necessarily, considerable inertia, so that it needs a deep notch in order to hold it firm at full cock when jarred, and this deep notch necessitates a strong pull on the trigger. But with an electric gun the circuit-closing parts can be very small and light, and can be put into a recess in the butt of the gun out of the way of chance blows, so that when it is desired to fire, a very light pressure of the finger is all that is needed, and yet from the small inertia of the parts, a sudden shock will not cause accidental closing of the circuit and firing of the gun.

We have now taken a running survey of the uses of electricity in war, and we find that they include nearly all the uses to which electricity can be put. Warfare now means more than the mere handling and provisioning of troops and ships; it means, in addition, the intelligent employment of scientific instruments. Particularly is this the case with naval warfare, for a modern ship, considered as a whole, is, in itself, the most elaborate, complicated, and powerful machine existing.

Science has made warfare a greater thing than scientists themselves foresaw, for it has put into the hands of our naval and military commanders weapons surpassing in power and length of reach the fabled weapons of mythology. Yet our wonder and admiration at what has been accomplished pale before our wonder and admiration at what will surely be accomplished. Progress is marching with rapid steps. Looking forward to the future with the light the past affords us, we see the promise of great things to come. Let us welcome each new triumph of the discoverer and inventor, and take advantage of every resource that science can suggest. War is a sad necessity, but it comes seldom to a nation known to be prepared. Let us thoroughly prepare ourselves. Then, if war does unfortunately come, we need not fear the issue.

THE FORTH BRIDGE.*

From "Iron."

By B. Baker, M. I. C. E.

The North British Railway Company for many years have striven hard to obtain a physical connection of their lines north and south of the Forth by means of a bridge. Twenty years ago they were authorized by Act of Parliament to build a bridge across the Forth at a point five miles above the site now under construction, but borings 120 feet in depth showed nothing but soft silt and mud, and the bridge, which was to have been two miles in length, inclusive of the four spans of 500 feet each, was luckily abandoned, as the difficulties with the foundations would have proved practically insuperable. In 1873 another Act was passed for a bridge across a narrower and deeper part of the Forth at Queensferry. At low water the width of the channel there is about 4,000 feet; and the island of Inchgarvie affording a foundation for a central pier, it was possible to cross the 200 feet deep portion of the sea-way by a couple of spans from 1,600 feet to 1,700 feet each in the clear. Sir Thomas Bouch prepared a design for this bridge on the suspension principle, with towers 665 feet in height from base to summit, and the contract for its construction was let to Mr. Arrol. Owing to the subsequent fall of the Tay Bridge, public confidence in Sir Thomas Bouch's design was shaken, and in session 1881 a Bill for the abandonment of the Forth Bridge was proceeded with. Whilst in committee, the different companies interested, namely, the North British, Great Northern, North-Eastern, and Midland Railway companies, ordered a final reference of the whole question to their respective consulting engineers, with the result that the abandonment bill was dropped, and the design for the cantilever or continuous girder bridge, prepared by Mr. Fowler and myself, in consultation with Mr. Harrison and Mr. Barlow, was substituted for the original suspension bridge. In 1882 the necessary Parliamentary powers were obtained, and in January, 1883, the works were commenced by Messrs. Tancred, Arrol & Co., the contractors. The total length of viaduct included in the contract sum of £1,600,000 is about $1\frac{1}{2}$ miles, and there are:

2	spans of	1,700	feet each.
2	"	675	"
15	"	168	"
5	"	25	"

Including piers there is thus one mile of main spans and half a mile of viaduct approach. The clear headway is 150 feet above high water, and the tops of the

* Read at the meeting of the British Association.

great cantilevers are more than 200 feet higher still. There will be about 45,000 tons of steel in the superstructure of the bridge, and 120,000 cubic yards of masonry in the piers.

PIERS.

The South Queensferry main pier consists of a group of four cylindrical piers of masonry and concrete, founded by means of pneumatic caissons on the strong boulder clay constituting the bed of the Forth at this point. Owing to the slope of the clay, the caissons required to be sunk to depths varying from about 70 feet to 90 feet below high water. The diameter ranges from 70 feet at the base to 60 feet at low-water lèvel, above which the iron skin of the caisson is replaced by a facing of Aberdeen granite. At the base of the caissons is a working chamber of 7 feet in height supplied with compressed air, and electrically lighted, for the men excavating the material. This chamber was kept clear of water by a pressure of air considerably less, as a rule, than that due to the head of water outside. For example, at 90 feet below high water, when the north-east caisson had been sunk through a considerable thickness of silt, the air pressure required to be maintained at 18 lbs. per square inch only, although at the reduced depth of 57 feet it was found convenient to work at 30 lbs. air pressure. Three shafts and air-locks were provided for each caisson, two for the excavated material, and one for the men. The former had two horizontal sliding doors actuated by small hydraulic rams, and the skip containing the clay and boulders was hoisted up by a 90-feet shaft by a steam-engine mounted on the side of the air-lock. As a rule, from 200 to 300 skips of excavated material were raised per day of 24 hours by a force of from 20 to 30 men. The maximum number of skip-loads was 363, and of men 33. The size of the skips was 3 feet diameter by 4 feet 3 inches high. Owing to the extreme hardness of the clay it was necessary to provide a certain number of spades having hydraulic rams in the handles, which, abutting against the roof of the working chamber, sliced the clay readily. At the present time, three of the South Queensferry caissons have been sunk successfully to the full depth, and the fourth and last would also have been completed but for an unfortunate accident which happened to it at the beginning of the year. By some means the caisson, which had been floated into position for some weeks, accidentally filled with water, and sank and slid forward on the mud. It is now being carefully cased in timber, to admit of the water being pumped out and the caisson floated again into position.

At Inchgarvie similar pneumatic caissons are used for two out of the four cylindrical piers, and the work on both is in full progress. Owing to the slope of the rock bottom, it is necessary to cut away as much as 18 feet in thickness of whinstone rock to form a level bench for the pier at 70 feet below high water, and the most convenient way of doing this was to convert the base of the pier practically into a great diving bell 70 feet in diameter. In this case, there being no silt over the rock, the pressure of air necessarily is that due to the depth of water outside, and somewhat sensational "blows" occur with a falling tide. Rock drills are provided, and blasting goes on in the compressed air chamber without necessitating the withdrawal of the men.

At North Queensferry, the four main piers were built either on dry land or within timber and clay coffer-dams. Above low water the whole of the main piers are built of Arbroath masonry in cement faced with Aberdeen granite, and hooped occasionally with 18 inches wrought-iron bands. The cantilever end piers, and the viaduct piers, are built of rubble, concrete, and granite in cement.

SUPERSTRUCTURE.

Although the piers of the Forth Bridge present many points of interest, it is the enormous span and novel design of the superstructure that has attracted the attention of the engineers of the world to the work now in progress at Queensferry. The chief desiderata in the biggest railway bridge ever proposed to be constructed are durability, strength, and rigidity under express trains and hurricane pressures; facility and security of erection, high quality of material and workmanship, and economy in first cost and maintenance. These, we considered, would be best met by a steel "cantilever" or "continuous-girder" bridge. Since the commencement of the Forth Bridge,

American engineers, ever bold and ready, have built three cantilever bridges of considerable span, and practical experience has confirmed our anticipations as to the advantages of the system; the Niagara Bridge, over 900 feet in length, which was manufactured and erected across the rapids in the short time of ten months, having stood all the tests of actual working in the most satisfactory manner. In the Forth Bridge, each span of 1,710 feet is made up of two cantilevers, projecting 680 feet, and a central girder connecting the same 350 feet in length. The cantilevers are 343 feet deep over the piers, and 40 feet at the ends. The bottom members consist of a pair of tubes tapering in diameter from 12 feet to 5 feet, and spaced 120 feet apart. center to center, at the piers, and 31 feet 6 inches apart at the ends. The top members consist of a pair of box lattice girders, tapering in depth from 12 feet to 5 feet. and spaced 33 feet apart at the piers, and 22 feet 3 inches at the ends. Each tube has a maximum gross sectional area of 830 square inches, and each girder a maximum net sectional area of 506 square inches. Upon each cylindrical masonry pier is bolted a bedplate carrying a "skewback," from which spring vertical and diagonal columns and struts. The former are 12 feet in diameter, and from 368 to 468 square inches sectional area; the latter are flattened tubes. Horizontal wind-bracing of lattice girders connect the tubes forming the bottom member of the cantilevers, and similar vertical wind-bracing connects the vertical and diagonal tubes, so that the whole structure is a network of bracing, capable of resisting stresses in any direction, and of any attainable severity.

The rolling load provided for is (1) trains of unlimited length on each line of rails weighing 1 ton per foot run (2) trains on each line made up of two engines and tenders, weighing in all 142 tons, at the head of a train of sixty short coal trucks of 15 tons each. The wind provided for is a pressure of 56 lbs. per square foot striking the whole, or any part of the bridge, at any angle with the horizon, the total amount of the main spans being estimated at no less than 7,900 tons. In practice only two trains, weighing 800 tons in all, would be on this length of bridge at the same time, so the wind pressure (if such a hurricane as 56 lbs. per square foot could ever occur) would be ten times as great as the train load. Under the combined stresses resulting from the test load in the worst position, and the heaviest hurricane, the maximum stress on the steel will not exceed 7½ tons per square inch on any portion of the structure, and on members subject to great variation in the intensity and character of stress, the maximum will not exceed 4 tons per square inch. For tubular columns and struts 34 to 37 tons steel, with an elongation of 17 per cent. in 8 inches, is specified, and for tension members 30 to 33 tons steel, with 20 per cent. of elongation. We have now about 15,000 tons of steel delivered and worked up, and are satisfied that the quality as supplied to us by the Steel Company of Scotland, and the Lándore Company, is admirably adapted for bridge construction. In making the tubes, the plates are heated in a gas furnace, and bent hot between dies, in a powerful hydraulic press. A slight distortion takes place in cooling, which is corrected by pressing the plates again when cold. After bending, all four edges are planed, and the plates built up into a tube. Traveling annular drill frames surrounding the tube, fitted each with ten traversing drills, bore the holes at once through plates, covers, and stiffeners, so that when again fitted in place for erection, every piece comes into exact juxtaposition. Similar traveling drill frames deal with the lattice-box girders, every hole being drilled as the machine advances. Generally the plant designed by Mr. Arrol for drilling the innumerable holes in the 42,000 tons of steel-work for the main spans is of signal merit and efficiency, and well worthy the attention of practical engineers. At the present time, although, as already stated, about 15,000 tons of steel-work is on the ground, only the approach viaduct girders and some of the bed-plates of the main spans are erected and riveted up. In a few weeks, however, the erection of the portion of the main spans over the North Queensferry piers will be proceeded with. The "skewbacks" and connecting tube will first be riveted up, and then a platform of temporary girders and planking will be constructed, and

raised gradually by hydraulic rams in the four vertical 12-feet diameter columns as the work of erection and riveting-up progresses. This platform will carry cranes and other appliances, and the men will be thoroughly protected, so that work will be carried on with as much confidence at a height of 350 feet as at sea level. When the portion of steel-work over the piers is erected, the first bay of the cantilever on each side of the same will be added, the work forming its own staging. This will be followed by succeeding bays until the cantilevers are complete, and the central girders will then be erected, probably on the same plan.

It will be observed that for certain parts of the Forth Bridge we use steel of a higher tensile strength than is at present considered admissible either for ships or boilers. This has not been done without full and mature consideration of the whole question. Our experiments showed that steel, having a tensile strength of from 34 to 37 tons per square inch, offered a decided advantage over very mild steel, when compressive stresses and the flexure of long columns were concerned. Indeed, an inferior quality of steel, such as would be used for rails, will stand compression far better than the best boiler steel or Lowmoor iron. Thus, I found a column twenty diameters in length of common Bessemer steel carry 27 tons per square inch, where one of mild boiler steel has stood but 17 tons. It would be inexpedient, however, to use inferior steel, even for the compressive members of a bridge, and, therefore, a high quality and high tensile resistance were indicated. Although this steel takes a temper, and becomes brittle, if cooled in certain ways, it will stand the ordinary Admiralty temper tests, bending to a radius of double the thickness, after being made red-hot and cooled in the usual way. In a boiler the steel plates are subject to great changes of temperature and consequent stresses from expansion and contraction. In a ship, almost every plate in the hull is subject to alternate tensile and compressive stresses when amongst waves; and, further, a vessel is liable to severe alternating stresses and shocks on taking ground, dry docking, and under other circumstances. In the compression members of the Forth Bridge, the steel is subject only to a steady pressure of varying intensity, and a quality of steel was adopted which combined perfect facility in working with a high resistance to compression. Although an increased tensile strength is accompanied by a decidedly increased resistance to flexure in columns and struts, the latter is not proportional to the former. If the thing were practicable, what I should choose as the material for the compression members of a bridge would be 34 to 37 ton steel, which had been previously squeezed endwise in the direction of the stress to a pressure of about 45 tons per square inch—the steel plates being held in suitable frames to prevent distortion.

My experiments have proved that 37-ton steel so treated will carry as a column as much load as 70-ton steel in the state in which it leaves the rolls, that is to say, not previously pressed endwise. It would be a matter of much practical moment to ascertain if some convenient treatment could be devised which would endow steel with this greatly increased power of resistance to compression without injuring its resistance to tension, or its ductility, which remained unaffected by previous compression in my experiments. At least one-half of the 42,000 tons of steel in the Forth Bridge is in compression, and the same proportion holds good in most bridges, so the importance of gaining an increased resistance of 60 per cent. without any sacrifice in the facility of working, and safely belonging to a highly ductile material, can hardly be exaggerated. Our experience has led us to the conclusion that sheared edges are a more fruitful source of fracture than partial tempering, or other contingencies. All of our bent plates are made red-hot, and the effect of the shearing is thus eliminated even before planing. Those plates which are not heated have the edges carefully planed so as to leave no trace of the shearing, and we find that whether we are dealing with 30-ton or 37-ton steel, the plates so treated stand all the desired tests. Experiments which I have made, and am still making, on the resisting power of different classes of iron and steel to repeated bendings, such as the shaft of a marine engine undergoes if the bearings get out of line, indicate that the superiority of low-tension steel is con-

siderably greater than the increased ductility would indicate. In conclusion, I may state that the approximate value of the plant now at the Forth Bridge is £250,000, and of the work executed £600,000.

THE DECAY OF STONE ON THE GROUND LEVEL.

From "The Building News."

The decay of stone on the ground level of buildings is a subject of great importance and anxiety to those responsible for substantial erections in this material; for in many instances, before the work can be got out of hands by the contractors, signs of disintegration present themselves, and before many years have passed the evil has intensified to such an extent that the lower parts of the buildings are in a state of decay bordering upon ruin. The same evil presents itself in connection with old buildings, and it is invariably the case that this dissolution in the lower part of the building hastens the process of disintegration over the whole fabric.

This detail of decay in stone is traceable, in a primary sense, to absorption of water from the foundations; but, in a secondary sense, to a variety of causes. Absence of damp course, as in old buildings, is a prime cause of this decay of stone on the ground line, and inefficient damp course is a secondary cause. These are intensified by thick walls, filled in with rubble and grouts, backed with a damp and humid atmosphere. In some cases it is accelerated by the finished ground being inadvertently brought above the damp course, or by stone paving being brought up to its level, wherein the beating rain gets access to the superstructure. The character of the stone used in the house of a building is an important factor. A porous sandstone, like the millstone grit of the Carboniferous system, is extremely durable, whilst a porous limestone, like the Bath and Ancaster stones of the Oolitic system, is extremely perishable. The reason of this is not far to seek, for the cementing medium in the sandstone is silica, impervious to the action of water traveling to the face of the stone to evaporate in the rarified atmosphere; while the cementing medium in the limestone is carbonate of lime, more or less in a state of crystallization, but, nevertheless, more or less solvent in water, containing, as it does, in important centers, a dangerous amount of carbonic acid. In the former case the stone will give out its water without ruin being stamped upon its face, whereas in the latter, the mineral matter, unable to pass into the air, will crystallize on the outer face or skin, an operation that will mark the decay of the stone by disintegration.

In a humid climate like that of England, stone, independently of its connection with the foundation of a building will, during half the year, be conducting the process of absorption. This is an operation so well known that stone walls have an unenviable character for their dampness, a character that invariably causes them to be built hollow, or lined with brick or a framework of wood. The same stone, during the summer season, will be giving off its stored-up moisture, an operation of no moment on the north side of a building, where the absorbing power of the sun is not experienced, but one that, on the south side, will be carried to a great and a dangerous extent. It is to the absence of the sun on the north side of a building, and the uniform character of the moisture in the stone, that it is always in a better state of preservation than the south side, and it is to the presence of the sun on the south side, and the extremes of moisture, heat, and dryness experienced by the stone, that it is invariably found in a decayed or ruinous state. The moisture present in the north and south walls of a building in the winter or humid season is identical, the reverse being the case in summer, for the north wall, if the surroundings are favorable, will be coated with moss or lichen, whilst the south wall will be dry and arid. It is to this

high or active state of evaporation in the sun that the decay of stone on the ground line is, if not actually brought about, certainly accelerated. So much is this the case, that if we examine the north wall of a building we shall find the line of disintegration on the ground level scarcely marked, while the line on the south, or other sides exposed to the influence of the sun, illustrates an advanced state of decay. It naturally follows that special attention should be brought to bear on all but the north sides of a building; the damp course should here be most effectual, and the walling upon it placed above any possible contact with the ground, or the influence of beating rains. The ashlar work should be constructed in a stone whose power of absorption is of a low order, for it is to the large measure of absorption and evaporation in the absence and presence of the sun, that dissolution is brought about. In carrying out this policy, care should be exercised in avoiding all projections, recesses, &c., which collect and distribute water, on what is known as the drip principle. a principle most markedly at variance with the preservation of stone. If these features are imperative, arrangements should be made for collecting and removing the water, a thing by no means impossible where wall pipes are introduced in connection with the roofs. If a porous stone is used, more especially if it be limestone or dolomite, we advise the coating of the same with preservative composition, a material, on the one hand, that prevents undue absorption, and on the other, undue evaporation. In giving this advice, we are not unmindful of the fact that it gives to the stone a paint-like surface for a time; but it has proved so efficient in the extensive restorations carried out on the south side of York Cathedral, by the late and lamented G. E. Street, that we have no hesitation in advocating it.

THE THOMAS-GILCHRIST OR BASIC BESSEMER PROCESS.

Extract from a recent report by ELIAS PETTERSON.

Translated from the "Jernkontorets Annaler" by MAGNUS TROILIUS.

THE Thomas-Gilchrist, or Basic-Bessemer process, is now extensively used in Germany, France and Belgium, particularly for the manufacture of *soft* steel. Owing to the low rates of freight at which the Spanish Bilbao ores are carried to Amsterdam and Rotterdam, the price of Bessemer pig has lately been only 48 to 50 marks per 1,000 kg., with Thomas pig at 42 marks. In Eastern France the Thomas pig was counted 10 francs cheaper than the Bessemer pig, blown from pure Spanish ores. The Thomas pig, however, causes more loss and expense in refining; the ingots from both processes are therefore about equal in price, as shown by the following figures:

ACID PROCESS.

	Marks.
Pig, 1100 kg., @ 52 marks per 1000......	57.20
Blowing expenses......................	14.40
Ingots per ton....................	71.60

BASIC PROCESS.

	Marks.
Pig, 1165 kg , @ 44 marks per 1000.....	51.26
Blowing expenses......................	20.00
Ingots per ton..................	71.26

MATERIALS FOR BASIC LINING.

Magnesia—bricks being too expensive—ground, burnt dolomite mixed with water, free or boiled coal tar is now mostly used. The dolomite has the following composition:

AT HOERDE.

SiO_2	= 2.02	per cent.
$Fe_2O_3+Al_2O_3$	= 2.30	"
$CaCo_3$	= 61.31	"
$MgCo_3$	= 34.32	"
	100.05	

AT ROTHE ERDE.

SiO_2	= .60	per cent.
$Fe_2O_3+Al_2O_3$	= 1.16	"
CaO	= 32.45	"
MgO	= 19.15	"
Co_2	= 46.45	"
	99.81	

At Ilsede.

SiO_2	= 1.85	per cent.
Al_2O_3	= 2.05	"
CaO	= 30.12	"
MgO	= 19.21	"
FeO	= .26	"
Co_2	= 44.97	"
H_2O	= 1.99	"
	99.95	

In most places the dolomite is crushed into fist-size pieces, and calcined in a cupola with alternate layers of coke. At Hoerde, with good coke, the consumption was only 13 per cent., but at Hayence, where poor coke was used, the consumption was 100 per cent. Good blast is required so to give heat enough to glaze the dolomite, thereby preventing a too rapid absorption of moisture.

The furnace at Hoerde was 6 meters high, and produced 10,000 kg. of burnt dolomite in 24 hours.

The burnt dolomite is freed from adhering slag and coke, and then ground and mixed with coal tar. The commercial tar contains 18–20 per cent. of water, which is removed by boiling in a cast-iron pan, from which the tar is afterwards drawn up into a measuring chamber; from there it is subsequently carried to the mixing machinery. The tar generally retains .2 to .5 per cent. of water after boiling.

For the sides of the converter a more finely-ground dolomite is generally used than for the bottoms, which are made of a mixture of half finer, half coarser, dolomite. Somewhat more tar is generally used for the bottoms than for the sides; this is, however, very different for different works. Thus, at the Westphalian Works, 18 to 20 per cent. of tar was used, whilst in France, at Hayence and Mont Saint Martin, only 7 to 9 per cent. was necessary.

A rammed basic lining is more durable than a lining built up of basic bricks, there being no joints. For this reason, both bottoms and sides are now generally rammed in with heated iron rams.

The sides of a 10-ton converter are made 450 mm., the bottom 650 mm. thick. As pattern for the manufacture of bottoms is used a conical cast-iron ring, which is placed upon a closely-fitting, cast-iron bottom plate. This plate forms the lower part of the converter bottom when completed. Into this bottom are fastened needles or tuyere stones. The following figures, giving the number of needles and tuyere holes used, deserve mentioning:

Kaiserslauten........	5-ton furnaces,	42 needles,	with 11mm. diam.
Oberhausen..........	8 "	45 "	" 12 "
Hoerde..............	10 "	50 "	" 12 "
Dortmunder Union...	10 "	50 "	" 12 "
Mongmedy..........	10 "	18 tuyere stones, with 9 holes,	" 11 "
Hayence..	8 "	12 " " 8 "	" 12 "
Mont St. Martin.....	15 "	21 " " 9 "	" 10 "
Athus...............	12 "	19 " " 9 "	" 10 "
RheinischeStahlwerke	7 "	7 " " 7 "	" 12 "
Phœnix.............	6 "	11 " " 7 "	" 10 "
Gebrüder Stumm....	8 "	8 " " 7 "	" 12 "

The tuyere stones are brought from England, where they can be obtained cheapest.

The height of the tuyere stones is adapted to the more or less burnt-out state of the bottom. Thus, at Athus, for new bottoms, were used stones 630 mm. high, and for burnt-out bottoms, stones 440 and 290 mm. high.

The bottoms are dried in a long chamber, holding many bottoms at the same time; the bottoms are kept there for 16 to 18 days, being moved, during that time, gradually up to the hottest part next to the fireplace. Confusion, however, is sometimes caused by having so many bottoms at different stages of dryness in one room, and at Hoerde it was therefore intended to build a special heating-room for each bottom. Every fresh converter lining must be well dried by burning about 16 cwt. of coke, or 24 cwt. of coal, in the converter. The drying requires 5 to 7 hours. The heat should be of such strength as to glaze the lining after expelling the tar fumes.

After inserting the converter bottom from below, the joint between bottom and converter is tightened by injecting plastic balls of dolomite and tar through the converter nozzle. The bottoms can stand 22 to 24 blows (at Rothe Erde they

were said to stand 30 blows); as a rule, however, they do not stand more than 15 to 18 blows, and sometimes only 9 or 10.

The converter bottom is always exposed to the hardest wear; the tuyere stones made of clay-chamotte, particularly, are worn rapidly. These tuyere stones, as already mentioned, can easily be replaced by shorter ones. The sides of the converter can stand from 80 to 100 blows without repair.

Plant.

Holley's arrangement for changing bottoms is used at the new works, Athus, in Belgium. Bottoms could be changed in 45 minutes. At Mont St. Martin the same arrangement was going to be introduced.

At all the works, except one, three converters were considered to be the right number for a continuous run. Only at Athus the Holley arrangement made it possible to turn out 400 tons a day, by means of 2 12-ton converters.

At other works, with only 2 converters, 3 to 4 hours were wasted every day for changing one or two bottoms, and only 22 to 24 blows could be made a day; at the works with 3 converters, 28 to 34 blows could be made in 24 hours.

Bessemer converters, which can blow 10-ton charges with an acid lining, can if lined with basic lining, only handle 8 tons.

Casting Arrangements.

In all newly-built Thomas works the casting pit is placed at some distance from the converters, so as to leave ample room for removing the large amounts of slag which are characteristic of this process. The ladle is transported by means of a crane attached to a locomotive, as at Hoerde and Ilsede, where the converters are all in one line, or the ladle is carried between converter and pit by means of two fixed cranes.

Thomas Pig.

Thomas pig generally contains 1.5 to 3 per cent. of phosphorus, and .1 to 1.5 per cent. of silicon. A few analyses may be here given:

Name of Works.	Phos. %	Silicon %	Mang. %
Neunkirchen	2.5	.5	2.0
Hayence	3 2	1.5	1.2
Mont St. Martin	2.0	1.5	1.5
Athus	2.0	.8	1.5
Rothe Erde	2.0	1.2	1.5
Ilsede	3.0	.1	2.8

The phosphorus must not exceed 3 per cent., as the loss and the deterioration of the converter lining increase with the phosphorus. The hotter the pig and the higher the per cent. of phosphorus is, the less silicon is required. A high per cent. of manganese is always useful, in order to render the slag more fluid and to remove sulphur, which occurs in considerable quantities in Thomas pig.

Lime.

An addition of 12 to 14 per cent. of lime is sufficient for low-phosphorus pig; otherwise 17 to 20 per cent. is required. The lime is generally taken *direct* from the calcining furnace, in order not to cool the converter unnecessarily. The lime used is very pure. At Hoerde it contained:

Lime, 89.05; magnesia, 3.05; alumina and iron peroxide, .62; silica, .82; phosphoric acid, .01; sulphuric acid, .42; carbonic acid, 5.37. Total—99.34.

In order to render the slag still more fluid, 1.5 per cent. of fluoride of calcium was added at the French works.

At Hayence, Athus, and Mont St. Martin the pig was taken direct from the blast furnaces, but at all the other Thomas works it was re-melted in cupolas.

The Blow.

Owing to the small amount of silicon present the carbon begins to burn very soon, and continues to burn with a blue, carbonic-monoxide flame for about 10 minutes. Then follows the after-blow, by means of which the phosphorus is removed from the metal. The after-blow lasts for 2 to 4 minutes; at most works a certain number of revolutions of the blowing engine is used for the after-blow, after the completion of which the converter is tilted over, so that most of the slag, now rich in phosphoric acid, may run out.

At the same time a sample of metal is taken out and tested by rapidly hammering it into a round plate, cooling in water and breaking in two. The more or less coarse fracture indicates whether the metal is dephosphorized or whether the after-blow has to be continued.

Great difficulties have to be contended with at this stage of the process, principally owing to the necessity of not continuing the after-blow too long, in order to save

the converter. The result is that the dephosphorization is seldom carried lower than .1 per cent.

Percentages of Phosphorus in the Product.

One works has classified the basic blows of 1884 according to the percentages of phosphorus in the refined product, showing the following not very good results:

80.00 %	of the blows had less than	.10 %	of P.
4.76 "	" exactly	.10 "	"
15.29 "	" more than	.10 "	"

Final Additions.

Spiegel is used for higher carbons' and ferromanganese for lower carbons. The spiegel is melted in a cupola, whilst the ferromanganese is only preheated or used cold. Sometimes when the bath was very unruly, 40 to 60 kg. of "ferro-silicium," with 10 to 14 per cent. of silicon, was added, in order to quiet the bath and secure solid ingots.

Mechanical and Chemical Tests.

Besides the test mentioned above, a sample was always hammered out and hardened from a white heat. The sample was then welded and hammered to 1″ square, cooled and fractured.

For control, the carbon and phosphorus were always rapidly determined in each blow by chemical analysis.*

At Kaiserslauten large quantities of plate, 5 mm. thick, for coats of mail, were manufactured. This plate must have 18 to 20 per cent. elongation, with a tensile strength of 50 to 55 kg. per sq. mm.,† and was tested by firing ten charges in succession from breech-loading rifles at 50 meters distance, against plates 300 mm. in diameter. The back side of the plate must then show no signs of cracking, unless two bullets strike the same spot.

Loss.

The loss in the Thomas process is from 15 to 17 per cent. For the manufacture of rails, less lime and shorter after-blow are used; the loss is, therefore, in this case less than when it is desired to remove the phosphorus more completely.

The softer the metal is desired to be, the more difficult and expensive becomes its manufacture, the bath being unruly to a high degree. During the cooling a violent evolution of gas takes place, causing bad ingot tops and much loss in rejection.

At Hoerde, a pig was for a long time made out of only Thomas and puddle slags. One pig thus obtained contained:

C	=	.87	per cent.
Si	=	.02	"
P	=	18.18	"
S	=	trace	"
Mn	=	4.53	"

As a rule, however, only 5 to 6 per cent. phosphorus was present in the pig used.

Thomas Slags as Fertilizers.

The following analyses give an idea of the composition of the Thomas slags:

	A	B	C	D
SiO_2	4.86	2.97	2.64	7.88
P_2O_5	20.58	23.58	23.25	20.16
CaO	46.31	60.28	59.57	54.42
MnO	5.64	1.49	1.72	4.77
FO	12.56	6.24	7.57	8.00
F_2O_3	6.97	1.28	1.60	1.92
MgO	....	1.82	1.31	1.10
Al_2O_3	....	.75	.64	.98
CaS	....	1.34	1.21	1.31
		99.75	99.51	100.54

Dr. Scheibler has patented a process for extracting the phosphate of lime from these slags for fertilizing purposes. His process is used at Schalke, in Westphalia, and at Stollberg, in Aachen, and is there carried out essentially as follows:

The slags are roasted in reverberatory furnaces, 9 meters long and 1.5 meter wide; 20,000 kg. slag are thus burnt in 24 hours, with a consumption of coal of only 130 kg. The roasting oxidizes the sulphide of calcium, and converts the iron and manganese into higher oxides. The roasted slag is pulverized, after a previous treatment with steam, which converts the *free* lime into calcium hydrate, thus aiding the pulverization.

From the pulverized slag the phosphate of lime and the silica are extracted with dilute hydrochloric acid (1.10.)

* For information regarding rapid chemical analysis in steel manufacture *vide* Trollius' "Chemistry of Iron," John Wiley's Sons, New York.

† To convert kg. per sq. mm. into lbs. per sq. inch, multiply by 1422.47.

About 1,450 kg. of acid are used for 1,000 kg. of slag. The solution requires only a few minutes for completion, and is carried out in big vats.

After some standing the solution is drawn off from the insoluble residue, and neutralized with lime water, which precipitates the phosphoric acid, together with a little silica, the larger part of the silica remaining in solution.

The precipitated phosphate is separated from the liquid by means of huge filtering presses. Thus, wet cakes containing 65 to 70 per cent. of water were obtained.

The phosphates are finally dried and pulverized simultaneously in specially-constructed contrivances. The final product contained:

	At Stolberg. Per cent.	At Schalke. Per cent.
P_2O_5	31.82	31.66
SiO_2	13.26	9.05
CaO	35.01	34.99
F	2.43	2.78
Loss on ignition	11.94	11.97

50 per cent. of the slag treated is obtained as phosphate. The residue obtained, after extracting the roasted slag with dilute hydrochloric acid, amounts to about 30 per cent. of the total slag, and forms an excellent material for the manufacture of manganiferous pig iron. At Schalke this residue was found to contain:

F_2O_3	=	52.4	per cent.
Mn_2O_3	=	15.10	"
MgO	=	9.90	"
CaO	=	14.18	"
P_2O_5	=	5.81	"
SiO_2	=	4.40	"

HYDRAULIC FORMULAS:

A COMPARISON OF FORMULAS AND RESULTS—DARCY'S FORMULA APPLIED.

By NATHANIEL HILL.

Written for VAN NOSTRAND'S MAGAZINE.

The following formulas for the velocity of flow of water through pipes, are selected from Table 65, of Fanning's "Treatise on Hydraulics," being all that seem, from their results, to have been designed for that use:

Fanning.

$$V=\left(\frac{2g\,H}{(1+c_r)+m\frac{l}{r}}\right)^{\frac{1}{2}}$$

Etelwein.

$$V=50\left(\frac{dH}{l+50d}\right)^{\frac{1}{2}}$$

Neville.

$$V=\left(\frac{Hr}{.0234+.0001085l}\right)^{\frac{1}{2}}$$

Hawksley.

$$V=48{,}045\left(\frac{dH}{l+54d}\right)^{\frac{1}{2}}$$

V being velocity, in feet per second.
d " dia. of pipe, in feet.
H " entire head, in feet
l " length of pipe, in feet.
m " coefficient of flow.
r " Hyd. mean rad., $\frac{d}{4}$, in feet.
c_r being ratio of head lost from contraction, to head to induce velocity, $\frac{h'}{h}$.
h " head to induce velocity.
h' " head lost from contraction of stream.
h'' " head lost from friction in pipe.
C_v " ratio of area of contraction to area of pipe.

These formulas will first be shown to be identical in construction, and founded upon the same considerations, viz.:

Head to induce velocity.
Head lost from friction.
Head lost from contraction of stream at entrance to pipe.

Referring to "Young's Summary of Etelwein's Hydraulics," better known as "Tredgold's Hydraulics," page 204:

$$f=\frac{alv^2}{d};$$

therefore $$v^2=\frac{fd}{al},$$

also $$f=h-\frac{v^2}{b^2};$$

hence $$v^2=\frac{b^2dh-dv^2}{ab^2l}$$

or $$v^2=\frac{b^2dH}{ab^2l+d}. \qquad (A)$$

Where f=head lost from friction.
a=a constant.
h=total head.
b=coefficient for determining velocity from height,

b then is $c_v\sqrt{2g}$ in Fanning's notation;

$$a=\frac{f}{l}\times\frac{d}{v^2}=\frac{4ri}{v^2}.$$ From Fanning, we take $$m=\frac{2gri}{v^2}$$

Hence $$a=\frac{4m}{2g}$$

Substituting $c_v^2 2g$ for b^2, and $\frac{4m}{2g}$ for a in (A), also $4r$ for d and H for h—

then $$v^2=\frac{c^2_v\ 2g\times4rH}{\frac{4m}{2g}c_v\times2gl+4r},$$

whence $$v=\left\{\frac{2gH}{m\frac{l}{r}+\frac{1}{C_v^2}}\right\}^{\frac{1}{2}};$$

which is Fanning's 12th Equation. This readily reduces to the more common form, the 11th Equation—

$$v=\left\{\frac{2gH}{m\frac{l}{r}+(1+c_r^2)}\right\}^{\frac{1}{2}}.$$

In the Etelwein formula, b is taken 6.6; and "for the whole velocity due to the height, the coefficient by which its square root is to be multiplied is 8.0458;"

then $$c_v=\frac{6.6}{8.0458}=.82$$

ab^2 is given from "Buat's Experiments" .0211;

whence $$a=\frac{0211}{6.6^2}=.000484.$$

To compare this with the m of Fanning's formula, it must be multiplied by $\frac{2g}{4}$ or 16.1. It is then found to be equivalent to .0075 in that formula.

The Etelwein formula, with the values of a and b inserted, is—

$$V=\left(\frac{43.6dH}{.0211l+d^2}\right)$$

or $$45.5\left(\frac{dH}{l+47d}\right)^{\frac{1}{2}} \qquad (B)$$

and is finally taken at

$$50\ \left(\frac{dH}{l+50d}\right)^{\frac{1}{2}}.$$

The Hawksley formula,

$$V=48.045\left(\frac{dH}{l+54d}\right)^{\frac{1}{2}},$$

is easily traced back to

$$V^2=\frac{42.73}{.01852l+d}; \qquad (C)$$

whence b=6.54, c_v = .815 and a=.000433, equivalent to m=.00697.

The Neville formula is equivalent to

$$V^2=\frac{.01852l+d}{42.74dH}$$

which compares with the last form of the Hawksley formula, (C).

The coefficient, in the equation for resistance head, is assumed constant in all these formulas, except Fanning's, where it is variable, and to be assumed, by aid of prepared tables. To avoid this necessity, Darcy varied the form of the equation, and what is known as Darcy's formula is merely a formula for loss of head from friction alone. In "Proceedings of American Society of Civil Engineers," XXXVII., 1872, the formula appears thus:

$$i=.00371\frac{(d+1)v^2}{d^2}$$

i=loss of head per foot of pipe, in feet.
d=dia., in inches.

Let $v^2=2gH'$, H′ being the head remaining after all losses are deducted:

$$v^2=2g\left(H-l.00371\frac{(d+1)v^2}{d^2}\right)$$

or, reduced,

$$v=\left(\frac{2gHd^2}{2gl.00371(d+1)+d^2}\right)^{\frac{1}{2}}$$

(See *Engineering and Mining Journal*, Sept. 7, 1878.)

or $$v=\left\{\frac{Hd^2}{l.00371(d+1)+\frac{d^2}{2g}}\right\}^{\frac{1}{2}} \qquad (1)$$

This formula is similar in construction to the four compared, except that it has

Darcy's value for loss of head from friction, and provides for no contraction.

For contraction $=c_v$:

$$v^2=2g\left\{H-l.00371\frac{(d+1)}{d^2}v^2-\right.$$

$$\left.-(1-c_v{}^2)\left(H-l.00371\frac{(d+1)}{d^2}v^2\right)\right\}=$$

$$=c_v{}^2 2g\left(H-l.00371\frac{d+1}{d^2}v^2\right)$$

$$\text{or,}\quad V=\left\{\frac{Hd^2}{.00371(d+1)l+\dfrac{d^2}{c_v{}^2 2g}}\right\}^{\frac{1}{2}} \qquad (2)$$

This is Darcy's formula adapted to the Etelwein construction.

If c_v be taken at .815 and $2g$ at 64.4.

$$v^2=\frac{Hd^2}{l.00371(d+1)+.0233776d^2} \qquad (3)$$

or introducing r in place of d

$$v^2=\frac{Hr}{l.0009276+\dfrac{l.0002319}{r}+.0233776r} \qquad (4)$$

or if i is taken $=\frac{H}{l}$.

$$v^2=\frac{ri}{.0009276+\dfrac{.0002319}{r}.+0233776\dfrac{r}{l}} \qquad (5)$$

Or, if r is taken in feet,

$$v=\left\{\frac{ri}{.000077292+\dfrac{.00000161}{r}+.0233776\dfrac{r}{l}}\right\}^{\frac{1}{2}} \qquad (6)$$

which may be compared with the formula called Darcy's, quoted by Fanning, and given below:

$$v=\left\{\frac{ri}{.00007726+\dfrac{.00000162}{r}}\right\}^{\frac{1}{2}} \qquad (D)$$

The latter may be used in place of the former, when the last term is an insignificant addition to the denominator; this would occur when the ratio $\frac{r}{l}$ approaches .00001.

Below is a comparison of the results of the formulas referred to, as they appear in Fanning's Table 63, with those of (3).

Pipe, 1 foot diameter; head, 100 feet; lengths as below in feet:

Lengths, in feet.............	5	50	100	1000	10,000
	vel.	vel.	vel.	vel.	vel.
Etelwein......................	67.40	50.	48.02	15.427	4.985
Neville........................	62.54	47.08	38.75	14.78	4.78
Hawksley....................	62.555	47.08	38.72	14.79	4.80
Darcy (D)..	244.12	77.133	54.64	17.279	5.464
Fanning......................	63.46	51.11	43.11	17.386	5.392
Darcy's adapted, formula (3)..	63.1796	49.92	41.93	16.706	5.44
Darcy's adapted, " (7)..	64.487	50.558	42.403	16.728	5.446

Sub-heads Compared, Fanning. (Page 253.)

Lengths, in feet	5	50	100	1,000	10,000
Velocities	63.463	51,111	43.111	17.386	5.392
$h=\frac{v^2}{2g}$	62.542	40.568	28.863	4.694	.451
$h'=\frac{.5055v^2}{2g}$	31.583	20.487	14.575	2.370	.228
$h''=m\frac{l}{r}\times\frac{v^2}{2g}$	5.878	38.943	56.571	92.941	99.33
H	100.0	100.0	100.0	100.0	100.0

By Formula (8).

Lengths in feet	5	50	100	1,000	10,000
Velocities	63.18	49.92	41.93	16.706	5.44
$h=\frac{v^2}{2g}$	61.98	38.70	27.30	4.33	.460
$h'=.5055\frac{v^2}{2g}$	31.34	19.56	13.81	2.19	.23
$h''=l.00371\frac{(d+1)}{d^2}v^2$	6.68	41.74	58.89	93.48	99.31
H	100.0	100.0	100.00	100.00	100.00

By Formula (7) given below.

Lengths in feet	5	10	100	1,000	10,000
Velocities	64.487	50.558	42.301	16.728	5.446
$h=\frac{v^2}{2g}$	64.57	39 69	27.80	4.35	.460
$h'=.5055\frac{v^2}{2g}$	32.65	20.07	14.05	2.19	.23
$h''=\left(l-\frac{3d}{12}\right).00371\frac{(d+1)}{d^2}v^2$	2.78	40.24	58.15	93.46	99.31
H	100.00	100.00	100.00	100.00	100.00

Etelwein's (B), Neville's, and Hawksley's are equally consistent in accounting for all the head, at the values assumed in those formulas for the coefficients of contraction and friction.

Fanning's coefficient is taken the same in determining h'' as was assumed in finding v: the results would vary a little from an exact accounting for the entire head, if the tabled value of m was taken, for the value of v found. This is not important, in the present condition of the tabled values.

Below are given a few values, being those most convenient for comparison, of coefficients computed from experiments, and from the tables, pp. 237–241.

Exps. by	Dia.	Vel.	Coeff. by Exp.	By Table.
Darcy...	1.6427	1.3765	.00629	.00545
"	.9751	10.35	.00600	.00507
Fanning.	1.667	4	.00525	.00498
	1.667	1.488	.00534	.00512
	1.667	1.935	.00525	.00532
Couplet.	1.6	3.4779	.00700	.00502

In the application of any formula to short lengths of pipe, l should be taken $\left(l-\frac{3d}{12}\right)$ when d is given in inches, or $(l-3d)$ when given in feet, as, a length of pipe equal to three diameters, is involved in the coefficient of contraction.

Formula (2) thus modified, becomes

$$v=\left\{\left(l-\frac{d}{4}\right)\frac{\mathrm{H}d^2}{.00371(d+1)+c_v^{\,2}2g}\right\}^{\frac{1}{2}} \quad (7)$$

When l is 3 times the diameter of pipe the velocity of discharge is known to be $(c_v^{\,2}2g\mathrm{H})^{\frac{1}{2}}$, to which this formula reduces.

The velocities and sub-heads by formula (7), are given in table under that by formula (3), for convenient comparison.

STANDARD WEIGHTS AND MEASURES.

By JOHN L. CULLEY, C. E.

The paper of Mr. Arthur Hamilton-Smythe, read before the British Institution of Civil Engineers, Jan. 20, 1885, is a valuable and interesting contribution to our literature. Not less interesting was the discussion that followed.

We are pleased at the full expression of individual views there given, though we must confess ourselves at times amused at the quibbles resorted to to sustain the old regime, as, for instance, the mental arithmetic argument. I am, however, surprised at the author throughout his paper stooping to small things and making egregious statements to maintain his position.

There are certain broad elements connected with the meter that appeal to the intelligence of every land, such, for instance, as he has expressed in his first two paragraphs. But when petty argument is used to sustain one's position, contempt therefor is begotten.

His statement that the yard or meter, on account of being the approximate human stride, are the natural units of lineal measurement, is an incorrect statement. The foot has become the most convenient unit of universal use, proved by the fact that it and its approximates have been used for all time, ancient and modern, and that, too, in modern times, in spite of the fact that the yard has been legalized in all English-speaking countries. Even in France, to-day, the old French foot is persisted in, and while the author is technically correct in the next paragraph, practically he is not, for the foot is our practical standard.

He seems to forget that while machinery is wearing out and becoming obsolete, the manufacturer is all the time replacing it by new and improved patterns, but graduated to the same old standard unit. Changes of machine systems are usually radical and involve great expense. His statement of the process a carpenter goes through to calculate the length of a stick of timber with a two-foot rule is ridiculous. It is quite evident the author never served a carpenter's apprenticeship. One would suppose from his reference to the fact that the sixteenth of an inch was too large for minute work, that he had

never heard of $\frac{1}{32}''$, $\frac{1}{64}''$, $\frac{1}{128}''$, etc. Small as may be the practical importance or advantage of these binary subdivisions, nothing is to be gained by ignoring their existence. The fineness of one-hundredth of a foot to one-hundredth of a meter is to the ordinary intellect as three to one, yet the author, in his comparison of foot and meter leveling rods, would seemingly have us infer the reverse. The rule he suggests of binary subdivision of the hundredths by ocular estimate applies with equal force to the foot rod as to the metric rod.

The same criticism applies to his comparison of the Gunter and the twenty-meter chain, wherein he states that the latter is capable of subdivision into smaller lineal measures. The surveyor, now, if he uses such a thing, divides his links into tenths. The Gunter chain is fast becoming obsolete, but was in keeping with early survey practice of angles read no closer than one-fourth of a degree; of tables of latitude and departure carried out to three decimal places for one-fourth of a degree only, and when their best results were only bad approximates. But now, with instruments reading to twelve seconds, with tables of six decimal places for every minute, and an improved chain to meet these requirements, he who persists in the use of the old Gunter chain has divided the links into tenths, while the live engineer uses steel tapes one hundred feet long, graduated throughout their entire lengths to one-hundredths of a foot. The superiority of the English one-hundred-foot chain is sustained by the friends of the meter, by the trouble they have to find arguments to show that the meter chain is nearly as good as the English chain. A ten-meter chain, while of convenient denomination, is too short for practical use, and the twenty-meter chain, while it approaches a convenient length for practical use, is of an inconvenient denomination for either use or calculation.

The meter advocates, believing us to be in the abandoning mood, since we believe in the abandonment of all lineal units except the foot, urge us to abandon everything, the foot as well as everything else, and insist that we should go one step further and take to ourselves the meter. We would willingly do this if convinced of the desirability of the change or of the superior claims of the meter over the foot.

There is a decided objection to the abandonment of the foot unit. It is the most convenient unit for all purposes. Its universal use in spite of legal enactments proves its desirability. It is the only unit by which people have, of their own accord, expressed all kinds of dimensions.

The meter came not from the choice of the intelligent masses, but by imperial decree, and is both inconvenient and unwieldy.

The most beautiful theory of the meter —the one ten-millionth part of the meridian quadrant—has long been abandoned by its most enthusiastic admirers. This like other claims for the meter, is of theoretical but not of practical importance.

Let National and State legislatures legalize the meter as the standard unit and it will not receive even the attention that the yard thus actually legalized with us now gets.

Amongst the numerous standards in use with us, there are several that for years and years have been in constant use giving rise to confusion, complications, distress, and loss of time. Many of them are complex and are objectionable both for measurement, calculation, and an intelligent understanding. For these and other good reasons, it is desirable to simplify and reduce these standards to a single unit. The best system of lineal measurements is of the simplest notation and of the most convenient standard.

The simplest notation is the decimal system, and the foot, as above stated, is the most convenient standard.

It is much easier to adopt the unit of most general use than one entirely foreign to our shores. This is the reason, as intimated by Hamilton-Smythe, that the public in all English-speaking countries are perfectly indifferent to the introduction of the meter—they have no hankering for it.

I am in favor of all wholesome reform that will improve our practice, and will gladly hail the introduction of the meter when convinced of its superior claims over the foot, or, of the desirability or utility of such a change. While open to such convictions I have yet to find good

and valid reasons for discarding the foot for the meter. This opinion holds with the intelligent masses. Hence their indifference to the meter introduction.

A reform movement started many years ago. The railroad engineers, needing something better than the Gunter chain, introduced the one-hundred-foot chain of one hundred one-foot links, and from this has been developed the present perfected one-hundred-foot steel tape with its fine subdivisions. For many years the new one-hundred-link chain was used exclusively by the railroad men, but gradually it has worked its way into the hands of a large body of engineers outside of the railroad fraternity, just as the steel tape, first used outside, has come to be that by which railroads are now measured. Now all large cities, the majority of the smaller ones, and other corporations are measured by the steel decimal one-foot tape. Even the land surveyors are beginning to return farm surveys and subdivisions in feet decimals, and people cease to marvel that our one-tenths are larger than their one-twelfths, or, as they express it, "Why are your inches larger than ours?" The Pennsylvania Railroad Co. designate their stations by the decimal division they are between mile-posts, and people have no difficulty in understanding their significance.

It might be noted that at the 1885 Convention of Ohio Surveyors and Civil Engineers, at Columbus, the majority of members used the old chain. The fact is, however, all the live, wide-awake men, are using the steel decimal-foot tape. The great work thus inaugurated has only made a beginning, yet I predict that fifty years hence the old chain will have become a matter of tradition, and many another thing, now tolerated, will have passed away. This, like all other reforms, requires patience, constancy, and the lapse of time before meeting full and complete success. There are many things to be changed, altered, and given up, nor do we expect all these things to be done at once. An old dog, says the proverb, slowly learns a new lesson. People have their prejudices to overcome, and the ignorant must be educated. Still, much has been accomplished, thus giving cause for congratulation. Already draughtsmen express their plans decimally rather than duodecimally. We are beginning to hear of such plan scales as these: 4 feet to one-tenth; 500 feet to one-tenth, etc., in place of the old 4 feet to 1 inch, 14 feet to 1 inch, etc.

The advantages of binary subdivisions are more imaginary than real. The advocates of duodecimals have attached undue importance to them. They are the most common inch divisions used, and as such are very useful, but they have nothing to do with tenths. The trouble is, one system is applied to the other, binary to tenth, and *vice-versa*. $\frac{3}{8}$ of $\frac{1}{10}$, or the reverse, is an awkward expression. The engineer, in actual practice, has no more need of binary subdivision than the carpenter with his inch has of tenths. However, if decimals were universally used, there would be but little use for binary subdivisions.

Thus it will appear, while the foot has year by year a greater and greater tenacity with us, the task of introducing the meter becomes each year more hopeless. Because of the want of qualities to recommend it, I am satisfied that the meter will never come to us. So much for lineal measurements, which are gradually but surely working out a final system. So will the weights eventually be overhauled. The advantages of interchangeable weight and measure systems—the great claim of the metric system—are only fanciful. What profit is it that the cube of a certain unit, or of a certain decimal part of such unit of water, at a certain temperature, exactly equals an even number of weight units? Nothing! Our own weight system might be decimal, whose unit was a hundredth part of a cubic foot of water. This unit we might call a *pound* or any other convenient name. One-tenth of this unit might be called an ounce, which would be almost identical with our present avoirdupois ounce. Some such decimal weight system, it is to be hoped, will eventually supersede our present complicated, clumsy weight systems. Still, I contend that so long as the relations of the two systems are known, it matters little whether a cubic unit of water, at a given temperature, exactly equals an even number of weight units or not. So long as the temperature is constant the relation exists, but the moment it is not, the balance is lost. Even if the temperature remains always con-

stant, the exact relation of weights and measures is then true only for water.

Then as the specific gravity of any substance is never an even number, an elaborate calculation, or a resort to the tables is always necessary to determine the weight of a given bulk, be the systems interchangeable or not. What we need is a decimal weight system, founded upon a good and convenient unit—good and convenient because thoroughly adapted to the wants of trade and commerce, and to the expression of large as well as small quantities, and not because founded on a mere fanciful relation to the standard lineal unit. There is no objection to having denominations in any system. It is, in fact, a practical convenience to have them. In our American decimal money system, the nickel, dime, eagle, etc., are desirable denominations. But it is very important for the simplification of their use, that all denominations in any system, money, weights, or measures, should be derived decimally from the unit of its system.

CREOSOTE.

Abstract of a Report of Dr. C. Meymott Tidy, M. B., F. C. S., to the Directors of the Gas Light and Coke Company.

From the Papers of the Institution of Civil Engineers.

Let me define, first of all, exactly what I understand by the word "creosote." This is important, seeing that it does not imply a compound of fixed chemical composition. It is, in fact, a composite liquid, made up of a variety of chemical bodies in different proportions, the quality depending (first) on the kind of coal from which the coal tar is obtained, and (secondly) on the details of the distillation and treatment.

Broadly speaking, I mean by the word creosote a product of the distillation of coal tar after it has reached a temperature of about 300° Fahrenheit; in other words, after what is known as the light oil has distilled over.

It may be taken that about one-third the bulk of the tar consists of the "creosote" or "heavy oil" employed in creosoting timber.

The process of creosoting is effected by placing well-weathered wood in a vessel so constructed that a more or less perfect vacuum may be obtained. The creosote, heated to a temperature of from 100° to 120° Fahrenheit, is allowed to pass into the exhausted reservoir, and thus finds its way into the pores of the wood.

The advantages to be derived from the process are, I consider, of a *threefold* nature, and I give them in what appears to me to be the order of their importance:

1st. *A physical action.* A very greatly increased solidity is effected by choking up the pores, thus agglutinating the whole mass of the wood into a more or less solid block. Apart from its rendering the wood more solid, this physical action is important in preventing the subsequent absorption of moisture.

2d. *A physiological action.* The smell of the creosote imparted to the wood prevents germinal life, well known to be destructive to timber, from being developed within it. Seeing that the preservation of timber has been effected by such materials as chloride of zinc, sulphate of copper, &c., with greater or less success, and that the action of these bodies must be mainly, although I admit not entirely, dependent on their toxic properties, this physiological action is one of importance. It must be remembered, moreover, that creosote has the advantage of a well-marked smell, which odor most of the lower animals dislike. In this respect it is superior to the other bodies I have named.

Further, it is worth pointing out that all the constituents of the coal tar, and not the tar acids only, have a more or less well-marked tarry odor.

3d. *A chemical action.* Respecting the chemical action I would draw attention to the fact that tar acids are not only antiseptic, but that they possess the power of coagulating albumen. It is to

this latter action that I shall have to refer later on in this report, as playing an important part, in my opinion, in the preservation of the timber.

Having now dealt with what I conceive to be the details involved in the process of creosoting, the two following questions arise (1st) Upon what constituents of the creosote does its value specially depend, and what are the relative values of its different constituents? (2d) If there be constituents in the creosote, which, of themselves, possess no special value, do they in any respect lessen the activity of the valuable constituents?

The importance of considering the precise value of the several constituents of creosote, arises as follows:

Speaking generally, creosote may be divided into two classes—London and country creosotes. By London creosote we mean the creosote derived from the tars of the London gas works, the east-coast generally, and from the gas works of towns such as Southampton, Brighton, &c., where the coal employed is Newcastle coal. So far as I am able to learn, the larger proportion of the creosote produced in England is of this character. The two creosotes, however, being very different in their composition, it becomes important to consider them separately.

The London creosote has a somewhat high specific gravity, and contains a comparatively large percentage of naphthaline, and a small percentage (*i. e.*, less than 10 per cent.) of tar acids. Further, it contains a considerable quantity of the heavier portions of the oil, that is, of those portions not volatile at a temperature below 600° Fahrenheit.

The country creosote, on the other hand, has a less specific gravity, and is considerably more fluid than London creosote. It contains considerably less naphthaline than the London creosote, a larger total percentage of tar acids, and a smaller percentage of the heavier portions of the oil present.

The real question I have had in view in this inquiry being country creosotes *v.* London creosotes, it became necessary to inquire the relative values of the heavier portions of the oil, of the naphthaline and of the tar acids, in creosoting. In view of making what I have to say clear, I may venture to place before you what I conceive to take place in the operation of creosoting.

The creosote, having been sufficiently heated to bring the whole of the suspended constituents into a perfectly liquid condition, is driven into the wood, from which the air has been more or less completely exhausted. The tar acids, in the first instance, effect the coagulation of the albumen of the wood sap. This coagulated albumen mixes with the naphthaline of the creosote, which, so soon as the temperature becomes sufficiently reduced, is re-deposited, and forms, along with the heavier portions of the oil, a solid magma within the pores and fibers of the wood. That this formation of a solid magma actually occurs, I have convinced myself by numerous microscopic examinations of creosoted timbers.

Tar Acids.

The success of the process, therefore, being presumably assisted by the coagulation of the albumen, the question arises, What quantity of tar acids is necessary to effect this object?

To determine this point I have made a variety of experiments.

There is very little doubt in my mind, supposing that 10 lbs. of creosote per square foot be injected into the wood, and that the timber be of the kind ordinarily used (although, in this respect, different kinds of wood do not differ so much as might be supposed), that 2, or from that to 3, per cent. of tar acids would amply suffice to effect this coagulation of the sap albumen.

We are now led to consider if any value, and if any, what value, is to be ascribed to the tar acids beyond that needed to effect the coagulation of the albumen.

I am far from prepared to say they are otherwise entirely valueless. Still, it is a remarkable fact I have over and over again verified, that in the timbers that have been creosoted for a considerable time (say a year) very small quantities indeed (if any) of free tar acids are to be found.

I have, upon this point, instituted a series of examinations of sleepers obtained from independent sources, and of ages varying from one to twenty years, and it is a fact worth noting that, within

a very short time after a sleeper has been in use, the tar acids appear to be entirely dissipated.

Seeing, however, that the life of a sleeper is by no means so limited, the facts I have mentioned suffice to show that the action of the tar acids *per se* cannot have any very great or permanently preservative influence in creosoting.

I admit it was natural to suppose that bodies, commonly regarded as powerfully antiseptic, should have been the active agent in the process. Further, I must admit that it was with such view I commenced this inquiry. My recent investigations, however, have clearly shown that the value of the tar acids in the creosoting process has been greatly over-estimated.

I am convinced that so long as the quantity of carbolic acid present in the creosote, is sufficient to coagulate the albumen of the wood sap, that that, for practical purposes, is sufficient.

Naphthaline.

I have now to consider the value of the naphthaline.

I am disposed to think that this body is of infinitely greater value than at first sight appears. Admitting that, as an antiseptic, it is inferior to the tar acids, nevertheless, so far as preservative action alone is concerned, it must not be supposed to be inoperative. Its special value, however, consists in helping to render the wood solid.

But, it may be said, granting this to be the case, naphthaline is so volatile that the heat of the sun, especially the intense heat of an Indian climate, would soon drive the whole of it off. It is true that, on exposing a block of creosoted timber in an oven to a temperature of 54.5° Centigrade (130° Fahrenheit), and this may be taken to be an extreme tropical heat, the door of the oven, after a short time, shows conclusively that some of the naphthaline in the sleeper has undergone volatilization by the heat applied.

I would, however, direct attention to the following experiment:

I exposed a large block of creosoted timber (accurately weighed) to a temperature of 65.5° Centigrade (150° Fahrenheit). On weighing this at the end of 24 hours, I found it to have lost 1,200 grains. On exposing the same block to the same temperature for another 24 hours, it lost 135 grains, whilst on continuing the exposure for a third 24 hours, it lost only 15 grains. After this the loss was practically *nil*.

I now planed off about $\frac{1}{4}$ inch of the block I had already heated. This done, I again exposed it to a heat of 55.5° Centigrade (130° Fahrenheit) for 24 hours, during which time it lost 1,150 grains. The loss on the second day was less than 100 grains, whilst on succeeding days the loss was practically *nil*.

The surface of the wood was again planed off, and similar experiments repeated a third time with almost identical results.

From numerous microscopical examinations of the timber, and from the experiments I have described, I consider that I am justified in drawing the following conclusions *re naphthaline:*

1st. That supposing, for the sake of argument, naphthaline, possesses no great antiseptic power, nevertheless, it acts beneficially by clogging up the pores of the wood, forming a more or less solid magma with the coagulated albumen. In this way it assists the physical part of the creosoting process, upon which the preservation of the timber materially depends.

2d. That although a certain quantity of naphthaline would undoubtedly be volatilized by a tropical heat, nevertheless that the loss would practically be limited to the *surface* of the timber, and would be complete a day or two after exposure, the naphthaline in the deeper parts of the wood remaining fixed by incorporation with the albumen coagulated by the action of the tar acids.

3d. That inasmuch as the naphthaline cannot injure the action of the tar acids, or other constituents of the creosote, and is itself a positive benefit to the process, there is not only no object in requiring that the oil used for creosoting should be free from naphthaline, but that it would be unadvisable to demand such freedom.

There are many other facts that, in my judgment, corroborate the views I have expressed. Thus, I am given to understand that, during the twelve years after the process of creosoting was first introduced into India, the whole of the

sleepers were prepared with heavy London creosote (that is, a creosote highly charged with naphthaline), with the occasional admixture of a small quantity of country oil for the purpose of dilution.

It is perfectly certain, further, that it was on account of the good results obtained that creosoting became a process of acknowledged utility.

So far as I can learn, it was not until the country oils became more extensively used, that any complaints respecting the inefficiency of the process arose. From independent inquiries, I think there is the strongest possible reason to believe that the sleepers that proved unsatisfactory, had been prepared with country, and not with London oil.

Heavy Oils.

Nothing has impressed me more strongly in the course of these inquiries than the value of the heavy oils present in the creosote, that is, of the oils that do not distil over under 600° Fahrenheit. Of a certain antiseptic power, and very difficult of volatilization, they are, I believe, bodies of great value in the oil employed in the creosoting process.

I have carefully examined numerous samples of the creosote supplied by your Company, and I give herewith the analysis of eighteen samples. (Analysis omitted.)

After a very careful consideration of the conditions necessary to insure the successful creosoting of timber, it appears to me that the following points need special attention:—

1. That the timber should be well dried, so that the pores of the wood may be completely pervious.

2. That the creosote should be of a heavy, rather than of a light description, *i. e.*, that it should contain oils which are given off at high temperatures, together with other matters that become solid within the timber after the creosote has been allowed to cool to a normal temperature.

3. That as much creosote should be put into the timber, as the timber can possibly be made to absorb.

Taking into consideration the whole body of the evidence now before me, and which I have submitted to you in this report in part only, I am of opinion that no oil could be better suited than your own for the purpose of creosoting timber, and I would suggest the following as a specification for creosote that would, in my judgment, insure to engineers and others interested in the process, the best possible results:—

1. That the creosote should be completely liquid at a temperature of 100° Fahrenheit, no deposit afterwards taking place until the oil registers a temperature of 93° Fahrenheit.

2. That the creosote shall contain at least twenty-five per cent. of constituents that do not distil over at a temperature of 600° Fahrenheit.

3. That, tested by the process hereafter to be described, the creosote shall yield a total of 8 per cent. of tar acids.

There are certain details connected with this specification to which I desire to draw attention.

1. The omission of any clause specifying the specific gravity of the creosote to be used. I have done this advisedly, because of the extreme difficulty in taking the gravity of creosote at normal temperatures with the 1,000 grain bottle, and the practical uselessness in my judgment of employing a hydrometer for the purpose. If it be considered necessary to introduce a specific gravity clause, I would suggest that the gravity be between 1,040 and 1,065, water being 1,000. I am of opinion, however, that for practical purposes a specific gravity clause is altogether unnecessary.

2. Believing strongly as I do in the value of those constituents of the oil that are the most difficult to volatilize, I have deemed it right to suggest a clause to the effect that the creosote shall contain at least 25 per cent, of matters that distil over above 600° Fahrenheit.

3. I have made a large number of experiments as to the best method by which the estimation of the tar acids may be determined.

I note—

(*a*) That very slight differences in the strength of the solutions used, and in methods of manipulation, considerably influence the results obtained. I therefore deem it necessary that as a part of the specification, the process to be employed for estimating the acids should be exactly stated.

(*b*) I have failed to discover any easy method of separating the carbolic from

the other tar acids. I have tried for this purpose numerous experiments, but with such unsatisfactory results, that I have decided to recommend that the total quantity of tar acids only should be stated. Further, the fact that as preservatives one kind of tar acid is, so far as we know, as good as any other, renders a further separation of the acids in my judgment unnecessary. My analysis of samples will show that in fixing not less than 8 per cent. of total tar acids, we obtain a fair index of the purity and genuineness of the creosote.

Dr. Tidy's Specification for Creosote.

1. That the creosote shall be completely liquid at a temperature of 100° Fahrenheit, no deposit afterwards taking place until the oil registers a temperature of 93° Fahrenheit.

2. That the creosote shall contain at least 25 per cent. of constituents that do not distil over a temperature of 600° Fahrenheit.

3. That, tested by the process hereafter to be described, the creosote shall yield a total of 8 per cent. of tar acids.

Process to be Adopted for Determining the Coal Tar Acids.

1. 100 c. c. of the well mixed creosote is to be distilled at a temperature of 600° Fahrenheit until no further distillate comes over. The distillate so obtained, is to be mixed and well shaken in a stoppered flask with 30 c. c. of a solution of caustic soda, having a specific gravity of 1,200, water being 1,000. The mixture is then to be heated. This done, the stopper is to be replaced in the flask, and the hot mixture again shaken vigorously for at least a minute.

The contents of the flask are now to be poured into a separating funnel and the soda solution drawn off. The creosote is to be heated a second and a third time in a similar manner with the caustic soda solution, except that only 20 c. c. of the soda solution shall be used for the second and third extractions, instead of 30 c. c., as in the first extraction.

2. The three soda solutions are now to be mixed together. *When cold* any particles of creosote are to be got rid of by means of a separating funnel. This done, the solution is to be thoroughly boiled in order to expel the last traces of creosote present in the solution. The mixture is then to be allowed to cool. When cold, dilute sulphuric acid (1 of acid to 3 of water) is to be added (about 35 c. c. will be required) until the solution becomes slightly acid to litmus.

The whole is then to be poured into a separating funnel, and allowed to stand until perfectly cold, and the tar acids well separated.

3. The tar acids are now to be dissolved in 20 c. c. of the caustic soda solution (specific gravity, 1,200), and 10 c. c. of water. The mixture is then to be boiled and filtered through a funnel fitted with a plug of asbestos. The asbestos plug is to be washed with not more than 5 c. c. of boiling water. The solution is to be allowed to cool *perfectly* in a 100 c. c. measure. It is then to be rendered slightly acid with dilute sulphuric acid (1 to 3), (10 c. c. will probably be found sufficient for this purpose).

The whole is again allowed to stand *for two hours* until *perfectly cold*, when the percentage of the tar acids is to be read off.

Process to be Adopted in Estimating the Quantity of Distillate.

The operation is to be conducted in a retort (fitted with a thermometer) immersed in an oil or hot air bath. The heat at first is to be low, and the temperature gradually raised to 600° Fahrenheit, and continued until no further matters distil over.

Fifty years ago the construction of the first French railway—that from Paris to St. Germain—was officially sanctioned. The late Emile Pereire undertook to make this line of 18 kilometers at his own cost and risk. The requisite capital of 6,000,000 francs was not easy to raise, but the difficulties were surmounted when Pereire won over the Rothschilds and Sampson Davillers. The line was opened the 27th of August, 1837, and became the nucleus of the western system. France has now 31,000 kilometers of railways, conveying 180,000,000 passengers a year, and the gross receipts are 1,150,000,000 francs. Two hundred and twenty-three thousand persons are employed on these railways, and the state derives a revenue of 83,000,000 francs from them.

THE WATT AND HORSE-POWER.

By W. H. PREECE.

From "The Engineer."

The most useful practical unit in use among electricians that has been derived from the CGS system of absolute measurement is probably the Watt, or the rate of doing work when a current of one ampere is maintained through a resistance of one ohm. The work done by an electric current is thus brought into intimate relation with all other kinds of work. The common gravitation unit rate of working is the horse-power, which is 550 foot-pounds per second. The absolute CGS unit is the erg per second, or the work done in one second in overcoming a force of one dyne through a distance of one centimeter. In any electrical measurement the electromotive force E in volts multiplied by the current C in amperes gives a product EC equivalent to so many 10^7 ergs per second, which is the Watt. There are 746 watts in a horse-power, and hence EC is reduced to horse-power when it is divided by 746. The rate at which electrical energy is developed or expended in any part of any circuit is given in watts when we multiply the number of volts by the number of amperes. Strange mistakes are made in confusing the unit of power with the unit of work. No less an authority than Professor Adams, F. R. S., in his inaugural address as President of the Telegraph Engineers and Electricians, spoke of the watt as the unit of work, and gave its value in kilogrammeters as kilogrammeters$=$watts$\times$.10192. It is evident that as the watt is the unit of power, it is equal to .10192 kilogrammeter per second, or, in other words, work done at the rate of a kilogrammeter per second $=9.81$ watts$=g$ watts.

The horse-power as a unit has all the defects of an arbitrary unscientific standard. It involves the use of coefficients, and it is not connected directly with the absolute system of measurement. It differs in different parts of the world, and its name is misleading. It could be changed both in value and name without any inconvenience except to those who are familiar with the existing coefficients and formulæ. If its value were raised 34 per cent. it would become the kilowatt, and be connected directly with the CGS system. It would thus become scientific, and diminish the use of coefficients. Even the present name could be retained and its value altered from 746 to 1,000 watts, or from 33,000 foot-pounds to 44,233 foot-pounds per minute, without any serious inconvenience. Existing numbers expressing horse-power would simply have to be multiplied by .746 to bring them to the value of the new unit, or numbers on the new unit would have to be multiplied by 1.34 to express their value in the old system.

I cannot help thinking that the good work of the British Association Committee will not be complete until the CGS system is authoritatively applied to work and power. The following table brings together nearly, if not all, the units in common use:

Horse-power	=	33,000 foot-pounds per min.
"	=	550 foot-pounds per sec.
"	=	746×10^7 ergs per sec.
"	=	7,460 megergs per sec.
"	=	75.9 kilogram'rs per sec.
"	=	1.01385 force de cheval.
"	=	746 watts.
Force de cheval	=	75 kilogram'rs per sec.
"	=	542.48 foot-pounds.
"	=	0.9863 horse-power.
"	=	736 watts.
Watt	=	.0013405 horse-power.
"	=	10^7 ergs.
"	=	10 megergs.
"	=	$\frac{98.1}{1}$ kilogram'rs per sec.
"	=	.1029 kilogram'rs per sec.
B. of T. unit	=	1,000 watts per hour.
C. G. S. unit	=	erg per second.
Megerg	=	10^6 ergs per second.
Watt	=	10^7 ergs per second.

According to Professor Dewar, a standard sperm candle develops 240,000 foot-pounds per hour, or 4,000 foot-pounds per minute. Now, since a watt is equivalent to 44.2 foot-pounds per minute, it follows that a standard candle develops

90 watts per minute. Again, according to the same authority, 5 cubic feet of coal gas in London develops 2,500,000 foot-pounds per hour, or 41,666 foot-pounds per minute, or 2,976 foot-pounds per candle per minute, which is equivalent to 67 watts per minute. A good glow lamp absorbs 2.5 watts per candle per minute. Hence a glow lamp has an economy in energy of about $\frac{1}{30}$ of a gas-light and $\frac{1}{40}$ of a standard sperm candle. A man working very hard expends about 100 watts per minute; hence, if a man-power is equal to, say, 1 standard candle, it is equal to 1.34 gas candles and to 36 electric candles! What a field for economy in lighting, and how essential it is that gas should be applied to the production of power rather than to the production of light.

TESTS OF BITUMINOUS COALS FOR STEAM MAKING.

By JOHN W. HILL, M. E.

During March of the present year, the writer, under direction of the Cincinnati Water Department, conducted a series of tests of several of the best known brands of bituminous coal, in the Cincinnati market. The results of which, in view of the large quantities of coal burned in each trial, and of the unusual length of trial and number of observations, are calculated to be of value to engineers interested in steam making or boiler performance, who may have occasion to employ the same or equivalent kinds of coal.

The purpose of the tests was to establish a standard of comparison for the determination of the relative money value to the Water Department of such coals as were, or might be in the future, available in large quantities at moderate cost for steam purposes.

Four kinds of the best known and most available coals were tried in the following order: Pittsburgh (Youghiogheny) No. 2, lump, furnished by W. H. Brown's Sons, Cincinnati, O.; Kanawha "Winifrede" mines, lump, furnished by East End Coal Elevator Co., Cincinnati, O.; Kanawha "Campbell's Creek" mines, lump, furnished by the Campbell's Creek Coal Elevator Co., Cincinnati; "Peach Orchard" (Kentucky) mines, lump, furnished by Daniel Stone & Co., Cincinnati.

Each of these coals were furnished by the dealers from their local stocks, without previous intimation that it was wanted for test purposes, and the several results may be accepted as fair averages for ordinary market coal of the different brands, under similar conditions of use.

Each kind of coal was burned continuously under the boilers for forty-eight hours, the firemen changing watch every six hours.

In order that the conditions might be as nearly alike as possible for the several kinds of coal, the same boilers, supplying steam to the same engines, at same pressures, and nearly similar temperatures of feed water, and the same firemen were used.

Battery of boilers No. 9 were selected for the tests; these consist of four return-flue boilers of the following dimensions:

Length of shells	26.66 ft.
Diameter ,,	52.75 in.
Flues (each boiler), 2 11, 2 11.75, 1 15.5 in. diam.	
Grate........4-ft. bars, width of grate	19.5 ft.
Heating surface (4 boilers)	2230. sq. ft.
Grate " "	78. sq. ft.
Chimney (sheet-iron).....diameter	5. ft.
" height from grate	91.5 ft.
Cross section of flues	16.544 sq. ft.
" " chimney	19.635 sq. ft.
Ratio heating to grate surface	28.59
Ratio grate surface to cross section of chimney	3.9725
Ratio grate surface to cross section of flues	4.7147
Distance from grate to boiler.....front,	22. in.
" " " .....back,	28. in.
Distance from floor to top of uptake	16.5 ft.
Distance from grate to floor of ash pit	29 in.

The bridge wall of these boilers is built close up under the shells, with an opening (one under each boiler) through it, 26 in. wide by 26 in. high, with an

arched top. The length of this opening, or passage for the products of combustion, through the bridge wall is 5.5 feet at the grate level, and 3.5 feet under the arched top. This perforated bridge wall was introduced by a former engineer of the works as a means of preventing smoke with soft coal, for which purpose it is an admirable failure.

Omitting the peculiar construction of the bridge wall, which, in the judgment of the writer, was not calculated to improve the economy of the setting, there was nothing in the furnace or boilers calculated to vary the economy of performance from that of the usual return-flue boiler with the same quality of fuel.*

The tests were made, however, for purposes of comparison, and not for absolute values of the coals burned, and, as previously stated, the conditions for the several coals were as nearly the same as it was possible to obtain.

The steam from the boilers was consumed by engines No. 7 or 8, or by both of them, as they were run during the trials.

The coal fired was weighed into the boiler-room in charges or barrow-loads of 300 pounds, and checked by the regular coal-weigher and by an assistant appointed for the purpose of the trials.

The boilers, according to the condition of the feed-water used, are usually operated without stoppage or banking of fires for two or three weeks, night and day continuously, and had been in service for several days previous to the tests.

At the end of each regular watch of six hours, or at 6 A. M., 12 M., 6 P. M. and 12 midnight, the fires were cleaned jointly by the fireman going off watch and the fireman coming on; and the ash, clinker, and unburnt coal were drawn from the ash pits and dumped into the gallery under the boiler-room floor.

The coal burned by each fireman during his watch of six hours, was separately charged up to the boilers under the title of the fireman on duty.

During the tests of coal, the usual method of changing the firemen, cleaning the fires and ashpits, and charging the coal to the watch was observed, and in addition thereto the ash, clinker and unburnt coal were weighed up after the firemen were changed at the end of a watch.

The water supplied to the boilers was drawn from the main into a tight tank of about sixty (60) cubic feet capacity, mounted upon a Howe dormant scale, in which it was weighed in net charges of (nominally) 3,200 pounds each, and discharged into a second tank of about fifty-four and one-half (54½) cubic feet capacity, from which it was drawn by the boiler feed pump. The suction pipe of the boiler feed pump connected with the second water-tank and the discharge pipe of the boiler feed pump, through the feed-water heater, and thence to the boilers, were known to be tight, and all connections with these pipes were either closed by tight valves or cut off entirely during the trial, to prevent any water going to the boilers or pump, save that weighed from the mains, and similarly, to prevent any of the weighed water being diverted in transit to the boilers. The scale for weighing the water was new and very sensitive to slight changes of weight.

Hourly tests for the quality of steam were made during the trials. The Howe platform scale used for weighing the water and condensation, was new and extremely sensitive to change of load and graduated to weigh to eighths of a pound. The thermometer used for calorimeter purposes was graduated to single degrees, with large divisions of the scale, and could easily be read to fifths of a degree.

The average initial and final readings of this thermometer for each trial, were corrected to agree with the corresponding readings of a Green U. S. Signal Service thermometer, to obtain the true observed temperatures.

Readings of steam-gauge were taken regularly every fifteen minutes.

* Mr. Chas. Hermany, in 1872, tested No. 6 boilers of same general pattern, containing 5 boilers 28 feet long by 72 inches diameter, with 8 9-inch flues each, aggregating 4,346 square feet heating surface, and 133.76 square feet grate surface, which, with Pittsburg Second Pool stock coal, gave an evaporation from and at 212 F. of 10.12 pounds of steam per pound of coal.

Messrs. Hill, Moore, and Ahrens, as a committee of investigation, in 1879, tested a battery of return-flue boilers, at Hunt Street Pumping Station, Cincinnati, containing 2 boilers, each 24 feet long by 48 inches diameter, with 2 10-inch and 4 8-inch flues each, aggregating 1082.98 square feet of heating surface and 19.04 square feet of grate surface, with Pittsburg Second Pool picked coal, giving an evaporation from and at 212 F. of 10.627 pounds of steam per pound of coal.

Readings of water level in the boilers, temperatures of feed-water to boilers, air in boiler room and external atmosphere were taken regularly every half-hour during the trials.

The barometer was read hourly. The temperature of water in the weighing tank was read for each tank charged to the boilers.

Attempts were made to take the temperature of the waste gases, by mercurial thermometers, and then by pyrometer, but owing to an occasional temperature beyond the range of these instruments, a connected record could not be obtained, and occasional tests were made with rods of iron inserted at the base of chimney, with the results given in the table of data from trial of Winifrede coal.

In the following tables are given the general observed and calculated data for the several trials.

Experiment with "Pittsburg" Lump Coal.

Trial began at 6 P. M. March 16th, and ended at 6 P. M. March 18th.

Duration	48	hours.
Firemen: Shea	18	"
Firemen: Friel	18	"
Firemen: Dowd	12	"
Steam pressure (observed) mean	90.125	pounds.
" (corrected) "	84.985	"
Water level, mean	4.423	inches.
Feed water temperature (observed)	181.709	Fahr.
" " (corrected)	182.85	"
Barometer	29.584	inches.
Air, boiler room, mean temperature	41.013	Fahr.
" external "	24.747	Fahr.
Water in tanks "	40.181	"

Totals.

Water pumped into boilers	799,438	pounds.
Coal burned	77,400	"
Ash, clinker and unburned coal from ashpits	4,619.5	"

Calorimeter.

Water heated (average each test)	200.	pounds.
Steam condensed (average each test)	10.5625	"
Initial temperature (observed)	54.698	Fahr.
Initial temperature (corrected)	54.7021	"
Final " (observed)	111.375	"
" " (corrected)	111.5547	"
Range " (absolute)	56.8526	"
Heat units per pound of steam	1,188.0537	
Thermal value of steam at corrected pressure (per pound)	1,213.78	
Difference	25.7263	
Latent heat of steam at corrected pressure (per pound)	883.08	
Efficiency of the steam	0.9788	
Water entrained in the steam	2.913	per cent.

Economic Results.

Water to boilers per pound of coal	10.264	pounds.
Steam per pound of coal from temperature of feed	9.965	"
Steam per pound of coal from and at 212° Fahr	10.6386	"
Steam per pound of combustible from and at 212° Fahr	11.314	"

Capacity Results.

Average evaporation per hour	16,068.56	pounds.
" coal burned "	1,612.50	"
Steam per hour per square foot of heating surface from temperature of feed	7.206	"
Coal per hour per square foot of grate surface	20.673	"

Experiment with "Winifrede" Lump Coal.

Trial began at 6 P. M. March 18th, and ended at 6 P. M. March 20th.

Duration	48 hours.	
Firemen — Dowd	18 hours.	
Firemen — Shea	18 hours.	
Firemen — Friel	12 hours.	
Steam pressure (observed) mean	90.148	pounds.
" (corrected) "	85.008	"
Water level, mean	4.513	inches.
Feed water temperature (observed)	179.782	Fahr.
" " (corrected)	180.881	"
Barometer, mean	29.384	inches.
Air, boiler room, mean temperature	38.505	Fahr.
Air, external, "	23.552	"
Water in tank, "	40.705	"
Waste gases, "	580.300	"

Totals.

Water pumped into boilers	791,119	pounds.
Coal burned	78,900	"
Ash, clinker and unburned coal from ashpits	4,885	"

Calorimeter.

Water heated (average each test)	200.	pounds.
Steam condensed (average each test)	10.835	"
Initial temperature (observed)	53.15	Fahr.
" (corrected)	53.1532	"
Final temperature (observed)	110.872	"
" (corrected)	111.0485	"
Range temperature, absolute	57.8953	"
Heat units per pound of steam	1,179.720	
Thermal value of steam at corrected pressure (per pound)	1,213.795	
Difference	34.075	
Latent heat of steam at corrected pressure (per pound)	883.049	
Efficiency of the steam	0.917	
Water entrained in the steam	3.859	per cent.

Economic Results.

Water to boilers, per pound of coal	10.0268	pounds.
Steam per pound of coal from temperature of feed	9.6398	"
" " " from and at 212° Fahr	10.3107	"
" " of combustible from and at 212° Fahr	10.992	"

Capacity Results.

Average evaporation per hour	15,845.421	pounds.
" coal burned "	1,643.75	"
Steam per hour, per square foot of heating surface, from temperature of feed	7.105	"
Coal per hour, per square foot of grate surface	21.737	"

Experiment with "Campbell's Creek" Lump Coal.

Trial began at 6 P. M. March 20, and ended at 6 P. M. March 22.

Duration	48 hours.	
Firemen — Friel	18 "	
Firemen — Dowd	18 "	
Firemen — Shea	12 "	
Steam pressure (observed) mean	90.568	pounds.
" (corrected) "	85.428	"
Water level, mean	4.302	inches.
Feed water temperature (observed)	168.219	Fahr.
" " (corrected)	169.085	"
Barometer, mean	29.553	inches.
Air boiler-room, mean temperature	35.638	Fahr.
Air external "	25.597	"
Water in tank "	38.921	"

TOTALS.

Water pumped into boilers	758,675	pounds.
Coal burned	77,700	"
Ash, clinker, and unburned coal from ashpits	5,390	"

CALORIMETER.

Water heated (average each test)	200.	pounds.
Steam condensed " "	10.655	"
Initial temperature (observed)	49.628	Fahr.
" (corrected)	49.6297	"
Final temperature (observed)	106.75	"
" (corrected)	106.9009	"
Range temperature (absolute)	57.2712	"
Heat units, per pound of steam	1181.9117	
Thermal value of steam at corrected pressure, per pound	1213.85	
Difference	31.938	
Latent heat of steam at corrected pressure, per pound	882.91	
Efficiency of the steam	0.9737	
Water entrained in the steam	3.617	per cent.

ECONOMIC RESULTS.

Water to boilers, per pound of coal	9.7642	pounds.
Steam per pound of coal from temperature of feed	9.411	"
Steam per pound of coal from and at 212° Fahr.	10.1808	"
Steam per pound of combustible from and at 212° Fahr.	10.94	"

CAPACITY RESULTS.

Average evaporation per hour	15,234.056	pounds.
" coal burned "	1,618.75	"
Steam per hour per square foot of heating surface, from temperature of feed	6.8314	"
Coal per hour per square foot of grate surface	20.7532	"

EXPERIMENT WITH "PEACH ORCHARD" LUMP COAL.

Trial began at 6 P. M. March 22, and ended 6 P. M. March 24.

Duration	48 hours.	
Firemen: Shea	18 "	
Firemen: Friel	18 "	
Firemen: Dowd	12 "	
Steam pressure (observed) mean	90.771	pounds.
" (corrected) "	85.631	"
Water level	4.295	inches.
Feed water temperature (observed)	170.916	Fahr.
" " (corrected)	171.833	"
Barometer, mean	29.706	inches.
Air boiler-room, mean temperature	38.843	Fahr.
Air external, "	29.742	"
Water in tank "	38.396	"

TOTALS.

Water pumped into boilers	744,425	pounds.
Coal burned	78,600	"
Ash, clinker, and unburned coal from ashpits	5,740	"

CALORIMETER.

Water heated (average each test)	200.	pounds.
Steam condensed (average each test)	10.824	"
Initial temperature (observed)	50.542	Fahr.
" (corrected)	50.544	"
Final temperature (observed)	108.054	"
" (corrected)	108.2127	"
Range temperature (absolute)	57.6687	"
Heat units per pound of steam	1,173.7837	
Thermal value of steam at corrected pressure	1,213.922	
Difference	40.138	
Latent heat of steam at corrected pressure	882.74	
Efficiency of the steam	0 9669	
Water entrained in the steam	4.547	per cent.

ECONOMIC RESULTS.

Water to boilers per pound of coal	9.4716 pounds.
Steam per pound of coal from temperature of feed	9.0409 "
Steam per pound of coal from and at 212° Fahr	9.756 "
Steam per pound of combustible from and at 212° Fahr	10.524 "

CAPACITY RESULTS.

Average evaporation per hour	14,804.47 pounds.
" coal burned "	1,687.5 "
Steam per hour per square foot of heating surface from temperature of feed	6.639 "
Coal per hour per square foot of grate surface	20.994 "

PERCENTAGE OF NON-COMBUSTIBLE.

Pittsburg Lump	5.9683
Winifrede	6.1914
Campbell's Creek	6.9369
Peach Orchard	7.3028

The relative values of the several coals based on performance of the coal, are shown by the following comparison calling "Pittsburg" coal 1000, then "Winifrede ranks 969, "Campbell's Creek" 957, and "Peach Orchard" 917. While the relative values based on performance of combustible, are:

"Pittsburg"	1000.
"Winifrede"	972.
"Campbell's Creek"	967.
"Peach Orchard"	930.

The comparison by combustible has no commercial value excepting the coals should be contracted for upon basis of combustible.

During the trials samples of each kind of coal burned were boxed, and subsequently sent to a competent chemist, who analyzed the same with the following results:

ANALYSES OF COAL.

Coals.	Pittsburgh.	Winifrede.	Campbell's Creek.	Peach Orchard.
Fixed carbon	59.88	57.21	53.24	52.83
Volatile matter	33.71	37.58	34.23	36.83
Moisture	1.87	1.93	2.15	4.60
Sulphur	0.44	0.56	0.72	0.76
Ash	4.10	2.72	9.66*	4.98
	100.00	100.00	100.00	100.00

In the appended tables is given a summary of the trials, in parallel columns, and the individual work of the firemen with the several coals, all of whom exhibited commendable skill in manipulating the coal and the boilers during the trials.

SUMMARY OF TRIALS.

Coal burned	Pittsburgh.	Winifrede.	Campbell's Creek.	Peach Orchard.
Dates of trial	Mar. 16–18	Mar. 18–20	Mar. 20–22	Mar. 22–24
Duration of trial, hours	48	48	48	48
Mean steam pressure	84.985	85.008	85.428	85.631
" temp. feed water	181.709	179.782	168.219	170.916

* The ash found for Campbell's Creek coal by analysis must be an error, from the fact that the non-combustible (6.9369 per cent. of original weights of coal burned on the grate) includes, in addition to ash and clinker (non-combustible) some combustible which filtered through the grates in charging coal and breaking the fires.

SUMMARY OF TRIALS (*Continued*).

Coal burned	Pittsburgh.	Winifrede.	Campbell's Creek.	Peach Orchard.
Dates of trial	Mar. 16–18.	Mar. 18–20.	Mar. 20–22.	Mar. 22–24.
Mean temp. atmosphere boiler-room	41.018	38.505	35.638	38.843
" barometer	29.534	29.384	29.553	29.706
Total coal burned, pounds	77,400.	78,900.	77,700.	78,600.
Coal per hour "	1,612.5	1,643.75	1,618.75	1,637.5
" " square foot of grate surface per hour, pounds	20.673	21.737	20.753	20.994
Steam per square foot of heating surface per hour	7.206	7.105	6.831	6.639
Water entrained in steam, per cent	2.918	3.859	3.617	4.547
Steam per pound of coal from and at 212° Fahr.	10.638	10.310	10.181	9.756
Steam per pound of combustible from and at 212° Fahr.	11.314	10.992	10.940	10.524
Relative value based on coal	1000.	969.	957.	917.
" " " combustible	1000.	972.	967.	930.
Percentage of non-combustible	5.968	6.191	6.937	7.303

GENERAL AVERAGES.

PITTSBURGH LUMP COAL.

Firemen.	Water.	Coal.	Steam from and at 212° Fahr. per pound of coal.	Percentage of Ash.
	Pounds.	Pounds.	Pounds.	
Shea	312,037.99	29,100	11.1054	6.6579
Friel	293,754.06	28,350	10.7421	5.8305
Dowd	188,645.95	19,950	10.3229	5.1783

WINIFREDE LUMP COAL.

Firemen.	Water.	Coal.	Steam from and at 212° Fahr. per pound of coal.	Percentage of Ash.
	Pounds.	Pounds.	Pounds.	
Dowd	287,971.33	29,400	10.0649	5.6383
Shea	301,844.28	30,000	10.3372	6.4358
Friel	201,303.39	19,500	10.6167	6.6035

CAMPBELL'S CREEK COAL.

Firemen.	Water.	Coal.	Steam from and at 212° Fahr. per pound of coal.	Percentage of Ash.
	Pounds.	Pounds.	Pounds.	
Friel	276,890	28.800	10.0537	6.3705
Dowd	283,125	29,100	10.0387	6.5410
Shea	198,660	19,800	10.4523	8.3756

PEACH ORCHARD COAL.

Firemen.	Water.	Coal.	Steam from and at 212° Fahr. per pound of coal.	Percentage of Ash.
	Pounds.	Pounds..	Pounds.	
Shea.................	289,578.97	30,300	9.8493	8.3733
Friel................	264,305.88	28,800	9.4599	6.5247
Dowd................	190.540.15	19,500	10.0566	6.7964

THE SLIDE RULE.

By C. V. BOYS.

It is a perpetual source of amazement to those who are familiar with this instrument that its use is not almost universal. People of every class have to make simple calculations, while those engaged in scientific work, in designing apparatus, or in invention, perpetually cover sheets of paper with figures, all of which trouble, and the loss of time which it involves, might be saved by the intelligent use of a good slide rule, and yet, for reasons difficult to find out, the habitual use of this instrument is limited to a very small proportion of the calculating community.

Most people know that the scales are logarithmically divided—that is, that the distance between the divisions marked 1 and 10 being in imagination divided into 10,000 parts, the division marked 2 is at the 3010th of these parts, the division marked 3 is at the 4771st of these parts, and so on, 3010 being the log. of 2, 4771 the log. of 3, and so on, and further, that the spaces between these whole numbers are similarly divided into fractional parts, thus 1.1 is at the 414th of the imaginary parts, and 1.01 at the 43d of these parts, 414 and 43 being the logs. of 1.1 and 1.01. This is very generally known, but it is more generally believed that to use the rule involves so much thought and anxiety that it is far simpler to work out results in the usual way, or at any rate that the rule can only be of any real assistance when a great number of similar calculations have to be made; and further, that as the results to be obtained are not absolutely correct, that as an extreme error of 1, $\frac{1}{10}$, or $\frac{1}{100}$ per cent. is possible, according to the nature of the instrument, it is not really to be trusted. These objections are easily answered. As soon as the slight difficulty of reading the rule has been overcome—a difficulty due to the fact that in ascending the scale the divisions become closer, so that if there is room for ten subdivisions between 10 and 11, there are only five between 20 and 21, and two between 40 and 41—a difficulty which once overcome never recurs—then the simpler calculations, such as multiplication, division and simple proportion can at all times, without an effort or a thought, be instantly performed, while those involving proportions in which some of the terms are squares, cubes, roots, sines, or tangents can, after a moment's reflection, be as easily completed, so that even in the case of single operations time is saved. It is true when many calculations of the same kind present themselves, especially if some of the terms in the series are identical, that the use of the rule is specially advantageous; but in any case mental labor and time are saved.

As to the probable accuracy of results obtained by the use of the rule, they are in general superior to the accuracy with which the figures which require reduction have been determined, or, if this is not the case, they are in general so nearly correct that the error is of no consequence. For instance, if the marks obtained by several examinees are to be reduced to correspond to a total of 100, the commonest rule, which gives an accuracy of $\frac{1}{300}$ part, is sufficiently good, for the nearest whole number only, and

the right order are all that are needed. It would be absurd to doubt the accuracy of the instrument because it cannot be trusted to give figures correct to one part in a thousand. Or, again, if the weight of a piece of metal has to be determined from its dimensions, a good rule, trustworthy to 1 part in 1,000, will in almost every case be more than good enough; for, even if the specific gravity of the material be known so truly, it is not often that the piece can be made so near the specified size that the discrepancy which may ultimately be observed will be due more to the error of the rule than to the inaccuracy of construction. In such a case it would be as absurd to discard the rule as untrustworthy as it is to use 7-figure logarithms for the calculations of an ordinary chemical analysis. There are cases, of course, where observations can be made with a degree of accuracy beyond that which is obtainable by any rule — for instance, determinations of mass, length, angles, and time can all be made with extraordinary precision. Where, then, uncertainty is not introduced by observations of another kind, where the entire precision to be obtained in any such observations may be expected in the result, as, for instance, in the determination of the refractive index of the glass of a prism, in such cases the slide rule is unsuitable, and tables of logarithms furnish the most obvious means of making the calculations. Or, again, when pounds, shillings and pence are involved, a result correct to the nearest farthing is generally desired to make accounts come right, and so, unless the sums dealt with are moderate, the slide rule is again unsuitable. However, the calculation of interest furnishes a good example of proper and improper use of the rule in making calculations. If it is required to find what a certain sum (s) will be worth at the end of a year, at so much (r) per cent., the result might be found from the proportion $100 : 100 + r :: s : x$. Here, the amount x would be determined with an accuracy of, say, $\frac{1}{1000}$ part. so that if £1,000 were involved, an error of £1 might arise. This is an improper use of the rule. A greater degree of accuracy would be obtained by the proportion $100 : r :: s$: the increase of s. Here the interest is found to the same proportionate accuracy, and so, in such a case the greatest possible error could only be one shilling, if the rate is five per cent. This example, though obvious, is given because it corresponds exactly with cases that arise in the laboratory, where the rule, if used properly, is of service, but, if improperly, is useless.

Calculations involving only the simple arithmetical rules, when extreme accuracy is required, are best performed by the help of a table of logarithms, or with an arithmometer; in fact, with an arithmometer a far greater degree of accuracy can be reached than with ordinary 7-figure logarithms, and though they are also suitable for calculations in which only three or four significant figures are required, their great size and expense compare unfavorably with the portability and cheapness of the rule, and, moreover, trigonometrical and logarithmic functions cannot be found with them. These machines are shown at the Inventions Exhibition by Tate and Edmonson, and are worth examining. There is another calculating machine close to Tate's, by which the interest on any sum at any rate per cent. for any time may be found to the nearest halfpenny in an incredibly short space of time, worthy of the attention of those who have to calculate interest. But, to return to the slide rule, it is astonishing that an instrument like Gravet's, 10 inches long, only, with which all calculations, arithmetical, trigonometrical, and logarithmic, can be worked out so easily and with an accuracy of from $\frac{1}{500}$ to $\frac{1}{1000}$, according to the nature of the calculation, should be so little used.

This is not the place to give instructions for using the rule, but an outline of the method is necessary to make it possible to compare the different makes, many of which are shown at the Inventions Exhibition.

With two similar scales of equal parts, as inches divided into tenths, or centimeters divided into millimeters, it is possible to add numbers, or, conversely, to subtract numbers; thus, if the zero of one scale is placed opposite, say, 6.5 of the other, opposite every number n on the first will be found $n + 6.5$ on the second, and so addition or subtraction could be performed, but there would be no advantage in so adding or subtracting. In the same way the slide of the ordinary

slide rule is employed to add distances, but these distances do not correspond to the figures attached, but to the logarithms of those figures, and so the sum which is found by such an addition is not the sum of the figures apparently added, but their product. If the slide is placed at random, all the pairs of figures which are opposite to one another are in the same proportion, and the multipliers which will change either series into the other will be found on each scale opposite the divisions marked 1 on the other. It requires no great amount of memory to bear this in mind: however the slide may be set, those numbers which are opposite to one another are in the same proportion, *i. e.*, have a common quotient, which may be found opposite any of the divisions marked 1; and yet this is all that has to be remembered in multiplication, division, and simple proportion. The two top lines of a slide rule are generally identical, and they are used for these simple operations; they are generally distinguished by the letters A and B. In general, the bottom line of the slide, that is, the third altogether, is identical with the first two, and is labeled C. This arrangement is convenient, for it is possible to insert the slide upside down, in which case all numbers which are opposite one another on A and C have a common product, which may be found opposite any of the divisions marked 1. This furnishes the most ready mode of finding actual or approximate factors of numbers, and is of great use to those who have to calculate wheel-work; further, by the use of the inverted C line under the A line, any harmonical progression can at once be read, and any number of harmonic means can be inserted between two quantities. The fourth line is generally made different from the others, in that it is on double the scale, and it is then distinguished by the letter D. If the units of the C and D line are placed opposite one another, a table of squares and roots is formed, or, if in any other position, the squares of the numbers on D vary in the same proportion, as do the numbers that are opposite to them on C. It is in calculations made on the C and D lines that so much time is saved, for proportions in which some of the terms are squares or square roots can be worked out as quickly and as accurately as those in which simple numbers only are employed. If the slide is inverted so as to bring the B line opposite to the D line, then the square of any number on D$\times$ the number opposite to it on B is constant. This product may, of course, be found in B opposite 1 in D. Cube roots, among other things, may be found in this way.

These four lines are all that are generally found in a slide rule; occasionally others are added: thus a line on one-third of the scale of the D line (sometimes called an E line) will, with the D line, enable one to directly work proportions in which some of the terms are cubes or cube roots, but this is not often required. With the usual four lines all arithmetical processes, except addition and subtraction, can be performed. There are, however, rules in which on the back of the slide are scales in which the distances are log. sines or log. tangents of the angles marked, then, these lines being placed against an ordinary A line so that 90° on the line of sines, or 45° on the line of tangents is opposite 1 on the A line, a table of sines or tangents will be formed; and if the slide is placed in any other position, the sines or tangents of the angles denoted by any divisions on either of these special lines will vary in the same proportion as do the numbers which are opposite them on the A line. In those rules in which lines of sines and tangents are given, there is generally a scale of equal parts in which the length of the D line is divided into 500 or 1,000 parts. If this is placed opposite the D line, with the ends of the two scales opposite one another, a table of logarithms will be seen; thus the logarithm of any number on the D line will be found opposite to it on the scale of equal parts.

Having pointed out the chief uses of a slide rule, it will be possible to describe the differences in construction in the several varieties. The most simple possible form is the original Gunter's scale to be found on any sector. With this and a pair of dividers calculations may be made, for if the dividers are set to the distance between any two numbers, any other pair of numbers which are found by the dividers to be the same distance apart will be in the same proportion, or have a common quotient just as a com-

mon difference would be found if a scale of equal parts were used. This, however, is troublesome; but if the same principle is applied to a scale in the circular form the result is much more convenient. In this case angular distance takes the place of linear distance, and a pair of arms which can be opened to any angle can be moved round, and every pair of numbers covered will bear to one another a constant proportion depending on the extent of the angle. This is the principle of some of Dixon's rules shown at the Inventions Exhibition, near the arithmometers. In the well-known pocket instrument, the calculating circle of Boucher, an instrument like a watch, one hand is fixed and one is movable, and the face is also movable. There is another instrument of the same kind, in which the scale is drawn on a helical line. Here the scale and one hand are movable, and there is one fixed hand. This, which is Professor Fuller's spiral rule, is made and exhibited by Stanley. Circular instruments are also made, in which scales slide over one another, which are in this respect like the straight rules. There is more advantage in the circular form than appears at first. In the straight rules the A and B lines are each double, the first and second halves are identical; this repetition of the scale is required in order that, however the slide may be placed, the part of each opposite to the other may contain at least a complete scale of numbers. In the circular form, however, the beginning and end of a single logarithmic scale meet, and so the scale itself is its own repetition both above and below. For this reason the openness of the divisors in a circular instrument is the same as in a straight rule, of which the length is six times, instead of three times, the diameter of the circular line.

Of the two types of instrument—one in which one slide works against another, generally straight, sometimes circular, and the other in which there is no slide but only a line divided logarithmically with a pair of hands, which type is always circular—which may be called respectively the slide and the index types, each has certain advantages. The slide form is preferable, in that each setting of the slide furnishes a complete table of pairs of related numbers, as, for instance, of any English and foreign measure, of squares and roots on any scale, such as diameters and areas of circles, or of sines or tangents on any scale, so that, without moving the slide, any number of results may be read off, whereas with instruments of the index type the scale must be moved under the hands, or the hands over the scale, for each result. On the other hand, index instruments are more convenient than the usual slide rules in working out long expressions of the form $\frac{a\times b\times c\times d}{e\times f\times g}$, in which any of the terms may be squares, cubes, sines, or tangents, for the terms are taken alternately from the numerator and denominator and set in order with the fixed and movable hand until all are worked off, when the answer is found under the fixed hand. There is no necessity to observe any result till the process is complete; on the other hand, with slide instruments, each result of the form $\frac{a\times b}{e}$, $\frac{a\times b\times c}{e\times f}$, etc., must be read and set before it can be operated upon by the next pair of factors. In Gravet's rules, however, this disadvantage of the straight form is removed by the addition of a crusor or sliding index, which in other ways is a great comfort.

All instruments of the index type suffer terribly from parallax, owing to the hands being above the face, so that they do not in practice give the accuracy that from the length of scale upon them might be expected.

This is especially the case in small instruments: for instance, Boucher's calculating circle, made in the form of a watch, is probably divided so accurately that on that score an error of one part in a thousand does not exist; yet, owing to parallax, the practical limit is about $\frac{1}{300}$. This instrument has, besides the ordinary line, one on a double and one on a treble scale for squares and cubes, a line of sines, and another of equal parts for logarithms.

The possible accuracy of any instrument depends upon the length of the scale included between 1 and 10, called the radius, and also upon the linear accuracy with which a setting or reading can be made; this is at least twice as great in slide as in index instruments. In order to obtain great accuracy, various means have been adopted whereby a great

length of scale is brought within a small compass. Among slide instruments are Professor Everett's "Universal Proportion Table," published by Longmans, Green & Co., and General Hannyngton's slide rule, made and exhibited at the Inventions Exhibition by Aston & Mauder. In these the slide is made in the gridiron form. In Everett's instrument there are twenty bars, the total length of which is about 13 ft.; a scale of equal parts is also printed, so that logarithms can be read with it. In both of these instruments only simple proportions can be effected, unless special grids, divided on a double scale or trigonometrically, are provided. Far the most ingenious of all devices for obtaining a great length of radius in a comparatively short space is due to Mr. Beauchamp Tower, whose name is well known in connection with the spherical engine. His instrument is a slide instrument consisting of two tapes running side by side over equal and independent rollers, but the tapes have a half twist in them, so that they have each only one surface, and one edge. In this instrument, made privately for his own use, each tape is about 12½ ft. long, and as both sides of the tape are used the radius is about 25 ft., and therefore, as far as openness of scale is concerned, it is equivalent to a straight rule 50 ft. long, while the instrument itself is only just over 6 ft. in length.

Slide rules of the index class can have a great length of scale more readily employed than others. Thus, Professor Fuller's helical instrument has its radius equal to 42½ feet, and is, in openness of scale, equivalent to a straight rule 85 feet long, while the box which contains it is only $17 \times 3\frac{3}{4} \times 3\frac{3}{4}$ inches, inside measure. Dixon exhibits a special rule with the scale extending over 10 concentric circles, but with this form a less degree of accuracy is attainable when using the inner than when using the outer circle. Thus, the inner circle is equivalent to a straight rule 30 feet long, and the outer to one 60 feet long. There is an outer circle equally and logarithmically divided to find logarithms. In another of Dixon's instruments, similar in size and form, there is the same outer circle for proportions and logarithms, and a series of inner circles divided so as to give sines, cosines, tangents, cotangents, secants, and cosecants. Each of these is on a board 14 inches square. Rules with very extended scales do not, in practice, give results with an accuracy which is proportional to their length, though the working accuracy is very much increased. They have this advantage, that they can be worked to their limit with ease, while with a well-divided pocket-rule the errors of construction are beyond the limits of vision, and so the calculator is apt to strain his eyes to get results as accurate as possible. For instance, results obtained by a good pocket-rule one foot long can be trusted to a thousandth part; at the same time, Prof. Everett's should be accurate to a thirteen-thousandth part, and Prof. Fuller's to an eighty-five thousandth part. In practice, a four and a ten-thousandth part are their limits. Again, instruments with very extended scales have only room for one line, so that simple proportions only and logarithms are all that can be directly obtained from them. For general use in the laboratory or elsewhere, where calculations of every kind have to be made, the straight form, on the whole, seems most convenient, because of its portability, the quickness with which it can be worked, the diversity of operations that it will directly accomplish, and the extraordinary accuracy in comparison with other forms of the results to be obtained. Far the best instruments of this type that the writer has yet seen are those made by Tavernier-Gravet, of Paris, already alluded to. They are different to those generally used in England, in that the line in the slide which works against the D line is itself a D line, so that squared proportions have to be performed by the aid of the cursor. This form has the further disadvantage that the inverted slide cannot be used for finding factors, which is a great loss; on the other hand, the two lower lines may be used for simple proportions, and they will give a double accuracy. On the whole, the original pattern with an A, B, C and D line seems preferable. Of the straight rules shown at the Inventions Exhibition, those made by Stanley exceed all the others in workmanship, and they are equal in this respect to the Gravet rule. Among them are rules for special purposes, as Hudson's scales and Ganga Ram's rules. Hudson's scales,

which are made in card, each having two slides, are a marvel of constructive skill. Dixon shows his "triple radius double-slide rule," with which very complex operations may be readily performed. Heath shows a slide rule for converting sidereal to mean solar time, or the reverse, correct to about .02 of a second, but this is not a slide rule proper, as the scales are not logarithmic.

There is entirely a different class of slide rule shown by Lieut. Thomson. In this there is, as usual, an A, B, and C line, but, instead of the D line, there is a "P" line, in which the distances, instead of being logarithmic, are logarithms of logarithms. By this instrument, fractional powers may be found as readily as simple products or quotients. It has, hower, this defect, that the scale converges so rapidly as the numbers ascend that high numbers can only be obtained with a proportionate accuracy far less than is possible with low numbers. It is one feature in the slide rule of ordinary construction that an error of reading of, say, $\frac{1}{100}$ of an inch will produce the same proportionate error in any part of the scale. This rule for involution is shown in the straight and circular form. It is right to mention that the same thing exactly was invented by the late Dr. Roget, and published by him in the *Phil. Trans.* of 1815.

No attempt has been made to give an account of every special form of rule that is made; those shown at the Exhibition, and some other well-known forms which well illustrate the different kinds of development, have been imperfectly described, and the general principles on which all depend sufficiently explained to make evident the advantages of each type of instrument.

REPORTS OF ENGINEERING SOCIETIES.

St. Louis Engineers' Club—St. Louis, Nov. 4, 1885.—A paper by Mr. C. W. Clark was read, entitled "Notes on the Influence of Inclination of the Limb and of the Axis of a Theodolite on the Measurement of Horizontal Angles," and discussed at length by Professor Johnson.

In the general discussion Mr. Hill gave the result of some tests of cement and sand bricks, mixed 4 of sand to 1 of cement. They were twenty-six days old, and averaged about 125 lbs. per square inch in compression; Mr. Russel gave an instance where common clay stood 135 lbs. per square inch in tension. Various other subjects were commented upon.

Engineers' Club of Philadelphia—Regular Meeting, October 17th, 1885.—This was the first meeting of the Club in its new house, No. 1,122 Girard Street, President J. J. de Kinder in the chair.

The Secretary presented, for Mr. P. F. Brendlinger, an illustrated description of a novel and cheap cement-testing machine, which can be built by an ordinary carpenter and blacksmith at a cost of less than twelve dollars, and is sufficiently accurate for practical purposes.

Mr. John T. Boyd presented an illustrated description of the "Coventry" locomotive boiler, which is probably the latest novelty in locomotive construction.

It was built at the Brooks Locomotive Works, Dunkirk, N. Y., and placed on one of their standard 17-in. × 24-in. engines, with 61-in. drivers. The economy of the boiler as a steam generator has not been made public, but while in service on the New York division of Pennsylvania Railroad it has proved to be almost absolutely free from smoke and cinder-discharging qualities. The boiler is of the straight-top return-tubular type, is made of Otis steel throughout, and is remarkable in having but two barrel sections, excluding the smoke box.

The stack is "behind, instead of before," and is located over the front end of what might be called the upper crown-sheet, which forms the bottom of the back combustion-chamber, which is directly over the fire-box, the stack itself rising from top of boiler between the cab and dome. The crown-sheets are self-sustained by long stay-bolts opposing the pressure in inside of boiler.

Access to the back combustion-chamber is had by means of a man-hole in the rear head of boiler, through which the exhaust nozzles and lift pipes are put in motion.

The lower or 2-in. tubes are arranged as in the ordinary locomotive, while the upper or 3-in tubes are grouped around the dry-pipe as it passes from the dome to the "T" head in smoke-box.

In order to get the exhaust steam from the cylinders into the stack, side pipes provided with expansion joints, after leaving the smoke-box, are placed outside of, but close to, the boiler jacket, and enter the back combustion chamber close to the base of the stack.

The throttle lever is turned "upside down," to get a pulling motion to open the throttle, and the rod is forked to pass around stack at its base, in order to reach the bridge-pipe at dome.

The Secretary presented, for Mr. Walter C. Brooke, a description of appliances for landing mine cars at the top of slope..

Mr. A. Marichal, visitor, exhibited and described an instrument for at once describing arcs of any radii, from a few inches to infinity, and for determining the radii of arcs already drawn. It operates upon the general principle that all angles inscribed in the same segment are equal, and it is adjusted or read by means of graduations.

Mr. T. M. Cleemann, on behalf of Mr. W. W. Evans, of New Rochelle, N. Y., presented a small box, made of white oak from Delaware River Bridge, at Trenton, N. J, built in 1808, replaced by an iron bridge in 1875; in constant service for 72 years, and as a railway bridge for 80 years.

The Secretary announced that a committee of the Civil Engineers' Club of Cleveland desired that this Club participate in joint action with their and other societies for the purpose "of obtaining the passage of new laws which shall provide for the better condition of Civil Engineers employed on Government works, other than military." Not being a business meeting, no action could be taken, but the matter was, by informal vote, referred to the Board of Directors.

The Secretary reported that the assessment of a considerable amount of customs duty upon books presented to us by the British Government, had been made the occasion for a determined fight for our heretofore disputed right to free entry. After unlimited affidavits, blanks, bills, bond, and other documents, including "Hospital dues" (on Abridgements of Specifications of British Patents!), in getting it there, the appeal to the Secretary of the Treasury met with prompt and fair consideration. We are now, officially, "a Society established for philosophical purposes, and entitled to the benefits conferred by the provision of law above quoted."

ENGINEERING NOTES.

The Construction of Factory Chimneys.—The *Chemiker Zeitung* has lately been devoting attention to this subject with special reference to the question whether decrease of height might not cause a saving in fuel without impairing general efficiency. Herr P. Huth records a case in which the erection of a new boiler in a relatively disused building necessitated (after an unsuccessful attempt to use it) the demolition of the old chimney, the dimensions of which were: height, 65.61 ft., lower diameter, 19.68 in., diameter of interior of chimney, 13.78 in. The entire length of the draught, including the flue, was about 98.42 ft. In the new chimney the entire length of draught, including the flue, was proposed to be about 95,14 ft., and the diameter at the narrowest square portion, 25.59 in.

Partially for experimental purposes, and partially with a view to economy, a trial was made of heating the boiler when the chimney was 39.37 ft. in height. Although the results were affected by the damp masonry, there was a distinct improvement perceptible as compared with the old chimney. At a height of 45.93 ft. the trials were still more satisfactory, and at 52.49 ft. all requirements were completely fulfilled, the smoke being absolutely white and sometimes scarcely noticeable, without any soot or flying ash. The heating of the boiler was excellent, and the consumption of coal 15 to 20 per cent. less than was the case with the old chimney. The top was then finished in the usual way without any further improvement or addition to the height.

From these facts Herr Huth deduces the argument that not only the height, but also the diameter, of a chimney in proportion to its height, deserves attention for economical and administrative reasons. High chimneys are, he considers, as a rule, too, narrow in proportion to their height, and hence do not draw well, or else waste fuel and cover the neighborhood with soot and flying ash. The effort to remedy these evils by still further increasing the height of the chimneys usually leads to their aggravation. Herr Huth suggests more detailed researches as likely to elucidate the subject further. In a later number of the *Chemiker Zeitung*, Herr Ramdohr, of Gotha, confirms the assertion that there is a dearth of exact information as to this point. He alludes to the opposite extremes of making chimneys decrease or increase in their internal diameter at the top as compared with the base. He recommends uniform diameter, and thinks this should be estimated rather fully in order that the heated gases in the center of the chimney may be, if possible, surrounded by a cooler stratum, which protects the brickwork to some extent from the heat

This uniformity of diameter can be obtained by cutting the bricks or by dividing the chimney into sections and ascertaining that no portion of these has less than the specified minimum diameter. This diameter can be estimated in proportion to the extent of the grate-surface of all the fire-places which the chimney serves, being about equal to the free grate-surfaces. This varies with different combustibles (brown or mineral coal, wood, etc.), from one-eighth to one-fourth of the entire grate-surface. The lower a chimney, the larger may be its cross-section.

The height, even when the boilers are small, should not be less than about 50 ft. For large steam appliances, when they are near the chimney, a height of 100 ft. to 120 ft. will usually suffice, provided the cross-section is suitable. When the fire-places are some distance from the chimney, the height of the latter would be about 160 ft. to 200 ft., the cross-section being modified on account of the cooling of the smoke gases during their passage.

As to the form of the cross-section of such chimneys, it is considered that a circular shape is preferable on account of its resistance to wind-pressure. An octangular form is considered a very suitable alternative, as only one special shape of brick is indispensable; and finally it is stated that the top of the chimney should not be called on to bear any but a very slight projection; the whole being carefully surmounted with iron round the top of the brickwork.

The Sukkur Bridge.—The Indus is already spanned at various points by bridges of considerable dimensions, which carry the Indian state railways over that river. The largest of these structures, however, will, as regards span, shortly be eclipsed by the bridge which is to be constructed over the Rohri Pass of the Indus at Sukkur, on the line of railway from Kurrachee and Attock. It has been designed on the cantilever principle by Mr. A. M. Rendel, M. Inst. C. E., and is 790 ft. clear span be-

tween the faces of the abutments and 820 ft. between the vertical pillars. The center lines of the main horizontal tie and the top of the large pillars and struts are 169 ft. above the bed plates. There is a girder of 200 ft. filling in the space between the ends of the cantilevers. The total weight of steel in the cantilevers will be about 3,200 tons, and this is exclusive of the 200-ft. girder. The main guys, which will have to hold back the whole of the structure, are 302 ft. long, and are connected to the anchors, which are constructed of steel plates, angles, and channel steel of large dimensions, built in masonry below the surface. The large pillars, which will stand on the abutments on bed plates 100 ft. apart center to center, and which incline inwards to a point meeting the guys 169 ft. above, will be 174 ft. long. Struts will spring from the same bed-plate as the large pillars, and incline at an angle of 35 degrees towards the center of the bridge, and also inwards towards the center line, meeting the horizontal tie, which is a continuation of the guys, at 169 ft. above the bed-plate, similar to the jib of a crane, and from the point of junction the inclined tie descends to the platform of the bridge. These struts will be 210 ft. long and 16 ft. square at the center. The booms, or compression members, diverge from the platform at a point about 40 ft. from the end, and convey the strains to the large bed-plates, from which the large pillars and struts rise. The platform for carrying the rails consists of two horizontal girders running from end to end, placed 18 ft. apart, centers, with cross girders every 8 ft., and on these longitudinal Z irons are placed on which is laid Westwood & Baillie's trough flooring, which gives great rigidity and strength to the bridge.

The steel is specified to be of a very high quality, and is to be made in very large scantling. For instance, the top plates of the anchors will be 20 ft. by 6 ft. by 1¼ in. thick, each weighing 2 tons 14 cwt., and the base-plates for the large bed-plates will be 22 ft. by 5 ft by 1¼ in., each weighing 2 tons 10 cwt. Each cantilever of the bridge has to be erected complete in the contractor's yard previously to being sent out to India, and in order to comply with this condition a staging or scaffold has to be provided. The great weight and size of the parts forming the bridge makes this a very formidable and difficult undertaking, the various members raking and inclining in every direction. After considerable study an arrangement has been designed which is expected to answer the purpose and conditions. It will consist of about 305 piles, 14 in. by 14 in., driven into the ground about 30 feet. These are cut off at 9 ft. above the bed-plate level, and on them are to be built, in some cases, four lengths of 40-ft. timbers one above the other, and braced together with horizontal and diagonal bracing. This staging covers a space 400 ft. long by about 120 ft. wide in some parts, and will be 180 ft. high, and will require over 2,000 loads of lumber besides several tons of bolts and nuts.—*Iron.*

THE UTILIZATION OF TIDAL POWER.—At a meeting of the Junior Engineering and Scientific Society, held September 10, a paper was read on "The Mechanical Utilization of Waterfalls and Tidal Power," by Mr. H. S. Wells, Wh. Sc. The author first dealt with waterfalls, and showed how the power of the fall could be ascertained by finding velocity of steam, area of flow in square feet, and height of fall in feet. He then described Allen's balanced float motor, for which, he said, an efficiency of 90 per cent. was claimed, but he would only assume efficiencies of 55, 75, and 85 per cent,, for waterwheels, turbines, and float motors respectively, when driven by waterfalls. As to the position of the motor, much depended upon local circumstances, such as formation of banks, but fixing on shore was the best if allowable. Modern turbines could be used for falls varying from 3 ft. to 247 ft., while some were giving good results in the Black Forest with only 9 in. fall. He then dealt with the utilization of Niagara Falls, and said it was calculated by considering the catchment area and rainfall that not less than 18,000,000 cubic ft. of water passed over the falls per minute; the mean fall was 276 ft., and that gave a theoretical power of over 9,409,000 horses, of which it might well be assumed over 7,000,000 horse-power might be utilized, that being equal to power obtainable from the best class of engines from the consumption of 50,000,000 tons of coal. The author described some mills built near and worked by the falls which utilized about 1,000 horse-power, and he spoke of various propositions that had been made to utilize the falls for generating electricity for lighting purposes. He believed that was quite within the power of the present mechanical world to perform, although the difficulty and outlay would be enormous Calculating from Sir William Thomson's estimate of £10 per horse-power per annum for twelve hours per day, the value of the power of the falls would be over £140,000,000 per annum. He then gave examples of other waterfalls utilized in England, Scotland, Ireland, America, Russia, and Belgium, and proceeded to consider tidal power. He briefly described the theory of tides and the various circumstances which affected them, and then dealt with the two movements of the waters, the horizontal and vertical, the mean velocities of the tides from recent experiments being Thames, 208 ft. per minute; Avon, 170 ft.; Rhine, 275 ft.; Connecticut, 180 ft. per minute; these being the velocities at mid-stream and at or about the surface. The velocity of the sea was about 176 ft. per minute, except around rocks and some parts of the coast, where it was as high as 506 to 616 ft. per minute. The Thames had 23,545,000 tons; Portsmouth harbor, 13,500,000 tons; Harwich, 60,000,000 tons; Yarmouth, 6,000,000 tons; and Montrose, 45,000,000 tons of tidal water per day. Tables were appended to the paper, giving the volume of the flow and ebb in the Thames, the range of spring and neap tides around the coast and in the tidal rivers; the maximum range being at Bristol, 40 ft. spring and 31 ft. neap, the minimum range at Wexford, 5 ft. spring and 3½ ft. neap, the maximum difference between spring and neap being 9¾ ft. at Granville, and the minimum 1½ ft. at Wexford.

The two most important conditions of works to utilize this power were stated by the author to be, first, that they should not interfere with navigation, and second, that they should not alter the character of the tidal flow, except to improve it. A brief description was given of various patents, by means of which the horizontal movement of the tides could be utilized; of mills once used, rising and falling with tide, and driven by wheels, turned by flow and ebb, and of mills driven by water stored in reservoirs on shore at high tide and run out over wheels at low tide. The author suggested wheels fixed under sea-coast piers, driven by the tide and rising and falling in a groove, forming an arc, struck from the driven shaft as a center, increased speed being obtained which might be used to drive tramcars upon the pier by rope gearing. Some time was devoted to the consideration of motors fitted between walls built across the mouths of creeks and non-navigable rivers, the motors to rise and fall with the tide, and the water to flow through the motors into the creeks and back again when the tide ebbed, in the same way. Continuous power could not thus be obtained, but about six hours' work in every twelve hours in two portions could be done, or even more with a great range, by using about five-sixths of the total range. Reservoirs on shore for the waste water, the author observed, were objectionable (although they allowed of continuous power) on account of the area required, and impracticable unless the ground was unfit for other more useful purposes. For 50 horse-power, for six hours, the reservoir would cover about 22 acres, if 10 ft. deep, but the author suggested that buildings might be erected above these reservoirs, and the space thus utilized. A detailed description was given of such an arrangement, and the author concluded by stating his belief that great use could be made of this vast power, in such a manner as to soon repay any first outlay, and to be of great advantage for many purposes, especially for generating electricity. He believed the subject deserved more consideration than it had at present received, as being not unlikely to become one not only of personal, but of national importance. The paper was illustrated by diagrams, and a discussion followed, the author being accorded a hearty vote of thanks at the close of the meeting.—*Iron.*

ORDNANCE AND NAVAL.

Trial of a New Elswick Gun.—A trial has just been concluded at Woolwich of a new breech-loading steel-rifled Elswick gun. The weapon is a 6-inch 100-pounder gun, weighing five tons. The breech-loading mechanism is different to the Woolwich gun, having a double interrupted screw instead of a single one, the object sought being greater strength and the prevention of liability to jamming. On the gun being fired a second time, the vent piece, which carries the primer or tube and firing apparatus, blew out and was projected a distance of 200 yards. It was recovered unimpaired, and seven more rounds were fired from the gun. Such an accident, the Woolwich artillerists say, could not have occurred with the Woolwich gun. Recently, however, in firing the Woolwich gun, the vent axial became cracked or jammed. This happened when firing the 43-ton Woolwich gun with only 265 lbs. of black powder, which is stated to be much more violent in action than its ordinary charge of 295 lbs. of brown or cocoa powder. The damage to the Woolwich gun was at once repaired, and the experiments with it were resumed at the government butts. With regard to the Elswick Ordnance Company's gun, the breech mechanism is more complicated than that of the Woolwich gun, inasmuch as it has the vent piece going through the breech screw, which has to be taken out each time it is fired for the tube to be refixed. Whether the new arrangement will prove stronger than the vent axial of the Woolwich gun, further experiments will have to decide.

Gun-bursting Experiments.—Experiments took place at the proof butts, Woolwich, several months ago to elucidate the explosion of an Elswick 6-inch brecchloading gun on board the Active, at Portsmouth. So far as they were made public, the results of those experiments showed that the accident was not caused by any obstructions in the gun, various kinds of wedges, ending with a formidable cold chisel, being placed in the trial gun before firing and doing it practically no harm. It was announced that the committee would carry the investigation further before making their report, and it now appears from this report that they not only allowed the gun to fall upon a lump of steel, but dropped upon it another gun, weighing 86 cwt., from varying heights up to 8 feet, which struck a blow on its side estimated at an energy of 34 tons. A few dents to the exterior only were produced by the falls, but the heaviest of the blows slightly bulged the interior and prevented the 6-inch gauge from passing through the bore. In this state it was fired once more with the charge of 17 lbs. of powder and a common shell weighing 100 lbs.; but the only effect was a slight reduction of the bulge, and the shell left the gun uninjured. The committee having been satisfied by the evidence of persons present at the accident, that no large obstruction, such as a tompion or drill shot had been placed in the gun which exploded, and having no explanation to offer on the strength of their experiments, were led to the conclusion that there must have been some imperfection in the steel of which the gun was constructed, and had several test pieces cut out of the broken gun and examined. Finally, their report states that the explosion of the gun did not result from weakness of design, from error in loading, from wave-pressure, from projectiles being of high gauge, or setting up, or broken, or jammed, and that it was highly improbable that it was caused by any kind of obstruction or malformation of bore. They were therefore forced to the conclusion that it must have been due to imperfection of material which remained undiscovered, although the gun had passed proof. The committee, however, thought it beyond their province to inquire into the large questions involved in gun manu-

facture, which subject was already under investigation by a very influential committee. In the meantime, they saw no reason to recommend that any restriction should be placed upon the use of guns of this nature in practice; and they were unable to recommend any special precaution in the use of them, the accident being in no way due to any cause connected with the care or service of the gun. It should be stated that the charge of 17 lbs. of powder, with which the gun was fired when it burst, was only half the service charge, and that the gun had been repeatedly fired with the full charge of 34 lbs. without showing any signs of injury.

Russian Navy Estimates for 1886.—The estimates just passed by the Russian Government for 1886, show that the vigorous naval policy initiated last year, just before the Afghan conflict came on the scene, is to be continued and extended. While a few years ago half a million sterling was considered a sufficient annual grant for naval construction, and the figure sometimes fell even below this, next twelvemonth will see an expenditure of 11,831,800 roubles, or, at the current rate of exchange, nearly £1,200,000. Of this amount, £830,000 will be devoted to the building of hulls, and the remainder to engines and machinery. The report further states that of the grant made for shipbuilding £600,000 will be assigned to private establishments, and only £230,000 to Government yards. To the uninitiated this would seem to imply a more flourishing condition of things on the part of private works, and a greater amount of paternal regard for them than is really the case. On the Neva—and the whole of the grant is made for St. Petersburg—there are only two private establishments in receipt of Government orders, and for that matter only two existing at all. Of these the Franco-Russian works, late the works of Mr. George Baird, is the only one that can claim to be a private concern, the shares of the rival Baltic works on the opposite side of the river being wholly held by the Government As the latter is doing the most work, and will doubtless receive the lion's share of the grant, it is hardly accurate to place the amount assigned to it under the heading of "To private firms," particularly as it has long been treated as a semi-official appanage of the Admiralty, and is only left wholly unabsorbed by the State for reasons of private interest on the part of a few high officials.

Brazil p ossesses five ironclads, the Riachuelo, built in 1883 by Messrs. Samuda, 5,800 tons, built of steel, steel armor 10 in. on the turret and 11 in. on the side; 6,000 indicated horse-power, speed 16 knots, and she is armed with four Armstrong guns of 20 tons each, 6 of 5½ tons, and 15 Nordenfelt machine guns. The Solimoes and Javany are of 3,600 tons each, and were launched in 1876. They are of iron, and have iron armor, 13 in. on the turrets and 12 in. on the side. Their speed is 12 knots, and they are each armed with four Whitworth guns of 25 tons each, and four Nordenfelts. The remaining two ironclads are of 928 tons and 1,196 tons, respectively, and have armor of 4 in. thickness. Brazil further owns a wooden ship plated with 4-in. armor, four small monitors for river service, and seven wooden cruisers. A steel cruiser of 4,000 tons, which is to steam 15½ knots, is at present being built for the Brazilian Government in England. Brazil has also seven wooden and five iron gunboats, and also five composite gunboats in course of construction, besides eight torpedo boats.

The explosion of gunpowder is generally, either in the bore of a gun or in a close vessel, extremely rapid; but, rapid as it is, when small charges are fired in a large vessel, there is a considerable reduction of pressure from the cooling effect of the vessel, owing to the great difference of temperature between the ignited powder and the vessel. With gun-cotton this difference is much more marked, both because the weight of the explosive employed in experiments is much less, and the temperature of explosion is much higher. Between charges of a few ounces and a few pounds, for instance, of the same gravimetric density, there is a very marked difference of pressure. The actual pressure reached by the explosion of gun-cottons experimented with by Captain Noble and Sir F. Abel, assuming the gravimetric density of the charge to be unity, would be between 18,000 and 19,000 atmospheres, or, say, 120 tons on the square inch. The pressure indicated has not been reached in their experiments, both because they would have had great difficulty in making a vessel to stand such pressures, and because charges of such density would not readily be placed in the vessels. The highest pressure actually recorded with a density of 0.55 was a little over 70 tons on the square inch.

If a charge of gunpowder be placed in the chamber of a gun, the gravimetric density of the charge being unity, and if it be completely exploded before the shot be allowed to move, the state of things immediately prior to the shot being permitted to move in the powder chamber, roughly speaking, is as follows: The products of explosion are divided into two classes of substances, about two-fifths, by weight, of the powder being in the form of permanent gases, and three-fifths solid matter, the solid matter being perfectly liquid at the moment of explosion and in an extremely fine state of division. By the combustion is generated some 730 units of heat. The temperature of the explosion is about 2,200 deg. Cent., or about 4,000 deg. Fah., and the exploded powder exercises a pressure of about 6,500 atmospheres, or about 43 tons per square inch, against the walls of the chamber and against the projectile.

In his lecture to the Institution of Civil Engineers, on "Heat Action of Explosives, Captain Noble remarked: "Twenty-five years ago our most powerful piece of artillery was a 68-pounder, throwing its projectile with a velocity of 1,570 ft. per second. Now the weight of our guns is increased from 5 tons to 100, the projectile from 68 lbs. to 2,000, the velocities from 1,600 ft. to 2,000, the energies from 1,100 foot-tons to over 52,000 foot-tons." We

may remark, however, that it is only about twenty years since Mallet urged, amongst other things, the use of a mechanical means of training guns, but the wisdom of the then Ordnance Committee said it was not probable that larger guns would ever be used than were then employed, and the handspike was all that was necessary. The Woolwich authorities, however, afterwards used Mallet's invention, and paid nothing for it.

The Kunstadter Screw.—The report by the board of engineers appointed by the Secretary of the Navy of the United States on the working of this screw, the principles of which have been explained in these columns on a former occasion, contains several valuable features, the importance of which should not be lost sight of by naval men. It is pointed out that the ability of a steamer furnished with the steering and propelling screw to turn in a small circle, if opposed to vessels which take longer time to turn, will be of immense importance in the naval warfare of the future, and it would not be very surprising if this novel steering apparatus were not readily applied to their navies by all the powers having a pretension to naval preponderance. This increased movability imparted to vessels would enable a ship provided with the Kunstadter screw, if not otherwise inferior, to speedily disable a vessel provided only with the ordinary steering apparatus. The screw would permit such a vessel to ram her opponent, or to escape a thrust aimed at her, and to generally choose the most favorable positions. She would outmanœuvre the enemy. But this is not all. In line of battle, a fleet provided with Kunstadter screws, being more easily handled, could act in closer order, and thus annihilate the enemy's fleet by a concentrated fire. The report concludes with the following remarks: "We do not consider a vessel perfectly equipped for war which is only provided with the rudder if the Kunstadter steering and propelling screw could be applied. The greater safety arising from greater movability is a sufficient reason for the introduction of the apparatus in all steamers, war as well as trading. Many collisions would thereby be avoided; much time and anxiety saved in handling a vessel in a narrow channel."

The following figures concerning the Great Eastern and the Ark are of interest. Somebody is comparing the size and cost of the Great Eastern and Noah's Ark. The cost of building and launching the Great Eastern was $3,650,000, and this broke the original company. A new company was formed, which spent $600,000 in fitting and furnishing her. Then this company failed, and a new company was organized, with a capital of $500,000. At the close of 1880 this company sunk £86,715 upon the vessel, thus making her total cost $4,703,575. Nothing ever built can stand comparison with the Great Eastern, excepting Noah's Ark, and even this vessel could not match her. The length of the ark was 300 cubits, her breadth 50 cubits, and her height 30 cubits. The cubit of the Scriptures, according to Bishop Wilkins, was $21\frac{65}{100}$ in., and computed to English measurement the Ark was 547 feet long, 91 feet beam, $54\frac{7}{10}$ feet depth, and 21,762 tons. The Great Eastern is 680 feet long, 83 feet beam, 56 feet depth, and 28,093 tons measurement. So Noah's Ark is quite overshadowed by the Great Eastern.

BOOK NOTICES.

Publications Received.

Papers of the Institution of Civil Engineers:

Paper No. 2,027.—The Construction of Locomotive Engines. By William Stroudley, M. Inst. C. E.

No. 2,086.—Machinery for the Manufacture of Nitrate of Soda. By Robert Harvey, Assoc M. I. C. E.

No. 2,092 —Erection of a Howe Truss Bridge on the Canadian Pacific Railway. By Charles Anthony Stocos, Assoc. M. I. C. E.

Annual Report of the Chief of Engineers of the United States Army. 1885.

Student's Paper No. 186.—Iron Bridges. By Frederick Wilfred Stokes.

——— Forms of Ships. By Sir Edward J. Reed, K. C. B.

——— Tides and Coast Works. By Thos. Stevenson, M. Inst. C. E.

——— Abstracts of Papers in Foreign Transactions and Periodicals.

Monographs of the United States Geological Survey:

Vol. VI.—Contributions to the Knowledge of the Older Mesozoic Flora of Virginia. By William Morris Fontaine.

Vol. VIII.—Paleontology of the Eureka District. By Charles Doolittle Walcott. Washington: Government Printing Office.

These volumes are invaluable to the students of Geology, affording, as they do, accurate descriptions of characteristic fossils. The typography and plates of these books may be regarded with pride by Americans.

Vol. VI. contains fifty large plates of fossil plants, and Vol. VIII. is embellished with twenty-four plates of paleozoic fauna, mostly mollusca.

A Guide to Sanitary House-Inspection; or Hints and Helps Regarding the Choice of a Healthful Home in City or Country. By William Paul Gerhard, C. E. New York: John Wiley & Sons.

The principal aim of this book, as the author states, is to instruct the householder—to open his eyes to the dangers which threaten him if he allows his family to occupy a dwelling without having first thoroughly examined its sanitary condition, its stability and sound construction, its safety from fire, and its means of protection against the elements. The book is a guide in the search for defects within, as well as around and about the house, and, as the chief points to be looked after in such an inspection, the author discusses the surroundings of the house, its site, location, aspect, and the character of the soil; the house foundations, walls, cellar, and yard; structural details—such as walls, roofs, windows, floors, stairs,

etc.; the heating apparatus, the water service, the sewerage and plumbing, the gas-lighting, ventilation, garbage disposal, and disposal of household wastes.

The hints given refer not only to city houses, but also to apartment houses, tenements, suburban and country houses, summer hotels and boarding houses. The evils due to skin building are clearly and vividly pointed out, and, in contrast to such health-endangering structures, the essentials of a healthful house in city or country are briefly stated.

This short enumeration of the contents will be sufficient to show that although the book is written primarily for laymen, in a language free from technical terms, it will also be useful and interesting to architects, civil engineers, builders, health officers, sanitary inspectors, and family physicians.

FOURTH ANNUAL REPORT OF THE UNITED STATES GEOLOGICAL SURVEY, 1882–83. By J. W. POWELL, Director. Washington: Government Printing Office.

The accompanying papers of this report are:

Hawaiian Volcanoes. By Capt. C. E. Dutton.

The Mining Geology of the Eureka District. By J. S. Curtis.

Popular Fallacies Regarding the Precious-Metal Ore Deposits. By Albert Williams, Jr.

A Review of the Fossil Ostradæ of North America. By Dr. Charles A. White.

A Geological Reconnoissance in Southern Oregon. By Israel C. Russell.

MANUAL OF GEOLOGY. By JOHN PHILLIPS, LL. D., F. R. S.

Part II.—Statigraphical Geology and Paleontology. By ROBERT ETHERIDGE, F. R. S. London: Charles Griffen & Co.

Only the *plan* of the original Phillips Manual of 1855 is retained in this new edition; the text has been almost entirely rewritten.

It is now a complete and late treatise on Geology.

The plates are upon tinted paper, and distributed throughout the book.

Of course, the work is of more interest to European than to American students, but scientific libraries are incomplete in the department of geology without Phillips' Manual.

A TEXT-BOOK OF THE MATERIALS OF CONSTRUCTION. By ROBERT H. THURSTON., M. A. New York: John Wiley & Sons.

This is an abridgment of the author's three-volume treatise on "Materials of Engineering." The present volume retains all that relates to the origin, nature, method of preparation, and common physical properties of the so-called useful metals, and deals especially with those qualities which the engineer deems essential.

The chapters on the reduction of the ores of the common metals are largely retained, as are, also, the portions relating to alloys.

In short, a serviceable text-book has been constructed from the valuable collection of data in the larger treatise, without impairing its value for class-room uses.

SELECT PLANS OF ENGINEERING STRUCTURES FOR RAILROADS AND HIGHWAYS. By RICHARD B. OSBORNE, C. E. Philadelphia: Richard Osborne & Son.

This collection of plans will be found of great convenience to engineers in charge of highway or railway construction. The details are given with such a degree of minuteness as practically to afford working plans and estimates.

The first volume—the only one as yet published—is devoted mostly to masonry structures. It is proposed to publish a second volume containing similar plans, and, subsequently, two other volumes, exhibiting plans and details of framed bridges and roofs.

Such works are important aids to the younger members of the engineering profession, as they afford an opportunity to compare detail plans with the actual finished structure.

MISCELLANEOUS.

IN a lecture to the Institution of Civil Engineers, on "Heat Action of Explosives," Captain Noble remarked:—"Helmholz has given an estimate somewhere of the heat that would be developed if our earth were suddenly brought to rest, but if, looking at our earth in an artillery point of view, we considered our earth as an enormous projectile, and if we supposed, further, that we could utilize the whole energy stored up in gunpowder, we should yet require a charge 150 times greater than its own weight, or 900 times greater than its volume, to communicate to the earth her motion in her orbit."

IT is stated of the new metal gallium that, with the exception of mercury, which only becomes solid at 37.9 deg. Fahr., there is no other element which liquefies at so low a temperature. It melts at 81.1 deg. Fahr., so that it liquefies when held in the hand. The metal is hard and resistant, even to a few degrees below the melting point. It can be cut, and possesses a slight malleability. When fused, it adheres easily to glass on which it forms a beautiful mirror, whiter than that produced by mercury. It oxidizes but very superficially when heated to redness in the air, and does not become volatile. Unlike lead, it acquires only a very slight tarnish on exposure to moist air. Its specific gravity is a little under 6, that of aluminum being 2.6, that of zinc, 7.1, and that of lead, 11.4. Unlike lead, again, gallium is a highly crystalline metal, its form being that of a square octahedron. In its chemical characteristics, the rare element gallium shows the greatest analogy to the abundant element aluminum.

THE *United Service Gazette* says:—Colonel Thackeray, the commandant of the Bengal Sappers and Miners, is taking a step which will result in the formation within the corps of small bodies of men, each thoroughly qualified to perform well one or the other of the operations in engineering which the corps may be called upon to accomplish, and of which at present the men can have but a vague and imperfect idea. One of the companies of the corps is to be set apart solely for providing trained

men to supply technical work in the field. The company will be formed of picked men from the entire corps, the most efficient men in each branch of sapper work being drafted into it. It will be subdivided into seven squads, each with a complement of European non-commissioned officers and native commissioned officers. The squads will be for torpedo, telegraph, printing, lithography, photography, pontooning, and company duty respectively. Each unit of the company will thus be complete in itself.

Mr. C. C. Hine, editor of the *Monitor*, relates the following:—"The Institute of Technology, at Boston, long ago decided upon the danger of steam pipes passing through and in contact with wood. It was shown that the wood, by being constantly heated, assumes the condition, to a greater or less degree, of fine charcoal, a condition highly favorable to spontaneous combustion. Steam was generated in an ordinary boiler, and was conveyed therefrom in pipes which passed through a furnace, and thence into retorts for the purpose of distilling petroleum. Here the pipes formed extensive coils, and then passed out, terminating at a valve outside the building. To prevent the steam when blown off from disintegrating the mortar in an opposite wall, some boards were set up to receive the force of the discharge, and as often as the superheated steam was blown the boards were set on fire."

The deepest boring yet made is at the village of Schladebach, near the line between Leipzig and Corbetha. It has been made by the Prussian Government to test for the presence of coal, and was bored with diamond drills. Its depth is 1390 meters—4560 ft.—its breadth at the bottom 2 in., and at the top 11 in. It has occupied 3¼ years to bore, and cost a little over £5,000. The temperature at the bottom is 118 deg. Fahr.

In some tests made with small squares of various woods buried 1 in. in the ground, the following results, says the *Garden*, were noted:—Birch and aspen decayed in three years; willow and horse-chestnut in four years; maple and red beech in five years; elm, ash, hornbeam, and Lombardy poplar, in seven years; oak, Scotch fir, Weymouth pine, and silver fir decayed to a depth of ½-in. in seven years; larch, Juniper, and arbor-vitæ were uninjured at the expiration of the seven years.

Telpherage.—The first line for the conveyance of goods by the electrical system invented by the late Professor Fleeming Jenkin, and entitled "Telpherage," was formally opened at Glynde, near Lewes, in October. The system was fully described in a paper read before the Society by the inventor on May 14th, 1884, but the details have been somewhat modified since that date. The line is a double one, nearly a mile in length, and is composed of two sets of steel rods, three-quarters of an inch in diameter, supported on wooden posts of T-shape, and about 18 ft. high. The wires are supported one on either end of the cross-piece of the T, which is 8 ft. long. The carriers, or skips, as they are technically termed, are iron, trough-shaped buckets, each holding about 2 cwt., and suspended from the line by a light iron frame, at the upper end of which is a pair of grooved wheels running on the line of rods. A train is made up of ten of these skips, which are in electrical connection with each other, and with an electrical motor which is placed in the middle of the train, having five skips in front of and five behind it. At a point about midway of the length of the line is the engine-house, in which is a steam engine which drives the dynamos. From these latter the current is led to the line, and thus to the electrical motor which moves the train. The use to which the line is put, is to carry clay from a pit to the Glynde railway siding, whence it is delivered into trucks, and transported by rail to the works of the Newhaven Cement Company. At the charging end of the telpher line the skips are loaded each with about 2 cwt. of clay, the train thus carrying one ton. A laborer, by touching a key, starts the train, which travels at a speed of from four to five miles an hour along the overhead line to the Glynde Station. Arrived there, another laborer upsets each skip as it passes over a railway truck, into which the clay is thus loaded. This upsetting, however, is eventually to be performed automatically by means of a lever on each skip, which will come in contact with a projecting arm as it passes over the truck.—*Journal of the Society of Arts.*

For testing the quality of the leather used for belting, M. Eitner proposes, in the *Revue Industrielle*, the following simple method: A small piece is cut out of the belt and placed in vinegar. If the leather has been perfectly tanned, and is therefore of good quality, it will remain immersed in the vinegar, even for several months, without any other change than becoming of a little darker color. If, on the contrary, it is not well impregnated with tannin, the fibers will promptly swell, and, after a short time, become converted into a gelatinous mass.

The deep boring being sunk by the German Government near Schladebach, with the object, especially, of obtaining trustworthy data concerning the rate of increase of the earth's temperature toward the interior, has at present given information corroborative of what has been obtained elsewhere. At the beginning of this year the bore had reached the depth of 1,392 meters, which is believed to be the lowest yet reached. The temperature at successive stages is ascertained by a special thermometer, the principle of construction being that, as the heat increases the mercury will expand so as to flow over the lip of an open tube. The difference of the overflows will give the rate of increase of the temperature. It has been ascertained that the temperature at the depth of 1,392 meters was 49 deg. Cent., or 120 deg. Fah. If the temperature increases regularly at this rate, the boiling point of water ought to be reached at a depth of 3,000 meters, or nearly two miles, and at 45 miles we should find the heat at which platinum melts. This would go to show that the rigid earth's crust cannot be more than about one-ninetieth of its radius; but the rate of increase is very different in different districts.